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A

JOURNAL

OF

NATURAL PHILOSOPHY,

CHEMISTRY,

AND

THE ARTS.

N. S.

VOL. XI.

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Illustrated with Engravings.

BY WILLIAM NICHOLSON.

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PREFACE.

THE Authors of Original Papers in the present Volume, are Mr. Thomas Reid; Mr. James Scott; Mr. Boswell; Mr. John Gough; Mr. Irvine; Mr. Cuthbertson; Mr. William Henry; John Bostock, M. D.; W. Brande, Esq.; Mr. Matthew Murray; Mr. J. C. Hornblower; Mr. A. F. Thoelden; Mr. John Clennell; J. P.; W. N.; W. F. S.; Mr. William Wilson; A. Thomson, Esq.; Amicus; Mr. O. Gregory; Mr. Ezekiel Walker; Mr. James Stodart; Count Rumford, V. P R. S.; Mr. Thomas Harrison. A Constant Reader.

Of Foreign Works, Mr. Erman; M. Bonnard; Messrs. Robertson and Sacharoff; Lalande; A. M.; Conſtat Duméril; C. L.; A. B. Berthollet; J. C. Delametherie; M. Bralle; Profeſſor Pini; Mr. Deyeux; M. Hassenfratz; Mr. Goettling; Mr. Steinacher; Mr. Marechaux; Mr. Schnaubert; Mr. J. G. Schmidt; Brugnatelli; Citizen Duhamel; C. L. Cadet; J. Machlachlan; Humbold and Bonpland.

And of English Memoirs abridged or extracted; Charles Hatchett, Esq. F. R. S.; Mr. C. Waistell; Mr. George Dodd; Dr. William Roxburgh; Captain Joseph Brodie; Mr. William Hardy; Mr. James Rawlinson; Humphry Davy, Esq. F. R. S.; Richard Chenevix, Esq. F. R. S. M. R. I. A.; Mr. Robert Seppings; A. Carlisle, Esq. F. R. S.; John Churchman, Esq. Imp. Acad. of Sciences, Petersburgh; Mr. D. Mushett, Sir A. N. Edelcrantz; Edward Bigott, Esq.

Of

PREFACE.

Of the Engravings, the Subjects are, 1. A very ſimple and perfectly safe Gun Lock, by Mr. Dodd. 2. The framing of a Field Gate with a considerable Accession of Strength, by Mr. Waistell. 3. A Compensation Curb for a Time Piece, by Mr. Scott. 5, 6. Plans and Sections of the Bavarian Salt Works. 7. Captain Brodie's Method of connecting Iron Bars, and coating them with Lead. 8. Mr. Rawlinson's Mill for grinding Colours. 9. Apparatus for grinding Indigo and other dry Materials. 10. Method of banking the Balance of a Time Piece, by Mr. Hardy. 11. A Pyrometer for meaſuring the Heat of a Furnace, by Mr. J. G. F. Schmidt. 12. A Portable Steam Engine, by Mr. Murray. 13. Apparatus for making the Gaseous Oxide of Carbon, by Mr. Baruel. 14. A Topographical Plan by Mr. Churchman, in which the Elevations and Depressions of the Surface of the Earth, are by a new Method correctly delienated. 15. Mr. Gregory's Apparatus for measuring the Power of Horſes. 16. The American Borer. 17. Diagram illustrative of Optics, by Mr. Ezekiel Walker. 18. Mr. Sepping's Blocks for supporting and suspending Ships. 19. Count Rumford's Apparatus for illustrating his Experiments in Proof, that the greatest Density of Water is higher than the freezing Point. 20. A Valve for Steam Engines, by the Chevalier Edelcrantz. 21. Figures and Diagrams, by Edward Pigott, Eſq. to illustrate the Causes of Periodical Decrease in Diminution of Light, in a Star in Sobieſki's Shield. 22. A Press for Botanical Specimens, by Mr. Harrison.

Soho Square, London, September 1, 1805.

TABLE OF CONTENTS

TO THIS ELEVENTH VOLUME.

MAY, 1805.

ENGRAVINGS of the following Objects: 1. A very ſimple and perfectly ſafe Gun-Lock, by Mr. Dodd; 2. The framing of a Field-Gate with conſiderable Acceſſion of Strength and great Saving of Material, by Mr. Waiſtell; 3. A Compenſation-Curb for a Time-Piece, by Mr. Scott; 4, 5. Plans and Sections of the Bavarian Salt Works.

I. Letter from Mr. Thomas Reid, on the Conſtruction of Time-keeping Machines. - - - - - Page 1

II. An Analyſis of the Magnetical Pyrites; with Remarks on ſome of the other Sulphurets of Iron. By Charles Hatchett, Eſq. F. R. S. From the Phil. Tranſ. for 1804. - - - - - 3

III. Extract of a Memoir of Mr. Erman, entitled Obſervations and Doubts concerning Atmoſpheric Electricity. - - - 17

IV. Deſcription of a Compenſation Curb. By Mr. James Scott. - 19

V. Letter from Mr. Boſwell, in Anſwer to an Old Correſpondent. 21

VI. Deſcription of an improved Gate for Fields. By Mr. C. Waiſtell. 23

VII. Deſcription of an improved Gun-Lock, by Mr. George Dodd. 28

VIII. Inveſtigation of the Properties of the Lines drawn in a Circle by Mr. Boſwell, in the Tenth Volume of this Journal. By Mr. John Gough. 29

IX. On the Culture, Properties, and comparative Strength of Hemp, and other Vegetable Fibres, the Growth of the Eaſt Indies. By Dr. William Roxburgh, of Calcutta. - - - - - 32

X. Deſcription of the Bavarian Method of evaporating Saline Waters. By M. Bonnard. - - - - - - 47

XI. Letter of Inquiry reſpecting the late Dr. Irvine's fundamental Experiment on the relative Capacities for Heat of Ice and Water. With an Anſwer by Mr. Irvine. - - - - 50

XII. Short Account of ſome of the moſt remarkable Facts and Obſervations in an Aeroſtatic Voyage, made from Peterſburgh, by Meſſrs. Robertſon and Sacharoff, under the Sanction of the Imperial Academy. - - 52

XIII. Letter from Mr. Cuthbertſon, containing Remarks on Mr. Wm. Wilſon's and Mr. Hauy's Experiments on the Electricity of Metals. - 56

Scientific News, Account of Books, &c. 57.—Note on the new Planet Juno, ib.—Diſcovery of Fluoric Acid in the Topaz, 58.—Examination of Crude Platina. By Meſſrs. Tennant and Wollaſton, 59.—Prize Queſtions in France, ib.—Ruſſian Circumnavigation, 60.—Geological Journey from the Academy at Warſaw, ib.—Lalande's Propoſal of a new Scale of the Thermometer, 61.—Two Kinds of Honey, ib.—Palladium, ib.—Traité élementaire d'Hiſtoire Naturelle, &c. An elementary Treatiſe on Natural Hiſtory, by A. M. Conſtant Duméril: compoſed by Order of Government for the Uſe of the National Lyceums. 1 Vol. 8vo, Paris. 62.—Elemens de l'Art de la Teinture, &c. or Elements of the Art of Dying; with a Deſcription of the Proceſs of Bleaching by the oxigenated Muriatic Acid. Second Edition. By C. L. and A. B. Berthollet. Paris. 63.—Conſiderations on organized Bodies. By J. C. Delametherie, ib.—*To Correſpondents*, 64.

JUNE 1805.

Engravings of the following Objects: 1. Captain Brodie's Method of connecting Iron Bars, and coating them with Lead; 2. A Mill by Mr. Rawlinſon, for grinding Colours more finely and expeditiouſly than is done by the Stone and Muller; 3. Apparatus for grinding Indigo, and other dry Materials; 4. Method of banking the Balance of a Time-Piece, by Mr. Hardy; 5. A Pyrometer for meaſuring the Heat of a Furnace, by Mr. I. G. F. Schmidt, Inſtitutor to the Sons of the reigning Prince of Moldavia; 6. A Portable Steam Engine, by Mr. Murray; 7. Apparatus for making the Gaſeous Oxide of Carbon, by Mr. Baruel.

I. Experiments on the Gaſes obtained by the deſtructive Diſtillation of Wood, Peat, Pit-Coal, Oil, Wax, &c. with a View to the Theory of their Combuſtion, when employed as Sources of artificial Light; and including Obſervations on Hydro-Carburets in general, and the Carbonic Oxide: By Mr. William Henry. (Communicated by the Author.) - Page 65
II. Experiments on the Analyſis of Goulard's Extract, or the Aqua Lithargyri Acetati. By John Boſtock, M.D. Communicated by the Author. 75
III. A conciſe View of the Theory of Reſpiration. By William Brandé, Eſq. From the Author. - - - - 79
IV. Inſtruction on the Proceſſes diſcovered by M. Bralle, of Amiens, for watering Hemp in Two Hours Time, in all Seaſons, without injuring its Quality. Publiſhed by Order of the Miniſter of the Interior of France. 86
V. Deſcription of a Portable Steam Engine. By Mr. Matthew Murray. 93
VI. Letter from M. J. C. Hornblower, Engineer, on the Meaſure of Force by Horſe Powers. - - - - - - 95
VII. Letter from Mr. A. F. Thoelden, communicating three manuſcript Tables from Mr. Bode, of Berlin, of the geocentric Places of the new Planets Ceres, Pallas, and Juno, for twelve Months to come. - - 99
VIII. Letter from Profeſſor Pini, Inſpector of Mines to the Italian Republic, to J. C. Delametherie, on Corindon found in Italy. - - 103
IX. On diſcloſing the Proceſs of Manufactories. In a Letter from Mr. John Clennell. - - - - - - - - 105
X. Queſtion reſpecting the Purification of Copper. By J. P. With a conciſe Reply. W. N. - - - - - - 107
XI. Deſcription of a Method of connecting Iron Bars, and coating them with Lead, ſo as to form ſolid Pillars for Light-houſes on Rocks covered at High-water, and to defend them from Corroſion. By Capt. Joſeph Brodie, of the Royal Navy. - - - - - - - 108
XII. Reply to Mr. Accum's laſt Letter on the Production of Nitrous Acid. By W. F. S. - - - - - - 109
XIII. Experiments on the Electricity of Metallic Filings ſifted through Metal; with Remarks in Anſwer to a Letter of Mr. Cuthbertſon. By Mr. Wm. Wilſon. (From the Author.) - - - - 110
XIV. Reply to Mr. Boſwell. By An Old Correſpondent. - 112
XV. Deſcription of an accurate Method of banking the Balance of a Time-keeper. By Mr. William Hardy. Extracted from his Letter to Charles Taylor, Eſq. Sec. to the Society of Arts. - - 114
XVI. Deſcription of a new Apparatus for making the gaſiform Oxide of Carbon. Communicated by Mr. Deyeux. - - - - - 116
XVII. Deſcription of an improved Mill for levigating Painters Colours. By Mr. James Rawlinſon, of Derby. - - - - 119
XVIII. A new and cheap Method of purifying Gold and Silver. By A. Thomſon, Eſq. of Banchory, near Aberdeen. In a Letter from the Diſcoverer. 125
XIX. Memoir on the Propagation of Sound. By M. Haſſenfratz. 127
XX. A very advantageous Mode of preparing Muriate of Barytes by the mutual Decompoſition of ſulphurated Barytes and Muriate of Lime. By Mr. Goettling. - - - - - - - 133

XXI. Obſervations on the Rectification of Nitric Acid, by Mr. Steinacher, Druggiſt at Paris. - - - - - 134
XXII. Eaſy Method of making the very combuſtible Oxide of Phoſphorus. By Amicus. - - - - - - 136
XXIII. Deſcription of an extremely ſenſible Micro-electrometer. By Mr. Marechaux. - - - - - ib.
XXIV. Action of Phoſphorus on the Solutions of Metals. By Mr. Schmaubert. 138
XXV. Account of a new Pyrometer, which is capable of indicating degrees of Heat of a Furnace. By Mr. J. G. Schmidt, of Yaſſa, in Moldavia. From the Author. - - - - - 141
XXVI. New, eaſy, and economical Method of ſeparating Copper from Silver. By Mr. Goettling. - - - - - 142
Scientific News, 144.—Extract of a Letter from Brugnatelli, concerning the Non-exiſtence of the charged Pile, ib.

JULY, 1805.

Engravings of the following Objects: 1. A Topographical Plan by Mr. Churchman, in which the Elevations and Depreſſions of the Surface of the Earth are by a new Method correctly delineated; 2. Apparatus for meaſuring the Power of Horſes, by Mr. Gregory; 3. The American Borer, or Inſtrument for boring Holes in Wood, and clearing out the Cuttings during the Time of Work; 4. Diagram illuſtrative of Optics, by Mr. Ezekiel Walker; 5. Mr. Seppings's Blocks for ſupporting and ſuſpending Ships in ſuch a Manner as to render it unneceſſary to lift them for the Purpoſe of Repair.

I. Remarks on the Eſtimation of the Strength of Horſes. In a Letter from Mr. O. Gregory, of the Royal Military Academy, Woolwich. Page 145
II. An Account of ſome Analytical Experiments on a Mineral Production from Devonſhire conſiſting principally of Alumine and Water. By Humphry Davy, Eſq. F. R. S. Profeſſor of Chemiſtry in the Royal Inſtitution. From the Philoſophical Tranſactions in 1805. - - - 153
III. On the Aberrations of Light paſſing through Lenſes. By Mr. Ezekiel Walker. - - - - - - 159
IV. New Method of decompoſing the Sulphate of Barytes for preparing the Muriate of that Earth, and Preparation of the Muriate. By Mr. Goettling. 161
V. On the Action of Platina and Mercury upon each other. By Richard Chenevix, Eſq. F. R. S. M. R. I. A. &c. From the Philoſophical Tranſactions for 1805, p. 104. - - - - 162
VI. Method of obviating the Neceſſity of Lifting Ships. By Mr. Robert Seppings, of Chatham Yard. - - - - 182
VII. On muſcular Motion. By Anthony Carliſle, Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, November 8, 1804. 189
VIII. Deſcription of a Boring Tube, in general Uſe in America; but leſs known in this Country. - - - - 203
IX. Geographical and Topographical Improvements. By John Churchman, Eſq. M. Imp. Acad. of Sciences, Peterſburgh. - - ib.
X. A Memoir on the refining of Lead in the large Way. Containing ſome Reflections on the Inconveniencies reſulting from Cupels made of Aſhes; with a new and œconomical Method of conſtructing theſe Cupels. By Cit. Duhamel. - - - - - - 206
XI. Letter from Mr. James Stodart, explaining the Method of gilding upon Steel by Immerſion in a Liquid, which has lately engaged the public Attention in various Articles of Manufacture. - - - 215
XII. On the peculiar Noiſe emitted by Water before it acquires the Temperature of boiling; which is commonly denoted by the Word Simmering. W. N. - - - - - - 216
XIII. *Experiments on Wootz. By Mr. David* Muſhet. From the Philoſophical Tranſactions, *1805.* - - - - 221

AUGUST, 1802.

Engravings of the following Objects: 1. An Apparatus communicated by Count Rumford, for illuſtrating his Experiments in Proof that the greateſt Denſity of Water is higher than the freezing Point; 2 A Valve for Steam Engines, which opens either inwards or outwards by Preſſures ſeparately adjuſtible, by the Chevalier Edelcrantz; 3. Figures and Diagrams by Edward Pigott, Eſq. to illuſtrate the Cauſes of periodical Increaſe and Diminution of Light in a Star in Sobieſki's Shield; 4. A Preſs for Botanical Specimens, by Mr. Harriſon.

I. An Account of ſome new Experiments, which prove that the Temperature at which the Denſity of Water is a Maximum, is ſeveral Degrees of the Thermometer above the freezing Point. By Benjamin Count of Rumford, V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c. Received July 16, 1805, from the Author; with a Letter dated Munich, 25th June, 1805. - - - - Page 225

II. Deſcription of a Preſs for preſerving botanical Subjects; with an Account of the Succeſs of the Improvement in the Art of Blaſting pointed out by Mr. Jeſſop in this Journal. In a Letter from Mr. Tho. Harriſon. 236

III. Deſcription of a Safety Valve, containing a Vacuum Valve in the ſame Hole of the Boiler. By Sir A. N. Edelcrantz. - - 243

IV. Obſervations and Experiments for the Purpoſe of aſcertaining the definite Characters of the primary Animal Fluids, and to indicate their Preſence by accurate chemical Teſts. By John Boſtock, M. D. Communicated by the Author. - - - - 244

V. On muſcular Motion. By Anthony Carliſle, Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, Nov. 8, 1804. 255

VI. On the Meaſure of mechanic Power. In a Letter from Mr. J. C. Hornblower. - - - - - - 264

VII. An Inveſtigation of all the Changes of the variable Star in Sobieſki's Shield, from five Years Obſervations; exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures reſpecting unenlightened heavenly Bodies. By Edward Pigott, Eſq. Abridged from the Phil. Tranſ. for 1805. - - - - - 271

VIII. Account of a luminous Meteor. By a Conſtant Reader. 281

IX. Precipitation of Platina as a Covering or Defence to poliſhed Steel, and alſo to Braſs. In a Letter from Mr. J. Stodart. - - 282

X. Experiments on Wootz. By Mr. David Muſhett. From the Philoſophical Tranſactions, 1805. - - - - 284

XI. A Memoir on the Webs of Spiders. By C. L. Cadet. - - 290

XII. Information on the Mines and Manufactures of the Eaſt Indies, and other Subjects. By J. Machlachlan, Eſq. of Calcutta. - 294

Scientific News, 299.—Imperial Academy of Sciences at Peterſburgh, ib.—Prize concerning Light, propoſed by the Imperial Academy of Sciences for the Year 1806, 300.—Voyages of Meſſrs. Humboldt and Bonpland, 301.—Fiſh ejected from Volcanoes, 303.—Water formed by mechanical Preſſure, ib. Malleability of Zinc, 304.—Palladium, ib.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

MAY, 1805.

ARTICLE I.

Letter from Mr. Thomas Reid, *on the Conſtruction of Time-keeping Machines.*

To Mr. NICHOLSON.

SIR,

Compenſation pendulum of Mr. Edward Troughton.

IN your intereſting and uſeful Journal of December laſt, I was glad to ſee the improvement of compenſation pendulums for aſtronomical clocks, ſo zealouſly taken up by ſuch an able hand, as that of Mr. Edward Troughton's.

Probability from Berthoud's experiments that the ſteel wires may be too ſlight.

But an excellent clock of this ſort becomes ſo very valuable and neceſſary an appendage in an obſervatory, to thoſe aſtronomical inſtruments with which he is ſo happily engaged in daily conſtructing and improving; that he muſt more readily ſee their advantage than even thoſe whoſe buſineſs it is to make ſuch clocks. If Mr. Berthoud, a celebrated author on every part that regards the improvement of time-keeping machines, is correct, it would appear, that the ſteel wires of Mr. Troughton's pendulum muſt be too ſlender. Mr. Berthoud, by his experiments, ſaw, that there was a certain ſtrength of materials neceſſary, in order to render the compenſation complete having found, that on the pendulum rods (if too ſmall) being lengthened by heat, the contraction by cold would not bring the ball again quite up to the place where it ſet out from when the heat was firſt applied, and this with a ball of a moderate weight; no doubt the weight of the ball may be made ſubſervient to any ſize of wires.

Mr. Troughton ſeems to have attended to this.

Whether Mr. Troughton has attended to theſe circumſtances, I know not, but ſuſpect from his propoſing, yet, to make ſome ſort of pyrometrical apparatus for the further proving of his pendulums, that the complete compenſation has not been fully aſcertained, only in ſo far as regards the calculation of the relative expanſions of braſs and ſteel.

The improvement of clocks, and time-keeping machines of every deſcription, more particularly thoſe deſtined for aſtronomical purpoſes, is a ſubject that has not a little engaged my time and attention.

The pendulum of Ludlam with a wooden rod examined.

There is a pendulum, having a wooden rod, the conſtruction of which is deſcribed with great neatneſs, perſpicuity, and mechanical knowledge, by its author, (Mr. Ludlam, late an eminent profeſſor at Cambridge) in his eſſays, and recommended by him, *who was no mean judge*, as one of the beſt in almoſt every reſpect, particularly in ſo far as regards the impulſe from the clock taking place through the middle line or centre of the rod, to be thence communicated in the ſame line to that of the ball, that hence no circumgyratory motion ſhould take place. Now, although the principle ſet out with *here*, ſeems completely adapted to prevent this ſort of motion, it will be found on trial, as I did, that of all pendulums yet made, it is the moſt liable to generate this very ſort of motion.

It is liable to a ſide oſcillation of the ball round the rod as an axis.

The ball being the middle fruſtum of a globe, a form whoſe matter is much ſpread out from the centre to the edge, and having a large hole, for the rod to paſs through; *this* taking away much of the matter from the centre, tends much more than the lenticular form, to produce the motion Mr. Ludlam wiſhed to avoid.

The ſcrews which are acted on by the crutch do not preſerve a conſtant diſtance,

Another great fault of this pendulum, is, that of putting in ſcrews through the wooden rod, to clip the flat part from the crutch; now in changes of weather from moiſt to dry, or by heat and cold, theſe ſcrews will accordingly be found, ſometimes to pinch the flat part of the crutch, and at other times, to leave it at liberty, or even to allow it to have a conſiderable degree of ſhake between the ſcrew points: hence will ariſe very different degrees of impulſe communicated to the pendulum ball. Wood has a very ſenſible latitudinary alteration, by the effects of heat and cold, or by dry and moiſt, yet theſe effects on its length are, rather but very imperceptible, or at leaſt, are, in ſo ſmall a degree, as have not been well aſcertained, to what extent they are; even by thoſe who have made experiments with it on the pyrometer.

becauſe wood varies acroſs the grain by temperature and moiſture, &c.

Mr. Berthoud condemns wood as being unfit for pendulum rods, and although he, and others have given tables of the expanſion of various materials, yet none of them have condeſcended to ſay, what were the effects of heat and cold on wood of any ſort.

The end expanſion of wood yet unknown.

I am well convinced, that a pendulum may be ſo fitted up with a wooden rod, as to perform with ſuch a degree of correctneſs, that it would be a very difficult matter to ſay, whether it, or the beſt compenſation pendulum yet conſtructed, when both comparatively tried, was the neareſt to accurate meaſuring of time.

Wooden pendulums ſeem equal to the compound pendulums,

There has been one circumſtance attending all thoſe pendulums fitted up with a wooden rod, that their errors have been imputed to the rod, when in fact, they ought to have been imputed to that of the ball, and theſe errors have ariſen from the manner by which the ball is hung on the rod, reſting on its lower edge on the regulating nut; and lead having a conſiderable degree of expanſion, clocks having ſuch pendulums have been found, by thoſe who attended properly to their going, to have gone conſtantly faſter in ſummer than in winter. Let the ball therefore be hung by its centre on the rod, and a much greater degree of accuracy in time-keeping will be ſeen to follow.

but are often ill conſtructed.

In conſequence of my trials with Mr. Ludlam's pendulums, they were found to be extremely troubleſome to put on beat, from their ſtrong tendency to this gyratory ſort of motion, it being ſome while, before they would come to move ſteadily; I hit not only on a method of putting a clock, as it were mechanically on beat, (the common way being by the ear) but was led to think on a way of conſtructing a pendulum, in which this gyratory motion could hardly take place, even although the pendulum ſhould be but indifferently fitted up. This laſt was by following a method quite the reverſe to that of Mr. Ludlam's, in making the pendulum ball, which I made in the uſual or lenticular form, and in order that it ſhould have as much of its matter preſerved at the centre, there were two

Improved pendulum.

ſteel wires put through the ball, paſſing parallel to each other, and each put a little to one ſide of the centre, through which pendulum rods are uſually made to paſs, and when the rod is wood, it neceſſarily takes away much of the matter from the centre of the ball.

A pendulum of this ſort of mine, and which has a degree of compenſation in it, I made to a clock, which my brother got, and which he has at his houſe, No. 31, Roſamon Street, Clerkenwell.

The experiment applied to watches, &c.

I mentioned this mode of putting clocks mechanically on beat, to my ingenious friend, Mr. Pennington, who has ſince very happily applied the ſame ſucceſsfully in his practice to watches, &c.

Pendulum for regulating the ſtriking part of clocks; not new.

I ſee you have mentioned in your Journal of July laſt, the application of a pendulum to regulate the ſtriking part of clocks, from the Society for encouraging Arts, &c. having given a premium for it to Mr. Maſſy.

This is not a new thing. Mr. Berthoud mentions it as his invention, and you will ſee a drawing, and the deſcription of it, in his *Eſſai ſur l'Horlogerie*, publiſhed in 1763. Julien Le Roy, in my humble opinion, is certainly intitled to the merit of it, as it appears to me, that Berthoud has taken the idea of it from Le Roy's method of regulating the ſtriking train of his repeating watches, which he *invented*, and applied to them about the year 1754.

The crank ſcapement, not new;

There was a premium given alſo in 1799, by the honourable ſociety above mentioned, for a new 'ſcapement by Mr. Goodrich*; now this 'ſcapement was made prior to the year 1740, and invented by the Abbé Soumille; and another nearly of the ſame ſort was made at Rome before that period, as may be ſeen in the collection publiſhed by Thiout, in the year 1741. Surely nobody would think of adopting ſuch a 'ſcapement as this, whoſe principle ſeems to be that of depriving the pendulum of the moſt valuable property it poſſeſſes, viz. that of having the liberty to operate freely under the influence of gravitation. This 'ſcapement keeping the pendulum, as it were in leading ſtrings.

and bad.

I am ſurpriſed that none of the members of this honourable and uſeful ſociety, ſhould not have known, that theſe

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† See Philoſ. Journal, quarto ſeries, III. 342, 416. It is a crank.—N.

things for which premiums have been adjudged were not new, perhaps they were nevertheleſs *ſo*, to thoſe to whom the premiums were adjudged.

Among Thiout's collection may be ſeen a 'ſcapement, which he gives to the ingenious Dutertre, about the year 1724. Peter Le Roy gave into the Royal Academy of Arts and Sciences in the year 1727, a deſcription of the ſame 'ſcapement which Dutertre claimed, or pretended to ſay was his invention. The miſtake lays with Thiout, for Dutertre's 'ſcapement is an improvement of a very old one, uſed by the Germans in large clocks, perhaps long before the year 1600, though neither the author of it, nor the time *when*, can now be traced.

A 'ſcapement made long ago

However it was before Dr. Hook's time, who invented one of the ſame kind before 1658. I would not have entered ſo minutely into this diſcuſſion, but to ſhow the progreſs of the duplex, which in its preſent form, was firſt made ſo by Peter Le Roy, who afterwards gave it up for a bad one; yet it ought to be allowed that that of Dutertre's muſt have led him very eaſily to it. So much for the duplex 'ſcapement, ſo called by the workmen, and now in ſuch general uſe. A celebrated Philoſopher in the ſupplement to the Edinburgh Encyclopedia, under the article watch work, has given it a French ſurname, that of Dupleix, for what reaſon, I know not.

leading to the duplex 'ſcapement which was firſt made by Peter Le Roy.

Thiout has given alſo among the number, a 'ſcapement of his own, a ſort of detached one, and which may be conſidered as the foundation of the detached one of the preſent day, now ſo much improved, and of ſuch general uſe in all our pocket and box chronometers: indeed it ſeems a ſcapement indiſpenſably neceſſary for theſe purpoſes. Yet Berthoud in his famous time-keeper, No. 8, uſed a very different one from that of the detached ſort.

The origin of the common detached 'ſcapement aſcribed to Thiout.

I am,
Sir, with much eſteem, Your's,
THOMAS REID.

Edinburgh, *25th March*, 1805.

II.

An Analyſis of the Magnetical Pyrites; with Remarks on ſome of the other Sulphurets of Iron. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

(Concluded from page 276.)

§ VII.

Whether the artificial pyrites with minimum of ſulphur be, like the natural, magnetical.

So far, therefore, as can be proved by ſimilarity in chemical properties and analyſis, the magnetical pyrites is indiſputably a natural ſulphuret, completely the ſame with that which till now has been only known as an artificial product; but, that the mind may be perfectly ſatisfied, another queſtion muſt be ſolved, namely, how far do they accord in receiving and retaining the property of magnetiſm? common pyrites do not appear to affect the magnetic needle, or, if ſome of them ſlightly act by attraction, (which however I never could perceive, nor recollect to have read in works expreſsly relating to magnetiſm,) yet they do not poſſeſs, nor appear capable of acquiring, any magnetic polarity. As, therefore, the iron of pyrites is undoubtedly in the metallic ſtate, and in a conſiderable proportion, the deſtruction of this characteriſtic property of metallic iron, muſt be aſcribed to the other ingredient, ſulphur.

The artificial compound is not a mere mixture.

But we have lately ſeen, that a natural combination of iron with 36.50 or 37 *per cent.* of ſulphur, is in poſſeſſion of all the properties ſuppoſed hitherto to appertain (in any marked degree) almoſt excluſively to the well known magnetic iron ore; and that the combination alluded to is ſtrictly chemical, and not (as at firſt might have been imagined) a mixture of particles of magnetic iron ore with common pyrites *.

The compound directly formed at red heat.

This is certainly very remarkable; and it induced me to examine the effects produced by ſulphur, on the capacity of metallic iron for receiving and retaining the magnetic properties. I therefore prepared ſome ſulphuret of iron, by adding a large quantity of ſulphur to fine iron wire, in a moderate red heat.

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* This has been ſufficiently proved, by the facts which have been ſtated; I ſhall however add, that upon digeſting a mixture of the powder of common pyrites and iron filings in muriatic acid, I only obtained hydrogen gas, exactly as if I had employed the iron filings without the pyrites.

The internal colour and luſtre of the product, were not very unlike thoſe of the magnetical pyrites; and, after the maſs had been placed during a few hours between magnetical bars, I found that it poſſeſſed ſo ſtrong a degree of polarity, as to attract or repel the needle completely round upon its pivot; and although ſeveral weeks have elapſed ſince it has been removed from the magnetical bars, it ſtill retains its power, with little diminution; like the magnetical pyrites, however, in its natural ſtate, it is not ſufficiently powerful to attract and take up iron filings.

It reſembled common pyrites; but was capable of magnetiſm.

But this ſulphuret did not contain ſo much ſulphur as the magnetical pyrites; I therefore mixed ſome of it, reduced to powder, with a large quantity of ſulphur, and ſubjected it to diſtillation in a retort, which was at length heated until the intire bulb became red.

It contained leſs ſulphur than the magnetical pyrites. More ſulphur added and low ignition.

The ſulphuret, by this operation, had aſſumed very much the appearance of the powder of common pyrites, in reſpect to colour; but, in its chemical properties, ſuch as ſolubility in muriatic acid, with the production of ſulphuretted hydrogen gas, as well as in the nature of the precipitates it afforded with pruſſiate of potaſh and with ammonia, it perfectly reſembled the magnetical pyrites. Moreover, by analyſis, it was found to conſiſt of 35 parts of ſulphur and 65 of iron; and although (being in a pulverulent ſtate) its power, as to receiving and retaining the magnetic property, could not ſo eaſily be examined, yet, by being powerfully attracted by the magnet, with ſome other circumſtances, there was every reaſon to conclude, that in this reſpect alſo it was not inferior.

It was attractable by the magnet.

Another proportion of ſulphuret was formed, as above deſcribed; it was placed between magnetical bars, and, in like manner, received and retained the magnetic power.

It is certain, therefore, that when a quantity of ſulphur equal to 35 or 37 *per cent.* is combined with iron, it not only does not prevent the iron from receiving the magnetic fluid, but enables it to retain it, ſo that the maſs acts in every reſpect as a permanent magnet.

Hence 36 or 37 per cent. does not prevent iron from becoming a magnet.

Black oxide of iron, by one operation, does not appear to combine with ſulphur ſo readily as iron filings; a ſecond operation, however, converts it into a ſulphuret, very much reſembling that which has juſt been deſcribed, including the

Black oxide of iron combines with ſulphur leſs readily.

chemical as well as the magnetical properties; but, undoubtedly, by theſe proceſſes, it is progreſſively converted, perfectly or very nearly, into the metallic ſtate.

Iron combined with a larger proportion of oxygen, ſuch as the fine gray ſpecular iron from Sweden, will not form a ſulphuret by the direct application of ſulphur, in one operation; although it becomes of a dark brown colour, partly irideſcent, and is moderately attracted by a magnet.

Magnetical pyrites combined with 9 *per cent.* more of ſulphur,

Fifty grains of the magnetical pyrites, reduced to powder, and mixed with three times the weight of ſulphur, were diſtilled in a retort, until the bulb became moderately red-hot. After the diſtillation, the pyrites weighed 54.50; conſequently, the addition of ſulphur was 9 *per cent.* making the total = 45.50 or 46 *per cent.* The powder was become greeniſh-yellow, very like that of the common pyrites: it did not afford any ſulphuretted hydrogen, when digeſted in muriatic acid; but it nevertheleſs was partially diſſolved, and the ſolution, when examined by pruſſiate of potaſh, and by ammonia, was not different from that of the crude magnetical pyrites.

—was ſtill attracted by the magnet.

The powder which had been diſtilled with ſulphur, and which had thus received an addition of 9 *per cent.* to its original quantity, *was ſtill capable of being completely taken up by a magnet.*

Iron ceaſes to be acted on by the magnet when the doſe of ſulphur is at ſome point between 46 and 52 *per cent.*

From the whole of the experiments which have been related, it is therefore evident, that iron, when combined with a conſiderable proportion of ſulphur, is not only ſtill capable of receiving the magnetic property, but is alſo thereby enabled to retain it, and thus (as I have already remarked) becomes a complete magnet; and it is not a little curious, that iron combined (as above ſtated) with 45 or 46 *per cent.* of ſulphur, is capable of being taken up by a magnet, whilſt iron combined with 52 *per cent.* or more, of ſulphur, (although likewiſe in the metallic ſtate,) does not ſenſibly affect the magnetic needle; and hence, ſmall as the difference may appear, there is reaſon to conclude, that the capacity of iron for magnetic action is deſtroyed by a certain proportion of ſulphur, the effects of which, although little if at all ſenſible at 46 *per cent.* are yet nearly or quite abſolute, in this deſtruction of magnetic influence, before it amounts to 52. But, what the exact intermediate proportion of ſulphur may be, which is adequate to produce this effect, I have not as yet determined by actual experiment.

As

Sulphur acts on iron like carbon in cauſing it to contain magnetiſm,

As carbon acts on ſoft iron, (which, although it moſt readily receives the magnetic influence, is unable to retain it ſo as to become a magnet, without the addition of a certain proportion of carbon, by which it is rendered hard and brittle, or, in other words, is converted into ſteel, ſo, in like manner, does ſulphur ſeem to act; for it has been proved, by the preceding experiments, that the brittle maſs formed by the union of a certain proportion of this ſubſtance with iron, whether by nature or by art, becomes capable of retaining the magnetic virtue, and of acting as a complete magnet.

—and ſo likewiſe does phoſphorus.

This remarkable coincidence, in the effects produced on iron by carbon and ſulphur, induced me to try the effects of phoſphorus; and my hope of ſucceſs was increaſed by the remark of Mr. Pelletier, who ſays, that "the phoſphuret of iron is attracted by the magnet;"* and therefore, although certain bodies may be thus attracted, without being capable of actually becoming permanent magnets, I was deſirous to examine what might be the power, in this reſpect, of phoſphuret of iron.

Experiment. Phoſphuret of iron was made;

—and by the touch formed a powerful magnet,

I therefore prepared a quantity of phoſphuret of iron, in the direct way, viz. by adding phoſphorus, cut into ſmall pieces, to fine iron wire made moderately red-hot in a crucible. The uſual phenomena took place, ſuch as the brilliant white flame, and the rapid melting of the iron, which, when cold, was white, with a ſtriated grain, extremely brittle, hard, and completely converted into a phoſphuret. The fragments of this were powerfully attracted by a magnet; and, after I had placed two or three of the largeſt pieces, during a few hours, between magnetical bars, I had the pleaſure to find that theſe had become powerful magnets, which not only attracted or repelled the needle completely round, but were able to take up iron filings, and ſmall pieces, about half an inch in length, of fine harpſichord wire; and, although they have now been removed from the magnetical bars more than three weeks, I cannot diſcover any diminution of the power which had thus been communicated to them.

The three inflammable ſubſtances, *carbon*, *ſulphur*, and *phoſphorus*, which, by their chemical effects on iron, in many reſpects reſemble each other, have now therefore been proved alike to poſſeſs the property of enabling iron to retain the power of magnetiſm; but I ſhall conſider this more fully in the following ſection.

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* "Le Phoſphure de Fer eſt attirable a l'aimant." Annales de Chimie, Tome XIII. p. 114.

§ VIII.

From the whole which has been ſtated we find,

General reſults.

Magnetic pyrites is a Britiſh mineral,

1. That the ſubſtance called magnetical pyrites, which has hitherto been found only in Saxony and a few other places, is alſo a Britiſh mineral, and that, in Caernarvonſhire, it forms a vein of conſiderable extent, breadth, and depth.

—containing about 37 ſulphur and 63 iron.

2. That the component ingredients of it are ſulphur and metallic iron; the former being in the proportion of 36.50 or 37, and the latter about 63.50 or 63.

It differs in its properties from common pyrites which contains more ſulphur.

3. That the chemical and other properties of this ſubſtance are very different from thoſe of the common martial pyrites, which however are alſo compoſed of ſulphur and iron, varying in proportion, from 52.15 to 54.34 of ſulphur, and from 47.85 to 45.66 of metallic iron; the difference between the common pyrites which were examined being therefore 2.19, and the mean proportions amounting to 53.24 of ſulphur, and 46.75 of iron; conſequently, the difference between the relative proportions, in the compoſition of the magnetical pyrites and of the common pyrites, is nearly 16.74, or 16.24.

It is identically the ſame as the artificial ſulphuret.

4. That, as the magnetical pyrites agrees in analytical reſults, as well as in all chemical and other properties, with that ſulphuret of iron which hitherto has been only known as an artificial product, there is no doubt but that it is identically the ſame; and we may conclude, that its proportions are moſt probably ſubjected to a certain law, (as Mr. Prouſt has obſerved in the caſe of the artificial ſulphuret,) which law, under certain circumſtances, and eſpecially during the natural formation of this ſubſtance in the humid way, may be ſuppoſed to act in an almoſt invariable manner.

In common pyrites the ſulphur predominates.

5. That, in the formation of common martial pyrites, there is a deviation from this law, and that ſulphur becomes the predominant ingredient, which is variable in quantity, but which, by the preſent experiments, has not been found to exceed 54.34 *per cent.* a proportion, however, that poſſibly may be ſurpaſſed in other pyrites, which have not as yet been chemically examined.

6. That

Limits of the ſulphur in magnetic pyrites.

6. That iron, when combined naturally or artificially with 36.50 or 37 of ſulphur, is not only ſtill capable of receiving the magnetic fluid, but is alſo rendered capable of retaining it, ſo as to become in every reſpect a permanent magnet; and the ſame may, in a great meaſure, be inferred reſpecting iron which has been artificially combined with 45.50 *per cent.* of ſulphur.

Proportion beyond which the magnetic influence is loſt.

7. That beyond this proportion of 45.50 or 46 *per cent.* of ſulphur, (in the natural common pyrites,) all ſuſceptibility of the magnetic influence appears to be deſtroyed; and, although the preciſe proportion which is capable of producing this effect, has not yet been determined by actual experiment, it is certain that the limits are between 45.50 and 52.15; unleſs ſome unknown alteration has taken place in the ſtate of the ſulphur, or of the iron in the common martial pyrites.

As carbon renders iron tenacious of magnetiſm, ſo alſo do ſulphur and phoſphorus.

8. That, as carbon, when combined in a certain proportion with iron, (forming ſteel,) enables it to become a permanent magnet, and as a certain proportion of ſulphur communicates the ſame quality to iron, ſo alſo were found to be the effects of phoſphorus; for the phoſphuret of iron, in this reſpect, was by much the moſt powerful, at leaſt when conſidered comparatively with ſulphuret of iron.

Concluſion.

9. And laſtly, that as carbon, ſulphur, and phoſphorus, produce, by their union with iron, many chemical effects of much ſimilarity, ſo do each of them, when combined with that metal in certain proportions, not only permit it to receive, but alſo give it the peculiar power of retaining, the magnetical properties; and thus, henceforth, in addition to that carburet of iron called ſteel, certain ſulphurets and phoſphurets of iron may be regarded as bodies peculiarly ſuſceptible of ſtrong magnetical impregnation.

Having thus, for the greater perſpicuity, reduced the principal facts of this Paper into a conciſe order, I ſhall now make ſome general obſervations.

Remarks.

The magnetical pyrites is an intereſting product.

It is undoubtedly not a little ſingular, that a ſubſtance like the magnetical pyrites, which, although not common, has been long known to mineralogiſts, ſhould not hitherto have been chemically examined, eſpecially as mineralogical authors have mentioned the analyſis of it as a deſideratum. The reſult of this which I have attempted, proves that it is really deſerving of notice; for thus we have aſcertained, that the ſulphuret of iron hitherto known only as an artificial product, is alſo formed by nature; and that the compoſition of this laſt, agrees with thoſe proportions of the artificial ſulphuret which have been ſtated by Mr. Prouſt.

No intermediate natural product between the common and the mag. pyrites.

But, from this ſulphuret or magnetical pyrites, I have not, by analyſis, as yet been able to diſcover any regular or immediate gradations into the common pyrites; for the leaſt proportion of ſulphur in theſe amounted to 52.15, and the greateſt proportion to 54.34; ſo that, between the magnetical and the common pyrites, the difference is conſiderable, in the proportions of their component ſubſtances, as well as in their phyſical and chemical properties; whilſt the difference which I have hitherto been able to detect in the proportions of ſome of the common pyrites (very diſſimilar in figure, luſtre, colour, and hardneſs,) has only amounted to 2.19.

Remarks on Prouſt's experiments.

Mr. Prouſt, in a general way, conſiders common pyrites to differ from the firſt ſulphuret, or that compoſed of 60 parts of ſulphur and 100 of iron, (= 37.50 *per cent.*) by containing a farther addition of half the above quantity of ſulphur, or 90 parts of ſulphur and 100 of iron, (= 47.36 *per cent.*) but this opinion he appears to have formed, in conſequence of reſults obtained by ſynthetical experiments made in the dry way.— Now, when we conſider how difficult it is to regulate the high degrees of temperature, and what a numerous chain of alterations in the relative order of affinities moſt commonly reſult from alterations in theſe degrees of heat, it ſeems to me that we cannot rely, with abſolute certainty, on ſynthetical experiments made in the above way, unleſs they are corrected, and contraſted with analytical experiments made on the ſame ſubſtances. But it does not appear, from the two memoirs publiſhed by Mr. Prouſt, to which I have ſo frequently alluded, that that gentleman did more, in reſpect to analyſis, than diſtil the cubic and dodecaedral pyrites found near Soria, from which he obtained about 20 *per cent.* of ſulphur; and, having obſerved that the reſiduum poſſeſſed the properties of the ſulphuret which has been commonly prepared in laboratories, he concluded that the ſulphur obtained from the pyrites, is the exceſs of that proportion which is requiſite to form the ſulphuret, the proportions of which, therefore, he by ſyntheſis aſcertained to be, as I have above ſtated, = 37.50 of ſulphur, and 62.50 of iron, or 60 of ſulphur combined with 100 of iron; and laſtly, having formed 318 grains of this ſulphuret from 200 grains of iron filings, he diſtilled the ſulphuret with an additional quantity of ſulphur, in an inferior degree of heat, and obtained 378 grains of a ſubſtance which, excepting denſity, was ſimilar to the common martial pyrites.*

He did not make a regular analyſis.

It is however to be regretted, that Mr. Prouſt did not make a regular analyſis of the pyrites of Soria, and of the reſiduum after diſtillation; for (unleſs theſe pyrites are very different from thoſe which I have examined) he would moſt probably have found the proportion of ſulphur greater than that which he has aſſigned to natural pyrites in general. This at leaſt there is great reaſon to ſuppoſe, if we allow that moſt or all of the pyrites have been formed in the humid way, by which, we may conceive, a larger proportion of ſulphur may be introduced into the compound, than can take place in high degrees of temperature. And this opinion is corroborated by the reſults of my analyſes; for, inſtead of finding the general proportions to be 47.36 of ſulphur and 52.64 of iron, the mean reſult of theſe analyſes is very nearly the reverſe, being 53.24 of ſulphur and 46.76 of iron.

Prouſt apprehends that pyrites holding the leaſt ſulphur are moſt liable to vitriolization:

Mr. Prouſt is alſo of opinion, that the pyrites which contain the ſmalleſt quantity of ſulphur, are thoſe which are moſt liable to vitriolization; and, on the contrary, that thoſe which contain the largeſt proportion, are the leaſt affected by the air or weather.† This opinion of the learned profeſſor, by no means accords with ſuch obſervations as I have been able to make; for the cubic, dodecaedral, and other regularly cryſtallized pyrites, are liable to oxidizement, ſo as to become what are called hepatic iron ores, but not to vitriolization; whilſt the radiated pyrites (at leaſt thoſe of this country) are by much the moſt ſubject to the latter effect; and therefore, as the reſults of the preceding analyſes ſhow that the cryſtallized pyrites contain leſs ſulphur than the radiated pyrites, I might be induced to adopt the contrary opinion. But I am inclined to attribute the effect of vitriolization obſerved in ſome of the pyrites, not ſo much to the proportion, as to the ſtate of the ſulphur in the compound; for I much ſuſpect, that a prediſpoſition to vitriolization, in theſe pyrites, is produced by a ſmall portion of oxygen being previouſly combined with a part, or with the general maſs,

But this diſpoſition moſt probably ariſes from a commencement of oxidation.

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* *Journal de Phyſique,* Tome LIV. p. 92.

† *Journal de Phyſique,* Tome LIII. p. 91.

of the ſulphur, at the time of the original formation of theſe ſubſtances, ſo that the ſtate of the ſulphur is tending to that of oxide, and thus the acceſſion of a farther addition of oxygen becomes facilitated. We have an example of ſimilar effects in phoſphorus, when (as is commonly ſaid) it is half burned, for the purpoſe of preparing the phoſphorus bottles; and the propenſity to vitriolization, obſerved in many of the half-roaſted ſulphureous ores, appears to me to ariſe from this cauſe, rather than from the mere diminution of the original proportion of ſulphur, or the actual immediate converſion of part of it into ſulphuric acid; nevertheleſs, I offer this opinion, at preſent, only as a probable conjecture, which may be inveſtigated by future experiments and obſervations.

The magnetical properties of the ſulphuret is a remarkable fact.

The magnetical properties of the ſulphuret of iron which forms the principal ſubject of this Paper, muſt be regarded as a remarkable fact; for I have not found, in the various publications on magnetiſm which I have had the means of conſulting, even the moſt remote hint, that iron when combined with ſulphur, is poſſeſſed of the power of receiving and retaining the magnetic fluid; and, judging by the properties of common pyrites, we might have ſuppoſed that ſulphur annihilated this power in iron, as indeed ſeems to have been the opinion of mineralogiſts, who have never enumerated magnetical attraction amongſt the phyſical properties of thoſe bodies; and, although Werner, Widenmann, Emmerling, and Brochant, have arranged the magnetical pyrites with the ſulphurets of iron, yet the magnetical property could not with certainty be ſtated as inherent in the ſulphuret, for, at that time, this ſubſtance had not been ſubjected to a regular chemical analyſis, and the magnetical property might therefore be ſuſpected to ariſe from interſperſed particles of the common magnetical iron ore. This probably has been the opinion of the Abbé Haüy; for, in his extenſive Treatiſe on Mineralogy lately publiſhed, I cannot find any mention made of the magnetical pyrites, either amongſt the ſulphurets or amongſt the other ores of iron.

Little or ſcarcely noticed.

The magnet ſaid to conſiſt of iron with 10 to 20 oxigen.

In the mineral kingdom, a great variety of ſubſtances, and even ſome of the gems, exert a feeble degree of attraction on the magnetic needle, and ſometimes alſo acquire a ſlight degree of polarity;[*] but, as this wonderful property has only been obſerved conſpicuouſly powerful in one ſpecies of iron ore, this has been always emphatically called *the Magnet,*[*] and is ſaid to conſiſt of metallic iron combined with from 10 to 20 *per cent* of oxygen.

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[*] Cavallo on Magnetiſm, page 73.

The ſulphuret is a different kind of magnet.

From the facts, however, which have been recently ſtated, we now find that there is another natural ſubſtance, apparently very different from the magnet in chemical compoſition, but nevertheleſs approaching very nearly to it in power, which is found in ſeveral parts of our globe, and particularly in a province of this kingdom, where it conſtitutes a vein, running north and ſouth, of a conſiderable extent, and ſeveral yards in width and thickneſs.

The phoſphuret magnet.

From the experiments alſo, which have been made on the artificial preparation of this ſubſtance, we find, that it is capable of receiving the magnetic properties when the proportion of ſulphur amounts to 37 *per cent.* and is ſtill powerfully attracted when a much larger quantity of ſulphur is preſent. There is, however, ſome point at which all theſe effects ceaſe, and this point appears to be, when the ſulphur is in ſome proportion between 45 or 46 and 52 *per cent.* The preceding experiments have alſo proved, that iron when combined with phoſphorus, likewiſe poſſeſſes the power of becoming a magnet to a very remarkable degree; and, by the ſimilarity, in this reſpect, of the carburet of iron called ſteel, to the above ſulphuret and phoſphuret, a very remarkable analogy is eſtabliſhed between the effects produced on iron, by carbon, ſulphur, and phoſphorus.

Of carburets, plumbago is not magnetical: other ſteel compounds form powerful magnets.

Carbon, when combined in a very large proportion with iron, forms the carburet of that metal called plumbago; a brittle ſubſtance, inſoluble in muriatic acid, and deſtitute of magnetical properties. But, ſmaller proportions of carbon, with the ſame metal, conſtitute the various carburets included between black caſt iron and ſoft caſt ſteel;[†] bodies which are more or leſs brittle, ſoluble in muriatic acid, and more or leſs ſuſceptible of magnetical impregnation; ſome of them form the moſt powerful magnets hitherto diſcovered.

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[*] In a future Paper, it is my intention to give an account of ſome comparative analyſes of the varieties of this ſubſtance.

[†] "When the carbon exceeds, the compound is carburet of iron "or plumbago: when the iron exceeds, the compound is ſteel, or "caſt iron, in various ſtates, according to the proportion. All "theſe compounds may be conſidered as ſubcarburets of iron."— Thomſon's Syſtem of Chemiſtry, Vol. I. p. 165.

Mr.

Sulphur and iron have ſimilar habitudes.

Sulphur, in like manner, combines with iron in a large proportion, forming the common pyrites, which are brittle, almoſt or quite inſoluble in muriatic acid, and devoid of magnetical properties. Sulphur in ſmaller proportions, forms ſulphurets which are alſo brittle, but are ſoluble in muriatic acid, and ſtrongly ſuſceptible of magnetical impregnation.

So likewiſe phoſphorus and iron.

Phoſphorus alſo, when combined with iron, makes it brittle, and enables it powerfully to receive and retain the magnetical properties; ſo that, conſidering the great ſimilarity which prevails in other reſpects, it may not ſeem raſh to conclude, that phoſphorus (like carbon and ſulphur,) when combined with iron in a very large proportion, may form a ſubſtance incapable of becoming magnetical, although, in ſmaller proportion, (as we have ſeen,) it conſtitutes compounds which are not only capable of receiving, but alſo of retaining, the magnetical properties, even ſo far as, in ſome caſes, to ſeem likely to form magnets of great power; and, ſpeaking generally of the carburets, ſulphurets, and phoſphurets of iron, I have no doubt but that, by accurate experiments, we ſhall find that a certain proportion of the ingredients of each, conſtitutes a maximum in the magnetical power of theſe three bodies. When this maximum has been aſcertained, it would be proper to compare the relative magnetical power of ſteel (which hitherto has alone been employed

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Mr. Muſhet, in the following Table, exhibits the proportion of charcoal which diſappeared, during the converſion of iron to the different varieties of ſubcarburet known in commerce.

" Charcoal abſorbed.	Reſult.
$\frac{1}{120}$ - - -	Soft caſt ſteel.
$\frac{1}{100}$ - - -	Common caſt ſteel.
$\frac{1}{90}$ - - -	The ſame, but harder.
$\frac{1}{50}$ - - -	The ſame, too hard for drawing.
$\frac{1}{25}$ - - -	White caſt iron.
$\frac{1}{20}$ - - -	Mottled caſt iron.
$\frac{1}{15}$ - - -	Black caſt iron.

" When the carbon amounts to about $\frac{1}{60}$ of the whole maſs, the " hardneſs is at the maximum." Thomſon, Vol. I. p. 166; and Phil. Magazine, Vol. XIII. pp. 142 and 148.

ployed to form artificial magnets) with that of ſulphuret and phoſphuret of iron; each being firſt examined in the form of a ſingle maſs or bar of equal weight, and afterwards in the ſtate of compound magnets, formed like the large horſe-ſhoe magnets, by the ſeparate arrangement of an equal number of bars of the ſame ſubſtance in a box of braſs.

An extenſive field for experiments of intereſt and importance.

The effects of the above compound magnets ſhould then be tried againſt others, compoſed of bars of the three different ſubſtances, various in number, and in the mode of arrangement; and, laſtly, it would be intereſting to make a ſeries of experiments on chemical compounds, formed by uniting different proportions of carbon, ſulphur, and phoſphorus, with one and the ſame maſs of iron. Theſe quadruple compounds, which, according to the modern chemical nomenclature, may be called carburo-ſulphuro phoſphurets, or phoſphuro-ſulphuro-carburets, &c. of iron, are as yet unknown as to their chemical properties, and may alſo, by the inveſtigation of their magnetical properties, afford ſome curious reſults. At any rate, an unexplored field of extenſive reſearch appears to be opened, which poſſibly may furniſh important additions to the hiſtory of magnetiſm, a branch of ſcience which of late years has been but little augmented, and which, amidſt the preſent rapid progreſs of human knowledge, remains immerſed in conſiderable obſcurity.

III.

Extract of a Memoir of Mr. ERMAN, *entitled Obſervations and Doubts concerning Atmoſpheric Electricity.*

(Concluded from p. 300, Vol. X.)

Two electrometers which do not indicate divergence alone, ſhew it when one is paſſed above the other.

AN electrometer furniſhed with a rod three feet in length, and placed in the open air, does not ſhew any divergence; but when the bent point of another electrometer, which alſo exhibits no divergence, is moved above the firſt, and even when the motion is parallel to the horizon, the leaves of the latter will be ſeen to diverge negatively, without the ſecond giving any ſign of electricity.

This effect probably ariſes from the diviſion of the electricity.

It is very probable that the effect of theſe vapourous and aqueous meteorological maſſes is manifeſted at the points of the electrometer, by this ſame action, which depends on the diviſion of the electricity, and this explains the ſudden changes which ſupervene in the poſitive or negative electric ſtate of the earth: it is not even probable that the clouds poſſeſs in themſelves a negative electricity, or that the vapours of water ſhould be always in this ſtate. Mr. Erman thinks he can prove that the vapours of water are only negative when the body from which they proceed is inſulated; but that they become poſitive as ſoon as it is brought into contact with the earth. Rain, after its fall, leaves bodies in the ſtate of *minus* electricity, which is agreeable to the preceding experiments; ſnow produces this effect ſo much the better, as it leaves the ſurrounding air in a ſtate of dryneſs, which augments its inſulating properties.

The poſitive or negative ſtate of aqueous vapour depends on inſulation.

Influence of the clouds on the earth.

The clouds which have a tendency to rain or ſnow muſt neceſſarily produce their influence on the ground, and it is for this reaſon that the oppoſition of the cloud and the ground is ſo quickly manifeſted. It would be intereſting to explain the complication ariſing from the variations which the electricity of the earth undergoes, as well with reſpect to its ſpecies as to its intenſity, and Mr. Erman is at preſent engaged in this ſubject.

The different phenomena of the divergence of an inſulated point explained by the ſame law.

With reſpect to the variable degree of the poſitive divergence of an inſulated point fixed in the earth, it may perhaps be attributed to the greater or leſs conducting quality of the ſurrounding air: the point, when it is very well inſulated from the earth, ſhows a certain degree of poſitive charge when the air is inſulated ſo that electricity cannot be communicated to it; it ſhows zero when the conducting power of the air is nearly equal to the rapidity with which the charge is made; and negative when this power is very ſtrong. Theſe phenomena are thus explained by the ſame law.

Probability that there is no free electricity in the atmoſphere.

Mr. Erman has alſo ſuppoſed in his memoir that there is not any quantity of electricity diſengaged in the atmoſphere; but he does not however aſſert that this is ſtrictly the caſe: he has only ſought to draw the attention of philoſophers to this ſubject, and to ſhow that theſe phenomena are equally well explained by the ſphere of activity of the electricity.

Convenience of the inſtruments.

The experiments are very eaſily made, and the electrometers he uſes are very portable; for when the leaves of gold are placed on one ſide of the cylinder, motion cannot occaſion any accident. With reſpect to the ſticks, they may be made of ſeveral pieces, and be ſcrewed together when they are wanted. Such were the inſtruments Mr. Erman took with him over ſeveral hundred leagues; the ſtick was three lines in diameter at one end, and one line at the other. This philoſopher does not yet know what may be the influence of the different thickneſs of the conductor. One of theſe conductors happened to be of braſs, the other of ſteel; it did not appear to him that the difference of the metal occaſioned any in the reſults. But he purpoſes making new enquiries on this ſubject.

Mr. Erman offers his concluſions with great diffidence: he does not pretend to have formed a new theory, but only to ſtate his doubts on the opinion of thoſe philoſophers who have attributed the phenomena hitherto obſerved to a diſengaged electricity in the atmoſphere. He notices ſeveral other intereſting experiments which he is employed in making upon ſmoke, and particularly on electricity in a vacuum. We ſhall haſten to publiſh the reſults of them as ſoon as they come to hand.

IV.

Deſcription of a Compenſation Curb. By Mr. JAMES SCOTT.

To Mr. NICHOLSON.

SIR,

INCLOSED I ſend you a ſketch of an inſtrument I have conſtructed, much wanted in the profeſſion of watchmaking, for the purpoſe of publiſhing in your Philoſophical Journal, which I preſume will be of much benefit to the public, and at the ſame time may prevent any other perſon from claiming it. Introductory letter.

I ſhall call it a compenſation curb. The conſtruction of this inſtrument is for the purpoſe of expanding and contracting in the different temperatures, ſo as to counteract the error which the pendulum-ſpring is liable to by the ſmalleſt variation of heat or cold.

The inventions that have hitherto been put in practice for the ſame purpoſe, I beg leave to make a few remarks on.—The compound balance, when carefully made and adjuſted, is certainly a very complete counteracting expanſion, and will anſwer exceedingly well on board a ſhip, if there be no material difference in the denſity of the air; if otherwiſe, the balance being loaded, will have to encounter a conſiderable deal more friction, and conſequently be impeded in its vibrations: if worn in the pocket, it is alſo liable to error, as exercise will alter its diameter. The compound balance has heretofore got the preference, becauſe artiſts have not been able to invent a compenſation curb adjuſtable to the exact expanſion required (which, by many experiments, I have proved the incloſed to be fully competent to); therefore, the plain balance having no projections on the ſurface, muſt certainly have the advantage. As I am not in the habit of expreſſing my ideas to the public, I hope you will have the goodneſs to rectify any errors in the ſtile, and alſo curtail any part which may appear unneceſſary to you.

I remain, Sir,

Your much obliged humble ſervant,

JAMES SCOTT.

39, *Grafton Street, Dublin.*

The following is a Deſcription of the Compenſation Curb.

Deſcription of a compenſation curb.

The ſteel index, *Pl. II. Fig.* 2, letter A, is for the purpoſe of ſupporting the curb, which is faſtened by a ſcrew and ſteady pin at R, the circle of which at A is turned with a dovetail, as ſhewn at H, and is ſlit ſo as to ſnap into the frame-plate, by which means it may be turned, and will carry the curb, ſo as to regulate the machine in the common way. D and E are two circles compoſed of braſs and ſteel ſoldered together, the outſide of E braſs and the inſide ſteel; but the outſide of D is ſteel and the inſide braſs; ſo that the one circle expands when the other contracts; by which means the acting part of the curb at C will ſhift towards the index with heat, and prevent the vibrations being ſlower, which the expanſion of P, the regulating ſpring and the balance, would otherwiſe occaſion; and on the contrary with cold, it will ſhift its poſition nearer the ſtud I, which the regulating ſpring is pinned to; ſo that let it receive heat or cold, the acting part of the curb at C will at all times keep the regulating ſpring the exact length, to counteract the expanſion of the balance and pendulum-ſpring. V is a piece of ſteel, with a notch cut in it to receive the expanſion circles D and E. F is a ſcrew for faſtening V at any part of the circles; ſo that, by ſtoving the watch, Deſcription of a compenſation curb.

you will immediately aſcertain towards what part of the circles you muſt ſhift V. If you find its rate ſlow with heat, you muſt ſhift V to lengthen your circles, and if faſt, the contrary; and by marking your circles each time you have occaſion to ſhift V, you will be enabled to adjuſt the curb to the exact expanſion required. W is a looſe piece of ſteel, which fits in the notch of V between the two expanſion circles, to keep them faſt in their proper poſitions when ſcrewed by F. S S are two ſteady pins made faſt in the frame-plate, which receive the circle E between them to prevent the action of P, the regulating ſpring, from affecting C, the curb, by moving it to or from the center during the going of the watch. The two expanſion circles require to be made very delicate; it is therefore to be obſerved by the manufacturer of this inſtrument, that the braſs in each circle is to be the thickneſs of the ſteel; ſo that when the two bodies are ſoldered together, they will make two thickneſſes of the edge of a main-ſpring of a watch. The larger your watch will admit the diameter of the curb, the better. It muſt be at leaſt the ſize of the balance.

V.

Letter from Mr. BOSWELL, *in Anſwer to* AN OLD CORRESPONDENT.

To Mr. NICHOLSON.

SIR, *April* 10, 1805.

Mr. Boſwell vindicates himſelf from confidence.

THE perſon ſigning *your Old Correſpondent*, has thrown ſome very undeſerved reflections on my laſt communication in your Journal.

In anſwer to his remarks, I beg leave to obſerve that the introduction to my paper contains ſufficient to juſtify me fully from his imputations.

I have there aſſerted no more than that "I have diſcovered "a method of coming ſo *near the truth*, that ſhould it turn "out to be in reality *not ſo exact* as appears to me, yet it promiſes to be ſo uſeful for *common computations* that I am "induced to ſend it for publication, *if you approve of it*."

I know not, Sir, how I could have written in any way farther from "announcing my diſcovery with confidence" than the above, or indeed with more diffidence.

His methods were ſtated as *nearly approaching the truth.*

n the firſt place, I ſtate it only as a *near approach* to the exact truth, which both your and his criticiſms have fully proved it to be: In the next place, I expreſs my doubt of its being even *ſo exact* as it appears to me: And thirdly, I have left its publication to depend *on your approbation.*

If this is not enough to remove all ſuſpicion of confidence, I have to add, that the ſentence which concludes that ſubject in the paper, ſtates, that an unperceived error might ariſe from the ſmallneſs of the circles which I uſed; and beſides this you can teſtify for me, that I wiſhed the paper to be ſuppreſſed altogether before publication, when you ſhewed me that it even wanted an hundredth part of being exact, though with a much greater inaccuracy the matter contained in it would be uſeful for the purpoſes to which I ſtated it might be applied.

As to the proof of the ſecond *fact*, it was not from the "conviction of its obvious accuracy not requiring proof" that I did not inſert any, but becauſe I concluded it muſt be ſufficiently obvious after what I had ſtated of the firſt, that it was the ſame ſort of *experimental* proof I had uſed for both: And if any gentleman will try the experiment as I did, he will find I have not misſtated the matter.

There may indeed be ſome little impropriety in uſing the word *fact* in a popular ſenſe, in any thing like a mathematical ſtatement; but to notice ſuch a trifle with inverted commas, only appears to indicate a ſpirit of cavilling on the part of your Old Correſpondent.

None of the quadratures of the circle are more than approximations.

But with all his preciſion he has forgot one fact, that puts his computations more on a level with my experiments, which is, that no method has ever yet been diſcovered of computing with *perfect exactneſs* the relative proportions of the circumference to the diameter, and of courſe to the other lines he mentions; and that it is only a far-laboured *approximation* to the truth that has been inſerted in the work from whence he has extracted the proportions which he has uſed; and that therefore what he has "announced with ſo much confidence," (to uſe his own words) is not preciſely demonſtrative truth, but only an approach to it.

I did not ever intend to recommend my method as a perfect and infallible geometrical problem applicable to the more ſublime mathematical ſpeculations, but as a more ready, and, permit me to add, more exact way than that in general uſe, for the humbler purpoſes of common life, ſuch as meaſuring round timber, conduit pipes, engine cylinders, &c.

Mr. B.'s methods have practical utility.

I am, &c.

J. WHITLEY BOSWELL.

VI.

Deſcription of an improved Gate for Fields. By Mr. Charles Waistell [*].

Dear Sir,

THE various methods uſed in bracing common gates for fields, prove that not one of them is greatly ſuperior to the reſt; for, if it was, that method would have been generally adopted. Moſt gates are loaded with ſuperfluous timber in ſome of their parts, and are conſtructed upon ſuch bad principles, that they are frequently broken by their own weight, aided by the concuſſion of the head againſt the falling-poſt; and this, long before any part of the wood has begun to decay. I have for ſome time given this ſubject conſiderable attention, being impreſſed with the idea, that if common gates could be conſtructed with leſs timber, and upon better principles, the ſaving of timber only would be of national importance; for we have many millions of gates to uphold in Britain, and their numbers are annually increaſing. The reſult of my labours has been the plan which accompanies this letter. Gates made according to it, poſſeſs great ſtrength, are very light, and of eaſy and ſimple conſtruction. Although uniformity of appearance be not eſſential in a common gate, yet is worth having when it can be obtained, as in this gate, without additional expenſe.

Imperfections of common gates.

Great ſaving in timber if they were made leſs cumberſome.

New plan.

My gate is made with ſhort, and conſequently leſs valuable, oak or aſh timber, than thoſe of the commoneſt conſtruction; its ſtrength is much greater than any other gate made with a like quantity

Account of the author's improvements.

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[*] Communicated in a letter to Charles Taylor, Eſq. Secretary of the Society of Arts, who returned their thanks for the ſame.

quantity of timber, there being at four diſtant points between the head and the heel, two bars and a brace croſſing each other: and I doubt not that it will be found proportionably more durable: it is, beſides, very eaſy to conſtruct, and requires leſs labour than moſt other common gates. Twenty-nine years ago I deſigned plans for ornamenſal gates, with ſemi-oval and ſemicircular braces, and had them executed; the plans were ſent to my friends in various diſtant parts of this kingdom, as alſo to Ireland; and I have the pleaſure to obſerve, that they are become almoſt the only ornamental gate in many parts of England. The plans of them I never publiſhed, although they were prepared for engraving fifteen years ago; and I ſhould be as indifferent about my preſent deſign, of a common field gate, if I did not conceive that its publication would materially benefit the public; the introduction of this form being, I conceive, of ſome national importance, as timber has been lately greatly enhanced in price, and is rapidly on the advance.

This gate was deſigned for the approach to a country reſidence; but for common purpoſes, the wicket on one hand, and the ſhort length of rails on the other, may be omitted. I ſhall thank you, if you will have the goodneſs to lay my plan before your reſpectable Society, of which I have, for many years, had the honour to be a member. And ſhould this plan be approved of, I may probably furniſh ſome deſigns for park gates on an improved conſtruction.

I am, Dear Sir,
Your very humble Servant,
CHARLES WAISTELL.

March 22, 1803.
Mr. Charles Taylor.

Reference to the Engraving of Mr. Waiſtell's Gate.

DIMENSIONS.—*(Plate II. Fig. 1.)*

Deſcription and dimenſions of the new conſtructed field-gate.

The heel of the gate to be about	3½	inches ſquare.
The head of ditto - -	2½	by 3 inches.
The top rail or bar - -	3½	by 1½ inches.
The bottom bar - -	3½	by 1¼ inches.
The bar in the middle of the gate	3	by 1¼ inches.
The other bars, and the 4 braces	2½	by 1¼ inches.

Obſervations

Obſervations on its Conſtruction.

Deſcription and dimenſions of the new conſtructed field-gate.

The head and heel of the gate may be of oak, and the bars and braces of fir. Narrow and thick bars, when braced as in this deſign, are ſtronger than broad and thin ones, containing the ſame quantity of timber, and they alſo oppoſe a leſs ſurface to the wind. The two points in the heel of the gate, to which the thimbles are faſtened, may be conſidered as firm or fixed points. From theſe points, viz. 1 and 2, two braces to proceed to 4 and 3, in the middle of the bottom and top bars, and being there ſecured, theſe become fixed points, and from theſe two points, viz. 4 and 3, two braces proceed to 5 and 6, fixing thoſe points. The gate is thus doubly braced, viz. from the top of the heel to the top of the head, by means of the braces 1, 4, and 4, 5; and from the bottom of the heel to the bottom of the head, by means of the braces 2, 3, and 3, 6. On each ſide of the gate are two braces, and thoſe parallel to each other. The brace proceeding from the bottom of the heel of the gate, and that which is parallel to it, as alſo the bottom bar, are all ſtrained in the way of compreſſion, and the brace proceeding from the top of the heel, and the other brace which is parallel to it, and alſo the top bar, are all ſtrained in the way of extenſion. The ſtrains in this gate being none of them tranverſe, but all longitudinal, it would ſupport a vaſt weight at its head without having its form altered. The braces all ſerve the double purpoſe of keeping the gate in its true form, and of ſhortening the bearings of the bars, and ſtrengthening them. Few gates have leſs timber in their braces; and perhaps in no other way can a gate be ſo firmly braced with ſo ſmall a quantity of timber.

At 3, 4, 7, and 8, two braces and a bar of the gate are firmly ſcrewed together by means of iron pins and ſcrew nuts. At the other points, where only one brace croſſes a bar, common gate-nails are uſed.

If, in ſome caſes, a ſtrong top-bar be wanted, to reſiſt the preſſure of heavy cattle, a bar or board, about ſix inches broad, and one inch thick, may be laid with its broad ſide upon the top bar, and fixed thereto by means of the ends of the braces in the middle, and by the heel and head of the gate at the two ends of it. This board will, in this poſition, reſiſt exactly the ſame preſſure as a thick top bar, three inches broad, by four inches deep, although it contain no more than half the timber.

In the ground plan, or horizontal ſection, *Fig.* 7. repreſents a piece of wood, about four inches cube, pinned to the falling poſt, a little below the catch, to ſtop the gate from ſwinging beyond the poſt: another ſtop near the ground may be uſeful.

When gates are hung to open one way only, their heels and heads generally reſt againſt the hanging and falling poſts; but when they are hung according to this deſign, gates may be made about one foot ſhorter for the ſame opening, and conſequently they muſt be lighter, ſtronger, and leſs expenſive.

Of the hanging of Gates.

Hanging of gates.

When the two hooks in the hanging-poſt are placed in the ſame perpendicular line, a gate, like a door, will reſt in any direction in which it may be placed. But, in order that a gate may ſhut itſelf when thrown open, the hooks are not placed exactly perpendicular; the upper hook declining a little towards the falling-poſt, or a few feet beyond it. In whatever direction that hook declines the fartheſt, in the ſame direction will the gate reſt, if unobſtructed, and its head cannot then ſink any lower. Make the head deſcribe half a circle, and it will thus have attained its utmoſt elevation, and will be equally inclined to deſcend either to the right or to the left*.

Particular deſcription of the method of hanging a gate.

The following method of fixing the hooks and thimbles, will, I think, be found to anſwer very well for a gate that is intended to open only one way. Suppoſing the face of the hanging-poſt to be ſet perpendicular, and the upper hook driven in near its inner angle, as is repreſented in the preceding deſign, and that the lower hook muſt be four feet and a half below it; ſuſpend a plumb-line from the upper hook, and at four feet and a half mark the poſt; then at one inch and a half farther from the gateway than this mark, drive in the lower hook; this hook muſt project about half an inch farther from the face of the poſt than the upper hook. In the ſection or ground-plan of the

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* See Chap. II. of Mr. Parker's Eſſay on the Hanging of Gates; and alſo the Agricultural Report for Northumberland, by Meſſrs. Bailey and Culley.

gate,

Particular deſcription of the method of hanging a gate.

gate, the two white circles near the hanging-poſt repreſent the places of the two hooks when brought to the ſame horizontal line; that neareſt the gateway repreſents the place of the upper hook. A line drawn through the middle of theſe two circles, and extended each way, will, on one hand, repreſent the gate's natural line of reſt, and, on the other, the line of its higheſt elevation. A gate thus hung will, when thrown open nearly to the line of its higheſt elevation, return to the falling-poſt with a velocity ſufficient to reſiſt a moderately ſtrong wind. This velocity will be either increaſed or diminiſhed, accordingly as the upper hook declines more or leſs from a poſition perpendicular to the lower hook. In order to adapt the thimbles to theſe hooks;—as the lower hook is one inch and a half farther from the gateway than the upper hook, the lower thimble muſt have its eye an inch and a half farther from the heel of the gate than the eye of the upper thimble, in order that the bars of the gate may be in a horizontal poſition when it is ſhut. And, as the upper hook projects half an inch leſs from the hanging-poſt than the lower hook, the upper thimble ſhould be fixed half an inch nearer the farther ſide of the heel of the gate than the lower thimble, in order that the gate may be in a perpendicular poſition when ſhut. If the thimbles have ſtraps embracing the heel of the gate, and proceeding a few inches along each ſide of the bottom and top bars, and if they are fixed to the heel bars and braces, by means of iron pins and ſcrew nuts, great firmneſs will be given to the gate at thoſe two points, which are thoſe that ſuffer the greateſt ſtrains.

⁂ To this communication are annexed a certificate from Mr. Edward Simpſon of Wooden Croft Lodge, near Barnard Caſtle, in favour of the advantage of theſe gates in ſaving and durability; and alſo a letter from Mr. T. N. Parker, author of a well-known Treatiſe on the hanging of Gates, expreſſing his approbation of the ſame.

VII.

Deſcription of an improved Gun-Lock, by Mr. GEORGE DODD.*

Deſcription of an improved gun lock, by reference to the drawings.

THE figures in *Plate* I. repreſent Mr. Dodd's improved gun-lock with its parts in their ſeveral ſituations and poſitions. The ſhaded drawing, *Fig.* 1. repreſents the exterior parts of the lock; and *Fig.* 2. repreſents its interior. *Fig.* 6. exhibits, in perſpective, the tumbler, the ſear and the ſear ſpring in the poſition of whole cock. The outline plans 3, 4 and 5 ſhew the ſeveral poſitions of the parts, at full cock, half cock, and immediately after the diſcharge. The tumbler A terminates on the lower ſide in a tail, as uſual; upon which the main ſpring acts: but, on the upper or oppoſite ſide, it is formed ſo as to have two notches or bents, one very deep for the half cock, and the other ſhallower for the whole cock, as is ſeen in the figures. The circle *a*, *Fig.* 3, ſuppoſed to be deſcribed by the extremity of the bearing face of the tumbler at whole cock, is larger than that through which the extremity of the bearing face of half cock paſſes, ſee *Fig.* 5. and the center of the ſear B is placed in the outer of theſe two circles, having the under ſide of its noſe faſhioned in the arc of the circle *b b*, deſcribed by the motion of its extremity. The bearing ſurfaces of the bents or notches of half and of whole cock are made to fit this face; or, in other words, they form parts of the ſame circle, when reſpectively at whole or half cock. D is the trigger, ſo formed and placed that, at whole cock it trips or draws out the ſear, with great facility and quickneſs, by the action of an inner ſlope or face lying in the direction of a radius of the circle it deſcribes; (ſee *Fig.* 4.) But when at half cock, *Fig.* 3, its action, by means of an outer ſlope or ſurface (which lies intermediate between radii drawn from the centers of the ſear and of the trigger to the inner point of their contact) is ſo far from diſcharging the motion, that it tends to keep the ſear more ſtrongly in its place. Theſe actions and properties are ſufficiently evident from the figures.

Enumeration of the good qualities of this lock.

The advantages of this lock are, 1. It is fully as ſimple in its conſtruction, or rather more ſo, than the common lock, and is therefore no leſs cheap and eaſy to be cleaned by a common ſoldier or workman: 2. It is diſcharged very ſpeedily, and cannot poſſibly catch or hang at the half cock in the act of diſcharging: 3. The bearing parts at half cock are extremely ſtrong and cannot miſs their hold or be thrown out of taking by any accident. In particular the trigger cannot be made to remove the ſear; becauſe its action at half cock is in the contrary direction. Hence it is much more ſimple and its means of ſafety are nearly as ſecure as any bolt, that is to ſay it is perfectly effectual as far as regards the trigger; though it does not, like ſome of the bolt ſtops, prevent the full cock being made. But on the other hand, as the inventor remarks, its ſecurity in no reſpect diminiſhes its ready uſe. For bolts, excluſive of the additional expence, have the diſadvantage of requiring a previous operation before the piece is fit for ſervice. Few people when alarmed have the preſence of mind to unbolt, but they inſtantly attempt to cock. Diſappointment tends to produce that agitation and confuſion of mind which, at ſuch a juncture, may occaſion the loſs of their lives from opponents who are little diſpoſed to ſhew mercy to an enemy from whom they had no reaſon to expect any.

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* From the Tranſactions of the Society of Arts for 1804.

Certificates of the utility and novelty of this invention, from reſpectable makers, with letters of approbation from the Board of Ordnance, were exhibited to the Society of Arts; and it is probable that this apparatus will obtain the encouragement it appears to deſerve. The Society expreſſed their ſentiments by awarding the ſilver medal with the ſum of ten guineas to the inventor.

VIII.

Inveſtigation of the Properties of the Lines drawn in a Circle by Mr. Boswell *in the Tenth Volume of this Journal. By Mr.* John Gough.

To Mr. NICHOLSON.

SIR, *Middleſhaw, April* 17, 1805.

THE theorems reſpecting the circle, given in your number for March laſt by the ingenious Mr. Boſwell, will undoubtedly prove uſeful to the artiſt and practical philoſopher. On this account

Propoſitions reſpecting the circle's area, &c.

Propoſitions reſpecting the circle's area, &c.

account they ought to be made as correct as poſſible; which has not been as yet done, either by Mr. Boſwell, or by his more ſcientific commentator in your number for the preſent month. This declaration in a manner compels me to undertake the following inveſtigation of the ſubject, in which I ſhall refer to *Fig.* 4. *Plate* XI. of your Journal for April, requeſting the reader to place the letter T at the upper extremity of the vertical diameter, and C at the oppoſite end.

Theorem 1ſt. Let the circle I T F C have unity for its diameter; draw the diameters I F, T C at right angles to each other; biſect the radius T O in W; join I W, and produce it until it meets the circle again in B: theſe things being done, the ſquare upon I B is equal to .8000; which exceeds .7854, or the common expreſſion for the area I T F C by the fraction .0146.

Demonſtration. Put the radius $IO = R = .5$; then $OW = \frac{1}{2}R$; ſince the triangle I O W is right angled at O, by hypotheſis, $IW^2 = R^2 + \frac{1}{4}R^2 = \frac{5R^2}{4}$, *Euc.* 47.1. Now the triangles O I W, B I F are equiangular; becauſe the angle at I is common to both; and the angle F B I is equal to W O I, being right, *Euc.* 31. III; conſequently, as $WI : IO :: IF (= 2IO) : IB$; hence as $WI^2 \left(= \frac{5R^2}{4}\right) : IO^2 (= R^2) ::$ $IF^2 (= 4R^2) : IB^2 \left(= \frac{16R^2}{5}\right)$; but $R^2 = .25$; therefore $IB^2 = \frac{16 \times .25}{5} = 16 \times .05 = .8000$. But $.8000 - .7854 = .0146$. Q. E. D.

Lemma. The area of the circle I T F C is equal to the rectangle under the radius O I, and the ſemi-circumference I T F; for this area is equal to a triangle having I O for its altitude, and the whole circumference I T F C for its baſe. (*Archimides de Circulo Prop.* 1ſt.)

Theorem 2nd. If the right line I B be the ſide of a ſquare, which is equal to the area I T F C; and B G be drawn perpendicular to the diameter I F; the ſegment I G of the diameter I F, cut off by B G, is equal to ¼ of the circumference I T F C or the arc T B F.

Demonſtration. The triangle I B F is right angled at B, 31 *E.* III. and B G is perpendicular to I F by hypotheſis therefore the rectangle $FI, IG = IB^2$, 8. *E.*VI.; but $IB^2 =$ the rectangle under I O and the arc I F F by lemma; conſequently as FI ($= 2IO$) : IO : : arc ITF : right line IG; 14 *E.* VI. hence $IG = \frac{1}{2}$ the arc ITF, $=$ the arc TBF, Q. E. D.

Propoſitions reſpecting the circle's area, &c.

Corollary 1. If I G, a ſegment of I F be equal to the arc T B F; draw G B perpendicular to I F; and let it meet the circle in B; the line I B is the ſide of a ſquare; which is equal to the area I T F C. This is the converſe of the Theorem.

Cor. 2. If any angle at the centre of the circle as I O B, be given in parts of the right angle T O F; and I G be equal to the arc T B F; find a right line N; which ſhall be a fourth proportional to the angles T O F, I O B and the right line I G; this line N is equal to the arc I T B; and a mean proportional betwixt I O, and $\frac{N}{2}$, is the ſide of a ſquare which is equal to the area I O B I; join I B, and from the laſt mentioned ſquare take the trinangle I B O; the remaining magnitude is equal to the circular ſegment I T B.

Cor. 3. The ſquare upon B F is equal to the difference of the areas of the circle and its circumſcribing ſquare.

Problem. If the circumference of a circle, whoſe diameter is unity be denoted by 3.1416; it is required to find a right line which ſhall approximate very nearly to $\frac{1}{4}$ of this number, or .7854.

Conſtruction. Draw I B, as in Theorem 1ſt, and make B G perpendicular to I F; then $IG \times 1 = IB,^2 = .8000$; conſequently $IG = .8000$, which is greater than .7854. Let the reader take *g* in I G, ſo that I *g* may be of the required length; then as 8000 : 7854 : : I G : I *g*; but 8000 is to 7854 nearly as 55 to 54; therefore divide I G into 55 parts and I *g* will be 54 of theſe parts. Draw *g b* perpendicular to I G; join I *b*; and the ſquare upon I *b* will be nearer the truth than that upon I B. If a more complete approximation be required, it may be diſcovered by the method given in the ninth Problem of Emerſon's Arithmetic. Q. E. F.

It is the buſineſs of the practical geometrician to determine the value of theſe propoſitions in practical geometry. The ingenious Mr. Boſwell conſiders the firſt theorem to be of utility; for which reaſon I imagine any improvement in the diſcovery will prove acceptable not only to the inventor, but to ſeveral of your readers who are artiſts and mechanical geometricians.

JOHN GOUGH.

IX.

On the Culture, Properties, and comparative Strength of Hemp, and other Vegetable Fibres, the Growth of the Eaſt Indies. By Dr. WILLIAM ROXBURGH, *of Calcutta**.

DEAR SIR,

YOUR letter of the 16th of May, 1799, I received on my return to Bengal in October laſt; but that from the Society eſtabliſhed in London, for the Encouragement of Arts, &c. of which you are a member, is not to be found.

Ill adviſed experiments for the culture of hemp in India.

I was rather ſurpriſed, on my return to Bengal, to find the directors had ſent out a perſon (Mr. Sinclair) to eſtabliſh the cultivation of hemp, a thing I had begun ſome time before. Even on the coaſt of Coromandel, ten or twelve years ago, I made a moſt ſucceſsful trial, the reſult of which was laid before that government, to be ſent to the Honourable Court of Directors; and again in Bengal, ſince my appointment to the ſtation I now hold. Mr. Sinclair is dead, and the experiment is ſtill carried on in a moſt expenſive manner; whereas it could be continued where it was firſt begun, in the botanic garden, at no expence, and with more proſpect of ſucceſs. Should government continue to be the cultivator, the price will be enormous. Eighty pounds weight is all, I believe, that is yet forthcoming, and coſts from 10,000 to 20,000 rupees. Such experiments throw a complete check in the way of all attempts to introduce new, or improve old, branches of agriculture and commerce.

Beſt method of promoting its cultivation.

A ſmall premium ſhould be offered to the natives, and honorary rewards to Europeans, after the example is ſet on a ſmall and not expenſive ſcale.

Botanic garden at Calcutta.

The botanic garden was at firſt made very large, four times more than was neceſſary for ſuch a garden, the intention of which was merely to make experiments, and to invite the natives to ſee and profit by the examples in huſbandry carried on there.

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* Communicated to the Society of Arts, 1804.

Bow ſtring flax: the ſtrongeſt of vegetable fibres.

A quantity of my bow-ſtring flax was, I underſtand, ſent from the coaſt about two years ago, for the directors. I could wiſh to know what was done with it; for, to me, it ſeems to be the ſtrongeſt vegetable fibre we are acquainted with. I mean to ſend ſome by the January ſhips from hence, through the medium of this government, and wiſh it may fall into your hands, and that its qualities may be properly examined by the Society for the Encouragement of Arts. I am really ſorry that the letter, inviting me to become an honorary correſponding member of that Society, ſhould have been loſt. I beg you will aſſure the Society, that I am ſenſible of the honour they have done me, and ſhall be very happy to have it in my power to contribute my mite to promote the views of that laudable inſtitution.

When any new object, promiſing to become uſeful in the arts or manufactures of our country is diſcovered, and reported to your Society (for example, the bow-ſtring flax,) the Society will probably addreſs the Court of Directors, and recommend the cultivation and importation into England of the commodity itſelf.

Excellent qualities of the *arrow* palm; for its fibres; its wine; and its ſago.

Another object, of more national importance, which I recommended to this government, before I went to the Cape, was the growth of one of the moſt noble of the palms, the *arrow*, mentioned in Marſden's Hiſtory of Sumatra, page 77, and ſaid to yield at an early age (from five to ſeven years,) fibres ready prepared by nature, flexible, ſtrong, and moſt durable, and the moſt convenient for cables and cordage of all kinds, that can be deſired. It alſo yields great abundance of palm wine, which can be converted into ſugar or ardent ſpirits; and when the tree is old, its pith is the baſis of the ſago we ſo much value. I have diſtributed many hundred plants, and have ſtill a great number in the garden*, beſide many thouſand ſeeds in the ground. Drawings, and a deſcription of the moſt valuable tree, were ſent to the directors, under the name of *ſaguerus rumphii*; but as the trees from which they were taken have advanced in ſize and age, a new ſet of drawings, and a new deſcription, of the old one corrected, becomes neceſſary. They will be ſent to the Directors this ſeaſon. The object may be ſuch as your Society wiſh to attend to; and on that account I have ventured to trouble you with the above ſtatement.

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* Feb. 1801. About 100,000 plants have been reared in this botanic garden ſince the date of this letter.

I am, dear Sir,
Your moſt obedient Servant,
W. ROXBURGH.

Calcutta, Dec. 24, 1799.
Robert Wiſſet, Eſq.

DEAR SIR,

Experiments on hemp.

MY letter of the 24th of December, 1799, I am afraid, has not reached you. The experiments on hemp, therein mentioned, have not, I believe, thrown much additional light on the ſubject. My friend, Capt. Burrows, of the Earl Howe, has done more to make our own indigenious ſpecies (the *ſun* of the Bengaleſe) better known, than any other perſon I am acquainted with.

Comparative examination of vegetable fibres.

For theſe laſt twelve months my attention has been much taken up in collecting and comparing the various vegetable fibres of Aſia, &c. uſed for cordage, cloths, and paper. The reſult of theſe I have lately preſented to the ſupreme government, to be ſent to the Hon. the Court of Directors, in reply to the 79th paragraph of their general letter of the 7th of May laſt. This paper may be intereſting to your Society, particularly at this time, when the attention of all good patriots is drawn towards the diſcovery of a ſubſtitute for Ruſſian hemp. This paper, with my former eſſays, contain much information on the ſubjects therein mentioned.

Neceſſity of a ſubſtitute for Ruſſian hemp.

The *ſun* plant is moſt promiſing.

The *ſun* of the Hindoos, which is the prepared fibres of the bark of a well-known Indian plant, the *crotalaria juncea* of Linnæus, ſtill appears to me to be the moſt promiſing ſubſtitute for hemp which has come to our knowledge; I mean, when every circumſtance, relative to its quickneſs of growth, its being already univerſally known and cultivated by all the nations of India, its low price, pliableneſs, ſtrength, durability, &c. &c. are taken into conſideration. All that can be neceſſary for the procuring and tranſporting to England any quantity of this material, is to enſure the cultivator a certain price, and ready market for the commodity; and to have it properly cleaned and packed, to render the freight as low and convenient as poſſible. Cleaned ſamples of this very ſubſtance were ſent by me, ſix or ſeven years ago, to the Directors; ſo that the fault is not mine, if it is not already better known than it ſeems to be.

It can be afforded at a reaſonable rate.

The diſcovery of a ſubſtitute for Ruſſian hemp is certainly an object of the firſt magnitude. If *ſun* is found to be the beſt ſubſtitute yet diſcovered, and coſts in India, ſay, when properly cleaned, ten pounds per ton, and the freight ſixteen pounds, there will ſtill be a conſiderable profit to the merchant, particularly in times of war; for, I believe, it rarely happens that hemp ſells ſo low in London as thirty pounds per ton. Should the ſubject appear of conſequence to you, I beg you will call the attention of the members of your Society to it.

That I may not encroach too much on your time, I will cloſe this letter by referring you to my friend Mr. Boſwell, the late Marine Storekeeper and Naval Paymaſter, for any farther information you may want. I ſend this package by him. Mr. Babb, late a Member of the Board of Trade here, can alſo give you much information.

I am, dear Sir,
Your moſt obedient Servant,
W. Roxburgh.

Calcutta, Feb. 27, 1801.
Robert Wiſſet Eſq.

Obſervations on the Culture, Properties, and comparative Strength of Hemp, Sun, Jute, and other Vegetable Fibres, the Growth of India, communicated by Dr. Roxburgh.

Importance of hemp and its very extenſive uſe in ſociety.

HEMP, conſidered merely as an article of trade, is an object of the firſt importance to the merchant; but, when we reflect on its various uſes, and obſerve that hardly any art can be carried on without its aſſiſtance, or of ſome other ſubſtitute; the objects it embraces are immenſe, and there are few that better deſerve the attention of the philoſopher or intelligent artiſt. Cordage makes the very ſinews and muſcles of a ſhip, and every improvement which can be made in its preparation, either in reſpect to ſtrength, pliableneſs, or durability, or in bringing to light ſubſtitutes equally good or better, particularly where hemp itſelf cannot well be had, muſt be of immenſe ſervice, particularly to the mariner, and to the commerce and the defence of nations.

Its cultivation in our colonies intitled to ſupport and encouragement.

The cultivation of this important plant in our colonies has not only, at all times, met with encouragement from the government, but alſo of late from the Eaſt-India Company in Bengal, where extenſive experiments were begun by the late Mr. Sinclair, and after his death carried on by Thomas Douglas, on the culture of hemp and flax, on account of the Honourable Company. A clear and impartial ſtatement of theſe trials is to be wiſhed for, as it will, no doubt, throw much light on the cultivation of hemp in India, and enable us to proceed with greater proſpects of ſucceſs than ever.

It may be very beneficially produced in India.

My own experiments and inquiries on the ſame ſubject, both on the coaſt of Coromandel and Bengal, have been many, though not extenſive. Their reſult leads me to think, that hemp may be cultivated to great advantage over the interior parts of Bengal and Behar, where the ſeed ſhould be ſown about the beginning of the periodical rains, or earlier, if there have been frequent ſhowers, on elevated ſpots of rich loamy ſoil, ſuch as the Ryots cultivate tobacco, ſun, and paat on, near their habitations. In ſituations of this nature it thrives well, and will be eaſily attended to. At firſt, ſome encouragement will certainly be neceſſary, to induce the Ryots to undertake this new branch of agriculture. For, although the plant is perfectly familiar to every Hindoo, yet the cultivation on an extenſive ſcale, for the fibres of its bark, is perfectly unknown to them. I would therefore ſuggeſt, that they ſhould pay no rent for the ground ſo occupied for a certain period; that ſeed ſhould be given gratis; that they ſhould be enſured a certain price for the hemp; and finally, a reward or premium to the perſon or perſons who produced the greateſt quantity of the beſt hemp within a ſtated period.

The Hindoos cultivate it only for the ſeed.

Seaſon for growing it.

In many parts of Bengal, particularly where the land is ſo low as to remain humid through the dry-weather ſeaſon, hemp thrives luxuriantly during the cold ſeaſon; but the water is then too cold for macerating the plants to the greateſt advantage: one day in June, July, or Auguſt, has more effect in looſening the bark, than eight in December, January, or February; conſequently, the prolonged immerſion injures the quality of the hemp much. The rainy ſeaſon is therefore preferable for the cultivation and maceration, even if the plant grew better during the cold, which is by no means the caſe, particularly on lands elevated above the level of the annual inundation of the low rice-fields. We muſt therefore content ourſelves with one crop in the year; for it is a very falſe notion, and a very prevailing one, that the fertile fields of Aſia produce at leaſt two crops annually; as well might we ſay, that the fertile lands of England yield at leaſt two, becauſe a well-managed garden, near London, or ſome other large city, will produce repeated crops in the year: ſo in India, by great care and induſtry, a ſpot here and there will produce two or more crops. The burning heats of Aſia, while they laſt, are as unfavourable for vegetation, as the froſts of winter in Europe.

One annual crop only can be had.

Beſides hemp and flax, the vegetable kingdom, particularly that natural diviſion called by Linnæus, Columniferæ, abounds in plants which produce materials fit for cloths, paper, and cordage: almoſt every nation or country poſſeſſes ſomething of the kind peculiar to itſelf. To aſcertain what theſe are, as well as to find out new ones, to try their comparative ſtrength, durability, texture, &c. has, at various leiſure hours, employed my attention for many years paſt. Drawings and deſcriptions of many of them have been already laid before the honourable Court of Directors. There are, however, ſome others which remain to be brought under view and compared with the kinds we are beſt acquainted with, which I have attempted in the following experiments. Many other ſorts are alſo mentioned by various authors and travellers, of which I know nothing more than the names. Two of theſe are mentioned by Marſden, at pages 75 and 76 of his hiſtory of Sumatra. Others are peculiar to Pegu, &c. &c. Theſe I muſt omit for the preſent, and confine myſelf to ſuch as I am more intimately acquainted with. The better to enable me to proceed in this inquiry, I have cultivated, in the Botanic Garden at Calcutta, many of the plants themſelves which produce the materials hereafter mentioned; prepared their fibres in general by maceration, &c. as with hemp and flax in Europe. And, to compare their ſtrength (plain, tanned, and tarred,) had then made into cords, compoſed of three ſimple yarns, as nearly of the ſame ſize and hardneſs as a

Other vegetables particularly the columniferæ, fit for the uſes of hemp.

Deſcriptive outline of the author's experiments on various plants for cordage, &c.

Preparation of the fibres.

Cordage manufactured,

Hindoo rope-maker could make them; but, in ſpite of my utmoſt care, they were always too hard twiſted to be of the greateſt poſſible ſtrength [*]. Of each ſort there were ſix, when there was a ſufficient quantity of the fibres. Three of them were about the ſize of a log-line, and three a ſize larger than a whip-cord; one of each ſize and ſort was kept white: the average number of pounds which broke them, (for repeated trials were made, and always with lengths of exactly four feet), will be found in the firſt and ſecond columns on the right. One of each was tanned [†] with the aſtringent fruit called *gaub* by the Bengaleſe, (Embryopteres Glutinifera, Roxb. Coromandel Plants, Vol. I. No. 70.) Their ſtrength is repreſented in the third and fourth columns of the following table. And, laſtly, one of each ſort was tarred: their reſpective ſtrengths will be found in the fifth and ſixth columns.

and tried in the white, and alſo tanned and tarred.

Tanning ſtrengthens animal fibres, but quære vegetable?

We know the tanning principle ſtrengthens the fibres of leather, (animal fibres) but are not ſo clear that its operation on vegetable matter is uniformly the ſame. The attention beſtowed to aſcertain this point in theſe experiments will, at leaſt throw ſome light on the ſubject; and may induce others, better qualified, to extend the inquiry (here in India), where tanning materials abound.

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[*] The experiments of Reaumur, Sir Charles Knowles, and Du Hamel, uniformly prove, that when hemp-rope is twiſted to the uſual hardneſs, which is that which brings them to two-thirds of the length of their reſpective yarns, their ſtrength is leſſened by nearly one-fourth, when compared with ropes conſiſting of the ſame number of the ſame yarns twiſted up to only three-fourths of their length. There will be no difficulty in accounting for this difference, if we conſider that a ſkein of fibres may be twiſted ſo very hard, as to break with any attempt to twiſt it harder. In this ſtate the fibres are already ſtrained to the utmoſt, and cannot ſupport any weight or additional ſtrain.

[†] The idea of tanning cordage is far from being new; for the fiſhermen of Aſia, as well as of Europe, not only tan their nets and lines, but alſo their ſails, to give them additional ſtrength and durability. The ſame proceſs might be productive of the ſame effects, if employed on cordage made of the materials (No. 2, 5, 6, 7, 8, and 15), ſpecified in theſe experiments, which induced me to recommend its being tried with ſun cordage, in my letter to the Board of Trade, in Auguſt, 1797.

Another

Whether tan will preſerve wet rope?

Tar does not ſtrengthen cordage; but the contrary.

Tar not produced in tropica Aſia.

Another point of the utmoſt importance to be aſcertained is whether tanned ropes will be preſerved by the tan, with which they are impregnated, when ſtowed away wet; as tarred ropes are preſerved by the tar when ſo circumſtanced. It is nevertheleſs a well-known fact, that tarred cordage, when new, is weaker than white; and that the difference increaſes by keeping. Tar can therefore only be employed to preſerve cordage, and not to ſtrengthen it; ſo that if tan will add ſtrength, or even not weaken vegetable fibres, and at the ſame time preſerve them; of how great advantage to the nation would the diſcovery of a convenient practicable proceſs be. For all cordage, expoſed to be alternately very wet or dry, requires to be impregnated with a preſervative. And, to conclude this long note, I beg leave to obſerve, that tar is not the produce of the warmer parts of Aſia. Are we, therefore, to conclude, that no material, the produce of theſe parts, can be applied for the ſame end? Let us not entertain any ſuch idea. Nature is abundantly kind, and furniſhes every country and climate with what is moſt proper for the uſe of its inhabitants.

Statement of experiments on the cordage.

The annexed ſtatement of the experiments made on the ſubſtances there ſpecified, can only be deemed an attempt towards aſcertaining their relative ſtrength; and though they are the average reſult of ſeveral trials made on the ſtrength of each cord, plain, tanned, and tarred, yet I muſt acknowledge they cannot be deemed any thing more than a firſt eſſay, chiefly owing to the lines being in general ill laid, ſome more and ſome leſs twiſted, and by no means to be compared with thoſe of Europe. For in ſome inſtances, I found a ſmall one ſuſtain a greater weight than a much larger, made exactly of the ſame materials. It is therefore my intention to repeat them on a larger ſcale, and, if poſſible, with better made lines; for every thing depends on their being exactly of the ſame ſize and degree of twiſt.

COMPARATIVE STATEMENT OF THE EXPERIMENTS.

Names of the Plants, and brief Remarks on the various Materials employed in theſe Experiments.	Average weight at which each ſort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
No. 1. Engliſh Hemp: a piece of new tiller-rope, opened out and made into cords, like the reſt, - - - - - - - - - - - - -	105	65	. .	. .	. .	. .
2. Hemp (Cannabis), the growth of this ſeaſon, from the Company's Hemp Farm, near Calcutta, - - - - - - - - - - - - -	74	50	139	60	45	46
3. Coir: the fibres of the huſk of the Cocoa-nut; much uſed for cables and cordage over Aſia, - - - - - - - - - - - -	87	60	. .	. .	. .	.
4. Ejoo (Saguerus Rumphii): the black horſe-hair-like fibres, which grow round the trunk of this ſpecies of Sago Palm, - - - - - - - -	96	79	. .	. .	. .	. .
5. Robinia Cannabina, Danſha of the Bengaleſe: the fibres of its bark, prepared by maceration from the plants that had nearly ripened their ſeed, they are then of a duſky grey colour, and harſh nature, - - - - -	88	64	101	55	84	39
6. The fibres of the bark of No. 5, prepared by maceration, from plants coming into bloſſom; at which time they are beautifully white, ſoft, and gloſſy, -	46	20	61	35	48	30
* 7. Crotolaria Juncea, Sun of the Bengaleſe: the fibres of its bark, and univerſally known over India, - - - - - - - - - - -	68	47	69	55	60	37
8. Corchorus Olitorius, Bunghi-paat of the Bengaleſe: the fibres of its bark, called Jute by the ſame people, - - - - - - - - - -	68	47	69	59	61	36
9. Corchorus Capſularis, Ghee-nalta-paat of the Bengaleſe: the fibres of its bark they call Nalta Jute, - - - - - - - - - - -	67	47	. .	. .	. .	. .
10. Flax (Linum Uſitatiſſimum): the growth of the Company's Hemp Farm, near Calcutta - - - - - - - - - - - - - -	39	37	. .	. .	. .	. .

* A cord, a very little thicker than a log-line, made of ſun ſail twine, broke with 148 pounds when dry; but, on being ſoaked in cold water for 24 hours, it bore, while wet, 222 pounds. This difference requires to be farther inquired into.

Names of the Plants, and brief Remarks on the various Materials employed in these Experiments.	Average weight at which each ſort of cord broke.					
	White.		Tanned.		Tarred.	
	Large.	Small.	Large.	Small.	Large.	Small.
11. Agava Americana: the fibres of its leaves. They are of a coarſe harſh nature, and white,	110	71	79	73	78	38
12. Aletres Nervoſus—In Sanſcrit, Murva; Murga of the Bengaleſe: the fibres of its leaves made into theſe cords, after having been kept above one year,	120	52	73	42	48	43
13. Theobroma Auguſta, Linn.—Abroma Auguſta, Hort. Kew.—Abroma Wheelerii, Kæn.—Woollet-comal of the Bengaleſe: the fibres of its bark prepared by maceration, &c. like hemp,	74	61	58	51	44	29
14. Theobrama Guazuma, Baſtard Cedav: the fibres of the bark of ſome ſtraight, luxuriant, young plants,	52	48	..	47	45	30
15. Hibiſcus Tiliaceus, Bola of the Bengaleſe: the fibres of its bark, and employed for cordage by the inhabitants of the South-Sea Iſlands, &c.	41	..	62	39	61	..
16. Hibiſcus Manihot, a tall white-flowered variety: the fibres of its bark, which are beautifully white, gloſſy, and ſoft,	61	..	..	..	..	..
17. Hibiſcus Mutabilis: the fibres of its bark, and they are of a harſh nature and ill coloured,	..	45	53	46	..	..
18. Hibiſcus, a new ſpecies, from the Cape of Good Hope, ſaid to be a native of Caffraria, where the fibres of its bark are ſpun,	..	22	..	..	..	..
19. Bauhinia, a large ſcandent ſpecies: the fibres of its bark, cleaned without maceration, and uſed to make ropes, &c. of by the people of Napaul, where the plant is common,	69	39	..	..	..	..
20. The ſame as No. 19, only maceration was uſed to help to take the bark off the twigs with more care,	56	41	..	..	..	..
21. Sterculia Villoſa: the fibres of its bark. Cords are made of them by the natives of the eaſtern frontier of Bengal, to bind wild Elephants, when firſt taken,	53	..	..	..	..	..

The cords, when the trials were made, had been kept about ſix months after they were prepared, chiefly with the view of allowing the effects of the tan and tar to take place. The reſult of theſe experiments ſhow, that tan has in general added ſtrength, while tar has had a contrary effect; and in no inſtance is this more clearly evinced, that in the common hemp (Cannabis) cultivated in Bengal.

Ejoo and Coir.

To Ejoo and Coir, neither tan nor tar ſeem applicable; and in ſeveral of the other experiments, I had not a ſufficient quantity of the materials to make the neceſſary number of cords, viz. ſix of each ſort, to try with tan and tar, as well as in their natural ſtate. At ſome future period, I hope to be more fortunate in procuring larger ſupplies of the materials, and alſo to add ſome other other ſorts, ſuch as the—

Bow-ſtring fibre.

Rajemahl bow-ſtring fibre, the produce of a new ſpecies of Aſclepias, diſcovered by William Roxburgh, junior, amongſt the Rajemahl Hills;

New Zealand hemp.

New Zealand Hemp;

Hiſbiſcus Cannabinus, and ſome other of the ſame natural order (Columniferæ); (for in general their barks abound with ſtrong fibres; witneſs the foregoing table, where ſix of them are to be found);

The leaves of a new ſpecies of Andropogon, &c. &c.

APPENDIX,

Containing Remarks on ſome of the Plants mentioned in the foregoing Table.

Remarks on hemp.

No. 2. Hemp, or Cannabis Sativa.—Banga, in Sanſcrit; Bunga, Bungh, or Bung, of the Hindoos; Bang, of the Perſians; Kinnub, of the Arabians, is no doubt our own famous plant, now ſo common and uſeful in Europe. I have at different times examined various figures and deſcriptions, as well as the plants reared from Europe ſeed, comparing them with our Indian plant through its various ſtages, and can diſcover no difference whatever, not even to ſound a variety on. Perhaps few vegetables, ſo widely diffuſed over almoſt every part of the known world, and under the immediate management of man, have undergone leſs change. It is perfectly familiar to all the nations of India, I may ſay of all the warmer

It is well known in India; but not for its fibres;

parts of Aſia; yet I cannot diſcover that the fibres of the bark have ever been employed for any purpoſe. It is cultivated in ſmall quantities every where, on account of its narcotic qualities.—The leaves of the male plant, and flowers of the female, are the parts in moſt general uſe.

nor in China, &c.

I have repeatedly applied for the ſeeds of all plants reared in China, and other countries to the Eaſtward of the Bay of Bengal, as well as to almoſt every other part of India we have any communication with, for an account of the plants employed to ſupply materials for clothes and cordage, and for their ſeeds; but could never learn that Cannabis was one of them; nor were its ſeeds ever ſent to me as ſuch.

Ejoo; very ſtrong, but no elaſtic.

No. 4. The great ſtrength of this ſubſtance makes it a very deſirable object. For a deſcription and drawing of the tree I refer to thoſe* which accompanied my letter to the Moſt Noble the Governor-General in Council, under date the 2d of January, 1800. The fibres employed in theſe experiments were taken from trees growing in the Botanic Garden at Calcutta, where they thrive well. I could obſerve, during the trials made in breaking the cords of this ſubſtance, that they were not ſo elaſtic as thoſe of Coir, which will probably render it leſs fit for cables, but better for many other uſes. Coir is certainly the very beſt material yet known for cables, on account of its great *elaſticity* and ſtrength.

Other plants:

Nos. 5, 6, 7, 8, and 9. Theſe four plants have already been figured and deſcribed by me, in a memoir ſent through the Governor-General in Council, to the Hon. the Court of Directors, in December, 1795. Since writing that paper, I have learned, that *ſun* (Crotolaria Juncea) is almoſt univerſally employed, over the warmer parts of Aſia, for cordage. On the Malabar coaſt, I find it is generally named by the gentlemen at Bombay after the province where reared. It is uſed in that place for lacing their cotton bales, on account of its great ſtrength. Samples of three ſorts, viz. Malwan, Rajapore, and Salſette, were ſent to me, from them, by Dr. William Hunter; and am induced to think, little or no maceration is

The *ſun* or crotolaria juncea.

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* A former ſet, No. 1179, ſent in to Government on the 23d of November, 1797, were not ſo correct as could be wiſhed, on account of their having been taken from young trees, juſt coming into bloſſom the firſt time.

employed in taking the bark from the ſtalks, or in cleaning the fibres, which may add to its ſtrength: for certainly maceration, particularly if long continued, muſt weaken freſh vegetable fibres conſiderably. The ſame gentleman ſent me ſeeds of the Salſette ſort: they have produced plants now in bloſſom, and from them have aſcertained the identity of this ſpecies.

In ſome parts of Bengal, a moſt luxuriant variety is cultivated immediately after the rains, which often grows to the height of twelve or fourteen feet; while the common ſort is generally reared in Bengal during the early part of the wet ſeaſon, and grows to only about half the height of the former.

Danſha of the Bengaleſe;

I muſt farther obſerve, that the fibres of No. 5, poſſeſs great ſtrength, and it ſeems to me to be one of the moſt fit of any of our Indian productions for cables and cordage. The plant grows generally to the height of from ſix to ten feet, the fibres long, but harſher than thoſe of hemp, if not cut at an early period. It is very generally cultivated about Calcutta during the rains. An acre yields of the half-cleaned ſubſtance (the ſtate in which the natives carry it to market), about 600 lbs. weight, and ſells for about a rupee and a quarter per maund of 80 lbs.

beautiful but weak fibres.

No. 6. By cutting No. 5, the laſt-mentioned plant, when beginning to bloſſom, we have the moſt beautiful ſhining white fibres that can well be conceived, but (by my experiments) greatly weaker that when the ſeed is ſuffered to be nearly ripe before the plants are cut.

Flax cultivated in India for its ſeeds only.

No. 10. Flax, the plant, is very generally cultivated during the cold ſeaſon, over the interior parts of Bengal and Behar, merely for the ſeeds, from which oil is obtained. The flax itſelf the Hindoos ſet no value on; for, after they have gathered the ſeed, they throw away the ſtalks as uſeleſs, having no knowledge of the fibres which their bark yields. Samples of the flax have repeatedly been procured by the Board of Trade, and ſent to England to the Hon. Court of Directors; ſo that it is from England we may expect to learn its properties. If the flax has been found good, large quantities may be reared at a ſmall expenſe; as the ſeed alone, which the crop yields, muſt be more than equal to the charges, to render it profitable to the farmer.

No. 11. This Agava is of ſlow growth; on that account I doubt if ever it can be advantageouſly cultivated; but, where found wild in plenty, it may be manufactured at a trifling expenſe. Its great ſtrength renders it an object worthy of attention. The fibres are coarſe, conſequently rope made of them harſh to the feel. Agava Americana; of no great promiſe.

No. 12. Drawings, and a deſcription of the plant Aletris Nervoſus Roxb. the method of extracting the fibres, with a quantity of the ſubſtance itſelf, were ſent to the Hon. the Court of Directors, through the Madras Government, above ten years paſt. I alſo gave a large quantity to Mr. Bebb, when he left Bengal in January, 1800, to take to England with him for trial there. The plant grows fully as well, and is as common as on the Coromandel coaſt. There has lately been about a biggah (third of an acre) planted out with it in the Botanic garden, the better to determine the expenſe, and the annual produce of any given quantity of ground. Aletris nervoſus.

No. 13. This plant, a native of various parts of India, New South Wales, Philippine Iſlands, &c. has been long known to botaniſts; yet I cannot, with all the attention that I have been able to beſtow on the ſubject, find that the fibres, ſo abundantly interwoven through its bark, have ever been uſed or even taken notice of by any other perſon; ſo that I think we may look upon it as a new diſcovery, deſerving of more than common attention, on account of the beauty, fineneſs, and ſtrength of theſe fibres. Abroma auguſta, very promiſing.

It is perennial, grows luxuriantly in the Botanic garden, and has been cut down twice within theſe ſix or ſeven months; ſo that I think it will, at leaſt, annually afford two or three crops of ſhoots fit for yielding this ſubſtance. My experience does not yet enable me to ſtate how much may be the yearly produce of an acre, but can venture to prognoſticate as large a produce as can be obtained from an acre of Dancha, Jute, Sun, Hemp or Flax. Its growth.

To render this bark ſeparable from the half-ligneous ſhoots it covers, to ſoften its external lamina, or epidermes, and the parenchymatous ſubſtance which firmly connects the fibres in their natural ſtate, maceration in ſtagnant water for from four to eight days, during the warmer parts of the year, anſwers well whilſt three times as many days are ſcarce ſufficient during the cold ſeaſon; indeed, the proceſs is ſcarcely practicable then; beſides, the fibres are greatly weakened by the length of the maceration. Treatment to obtain the fibre.

Immediately on being taken out of the water, and while wet, the ſhoots are ſingly taken in the hand, rubbed with ſome coarſe materials, ſuch as a little dry graſs or gunny, to remove the exterior pulpy lamina or epidermis of the bark, which is deſtitute of fibres. This part of the proceſs is eaſily effected; and when done, the clean ſhoots are to be made up into ſmall bundles, and placed under weights, or ſome other equal preſſure, to keep them firm at the middle and top, either under the ſurface of the waters, or out of it; the fibrous bark is then ſeparated with the fingers from a ſmall portion at the end of the ligneous ſhoot or ſtalk, which the operator takes hold of, and draws out one by one; when theſe are removed, the pulp, or parenchyma, which fills the interſtices between the fibres, and connects them together, forming in the living plant that part of the bark which may be called its inner lamina, or cellular tiſſue, is immediately waſhed out in cold water, and the clean fibres ſpread out in the ſun to dry. Such was the ſimple proceſs by which this ſubſtance (which may very properly be called Indian Hemp or Flax) was prepared.

I have now under cultivation about the third of an acre (a biggah) of ground in the Botanic garden with this plant; the reſult ſhall be carefully noted from time to time. It might have been prudent to have withheld this account until that time; but the ſtrong deſire of making known a diſcovery, which may in a ſhort time become beneficial to the public, induces me to be thus precipitate.

For a farther account of this plant, I beg leave to refer to my drawing and deſcription thereof, ſent to the Hon. the Court of Directors ſome years ago, and numbered 415.

W. ROXBURGH.

Botanic Garden, near Calcutta,
Jan. 31, 1801.

Additional Experiments on the Strength of Sun, (No. VII.)

Additional experiments on the ſtrength of *ſun.*

Some tanned ſail-twine, made of this ſubſtance four years ago, for the inſpection of the Marine Board, was made into a cord of three ſtrands; each of the ſtrands compoſed of four threads of the ſail-twine.

Some more of the ſame ſail-twine, tanned twelve months ago, was made into a ſimilar cord, and another was made of the white unprepared twine.

The firſt, which had been tanned four years, broke with 110 lbs. when dry, and with 130 lbs. after having been ſteeped in water 24 hours.

The ſecond, which had been tanned one year, broke with 123 lbs. when dry, and with 140 lbs. after ſteeping 24 hours.

The third, or plain white, broke with 148 lbs. when dry, and with 222 lbs. after ſteeping 24 hours.

X.

Deſcription of the Bavarian Method of evaporating Saline Waters. By M. Bonnard.

Bavarian method of evaporating ſalt waters.

THIS new method, practiſed in Bavaria, has been introduced into the ſalt work of Moyenvie, by M. Cleiſs, inſpector of the ſalt-works of Bavaria.

The pans are compoſed of ſquare plates of caſt iron, of 4 millimetres in thickneſs, and 4.76 centimetres on each ſide. Theſe plates are joined by their edges, which are turned downwards, and conſequently without the pan: they are ſolidly united by a piece in the form a ſquare gutter which receives the edges, and is ſecured by a great number of ſcrews.

An evaporating houſe is compoſed of ſix pans, of this conſtruction, diſpoſed in two rows; but theſe pans have different uſes, which require a particular arrangement.

That in the middle of the back row is the ſmalleſt; it has no particular fire-place, but it is heated by the junction of the chimnies from the other fire-places. The ſalt-water depoſits its impurities in it; it is called the ſmall pan.

From the ſmall pan the ſalt water paſſes into the graduating pan, which is lower than the firſt, and placed in the middle of the front row; it is there kept in a ſtate of conſtant ebullition: the water is concentrated in it to 20 degrees, and depoſits a part of its ſulphated lime.

From the graduating pan the ſalt water paſſes into the preparing pans, which are lower than it, and ſituated at the two extremities of the back row: there it is alſo kept conſtantly boiling, Bavarian method of evaporating ſalt waters.

boiling, it is completely concentrated, and depoſits all its ſulphate of lime; it is then paſſed into the cryſtallizing pans, ſtill lower than thoſe of preparation, and placed at the two extremities of the front row: there the water ſcarcely boils, and the ſalt cryſtallizes.

Each pan, with the exception of the ſmall pan, has a particular fire-place, the chimnies of which paſs round the ſides of the pan: they unite under the ſmall pan, by which means there is little heat loſt.

Theſe pans are placed two and two in chambers of wood, the joinings of which are well ſecured, which cloſe them hermetically: theſe chambers are low, and their ceilings are perforated in the middle with holes terminating in a tube, by means of which the aqueous vapour is diſengaged with rapidity. The chambers for the preparing and cryſtallizing pans have their ceiling pyramidal, or in the form of a reverſe hopper, while that for the ſmall pan and the graduating pan is horizontal.

The ſaline waters are paſſed ſucceſſively into theſe four kinds of pans; the workmen penetrate into the chambers, in the midſt of the vapour, to open the communications. This operation is performed every ſix hours, and the water in each pan is reſtored to the level at which it ſtood ſix hours before. Every three hours the ſalt in the cryſtallizing pans is collected, it is brought with ſcoops to elevations on the front edge of the cryſtallizing pans, where it drains; it is afterwards carried into drying rooms, which ſurround the outſide of the chambers: theſe are ſpaces covered with iron plates; they are warmed by heat-tubes leading from the fire-places.

Every eight days they take away the ſulphate of lime, throw out the mother-waters, and break the ſhell, that is to ſay, the incruſtations of ſalt which adhere to the bottoms of the pans: every twenty-four days the work is entirely ſtopped to repair the pans; an operation which is performed by the workmen themſelves.

Economy of fuel.

It has been found that this method of evaporation affords a ſaving of more than one-third of the fuel.

An improvement has lately been made in this proceſs at Dieuſe: the ſmall pan has been ſuppreſſed, and the drying rooms have been replaced by auxiliary pans, in which a coarſe ſalt is made.

The heated drying rooms are uſeleſs when the humidity of the ſalt ariſes from the muriate of lime it contains.

Bavarian method of evaporating ſalt waters.

Explanation of the Plates III. and IV.

Fig. 1. Plan of the pans.
No. 1. Small pan.
No. 2. Graduating pan.
No. 3. Preparing pan.
No. 4. Cryſtallizing pan.

The diſpoſition of the plates of iron which compoſe theſe pans, is ſhewn in No. 2.

a, a. Elevation on which the ſalt is placed to drain, as it is taken from the cryſtallizing pans.

b, b, b, Wooden partitions which ſeparate the chambers.

c, c, c, A raiſed wooden ledge which ſurrounds the pans.

Fig. 2. Section of the evaporating chamber which contains the pans 1 and 2.

d, d, d. Heat tubes which give heat to the ſmall pan, and contribute to heat the others.

e, e, e, Fire place for the pans.

i, i, i. Pillars of caſt iron under the gratings *g, g, g,* which ſupport the bottom of the pans.

h. Wooden chamber which contains the two pans.

k. Opening by which the vapours eſcape.

Fig. 3. Section of the evaporating chamber which contains the pans 3 and 4.

a. Elevation on which the ſalt from the cryſtallizing pans is placed to drain.

The other letters indicate the ſame parts as in the preceding figures.

Fig. 4. Method in which the plates of iron are joined to form the pans.

a. The iron plate.

b. The iron gutter which receives the edges of the plates, and is ſtrongly faſtened with ſcrews.

i, i. Pillars of caſt iron which ſupport the bottom of the pan.

XI.

Letter of Inquiry reſpecting the late Dr. IRVINE's *fundamental Experiment on the relative Capacities for Heat of Ice and Water. With an Anſwer by Mr.* IRVINE.

Requeſt that Mr. Irvine would deſcribe his father's experiment on the capacities of ice and water.

A CORRESPONDENT would be much indebted to Mr. Nicholſon if he would convey by publication in his Journal, or otherwiſe, the following requeſt to Mr. Irvine. That gentleman in his paper in the Journal of laſt month * mentions his having in his poſſeſſion the experiments of Dr. Irvine on the capacities of bodies. It has always been a wiſh with theſe chemiſts who have attended particularly to this ſubject to know in what manner Dr. Irvine made the experiment to aſcertain the comparative capacities of ice and water. If either of the bodies during the experiment change its form, if the ice were melted, or the water congealed, it becomes leſs deciſive, as it may be objected by thoſe who maintain the opinion that latent caloric exiſts in bodies either in whole or in part in a ſtate of chemical combination, that the reſult might ariſe from ſuch a combination, and not from a change of capacity. But if the experiment were made in ſuch a manner that no change of form took place, which though difficult is poſſible, Dr. Irvine's theory, which is ſo much ſuperior to the other, is unequivocally eſtabliſhed. It would be conferring a favour on the chemical world, if Mr. Irvine would take the trouble of ſaying in what manner the experiment was conducted, providing ſuch a notice would not interfere with the intention he has announced of giving a more full account of his father's inveſtigations, an account which would be eagerly received by chemiſts.

Change of form would influence the reſult.

September 24th, 1803.

Reply by Mr. IRVINE.

The experiments may be made without being liable to the objections before ſtated;

WITH regard to the enquiry of Mr. Nicholſon's correſpondent, it would give me pleaſure to inform him of any circumſtance within the ſphere of my own knowledge that ſhould tend to add to the illuſtration or proof of my father's theory.

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* Sept. 1803. See our Addreſs to Correſpondents of laſt month.

There does not ſeem to me any difficulty in explaining ſatisfactorily how experiments on the capacities of ice and water may be conducted without being expoſed to the objections above ſtated; at the ſame time, though theſe experiments may be concluſive as far as they go, I have not been accuſtomed to conſider them as altogether ſo deciſive of all arguments upon this ſubject.

—but may not be unexceptionable.

In a general way then the experiments of Dr. Irvine were conducted in the following manner. The capacity of water being taken at unity, pains were uſed to aſcertain the capacities of mercury, river ſand, pounded glaſs, and iron filings, with reſpect to water, and conſequently to each other in the ordinary manner. It is at preſent of no importance what proportional quantities of the materials were employed. This being done, the capacity of one of theſe ſubſtances was experimentally compared with that of pounded ice or ſnow.— Every precaution was uſed to enſure ſucceſs. The weight and capacity of the veſſel was determined, and the colder and hotter body alternately added to the other. The temperature of the air was always below 32, as was that of all the materials and of the veſſel. No water could therefore be formed. In his firſt experiments I believe Dr. Irvine uſed mercury, but afterwards I know that he preferred iron filings and ſand. For example, if the veſſel and room were at 11°, let half an ounce of powdered ice from diſtilled water, at temperature 30° be poured on four ounces of iron filings at 11°, let the temperature of the mixture be noted after ſtirring, the due allowances made for the heat gained by the veſſel and the air, and a proper calculation made. Let this experiment be reverſed by cooling the ice to 11°, and pouring the iron filings at 30° upon it, and let a calculation from this be compared with the former and corrected by it. Finally, let theſe experiments be compared with others where different quantities of materials are uſed, and of different temperatures, and you have a view of the method employed for determining this point by Dr. Irvine, which does not appear to be objectionable upon other grounds than all experiments for aſcertaining capacities are, none of which have any pretenſions to perfect accuracy. Dr. Irvine was far from being ſatisfied that his experiments were mathematically preciſe. But he uniformly found the capacity of ice to be leſs than that of water, and that in a greater ratio than is generally allowed.

Account of Dr. Irvine's method of experimenting.

The capacities of mercury, ſand, pounded glaſs and iron filings were taken with reſpect to water.

One of theſe was then examined againſt pounded ice at a temperature of the place always under 32°.

Example.

The common notion, of ice and water being directly applied to each other, unfounded and impracticable.

With reſpect to the common notion that experiments on the capacity of ice are made by mixing it with water, it is altogether falſe in the caſe of Dr. Irvine. No doubt this may be done theoretically. It is eaſy to ſay that ice loſes ſo many degrees which heat water only ſo many. But then no ice muſt be melted, which if not impoſſible is extremely difficult, or what is equally ſo, the quantity melted muſt be found, and an allowance made for the latent heat, which is itſelf not preciſely aſcertained, at leaſt not with ſufficient accuracy for this purpoſe. I have only to add that this gentleman's enquiries would have been earlier noticed if I had been informed of them, which I was only a few days ago.

W. IRVINE.

April, 1805.

XII.

Short Account of ſome of the moſt remarkable Facts and Obſervations in an Aeroſtatic Voyage, made from Peterſburgh, by Meſſrs. ROBERTSON *and* SACHAROFF, *under the Sanction of the Imperial Academy.* W. N.

Aeroſtatic voyage from Peterſburgh.

THE notice of an aeroſtatic voyage performed by Meſſrs. Robertſon and Sacharoff from Peterſburgh, under the direction of the Imperial Academy of Sciences, June 30, 1804, of which an abridged account was given to the National Inſtitute of France*, is very intereſting, from the ſcientific views and conduct of the managers Lowitz and Robertſon, as well as for its other particulars.

Aſcent of the balloon with two obſervers.

Their balloon was a ſphere of 30 feet diameter, and roſe at a quarter paſt ſeven in the evening, with the aſcenſional force of one pound, the whole weight of ſolid matter (including 110 pounds of ſand for ballaſt) being 622 pounds. When they were over the river Neva, at the elevation of 108 toiſes or 620 feet, they deſcended a little by the condenſation of the gas; but roſe again by throwing out a little of the ballaſt. The uſual phenomenon of a ſlow rotation of the balloon preſented itſelf, which doubtleſs aroſe from the unequal action of the air againſt an irregular ſurface, as we ſee in moſt other bodies riſing or falling in a fluid.

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* Inſerted in the Annales de Chimie, LII. 121.

But

Their direction obſerved by a floating log; —of paper, &c.

But one of the moſt ſtriking circumſtances attending this voyage is the rational means which theſe philoſopers made uſe of to determine, and in a certain degree to regulate their courſe. They made uſe of two inſtruments, a log and a teleſcope. The log conſiſted of two ſheets of very thin paper, blacked, and fixed at right angles to each other by a very light croſs of wood. This was ſuſpended from their car by a ſtring of ſixty feet in length, and affording a different reſiſtance to the air from that of the balloon itſelf,† it was found to draw the ſtring out of the perpendicular direction, or as the narrators ſay, *to follow the balloon:* ſo that by its poſition determined by compaſs they could aſcertain what direction they were purſuing. It alſo ſhewed by its relative riſe and fall whether the apparatus was deſcending or aſcending, before their barometer had indicated the ſlighteſt change.

Perpendicular teleſcope by which their courſe and velocity were ſeen on the ground.

Inveſtigation of the degree of accuracy of this method.

Their teleſcope was applied to ſhew the direction of their courſe, and muſt have been much leſs ſubject to doubt than their log. Its application would be univerſal and perfect, if the earth could be ſeen at all times from the elevated regions of the air. It was directed perpendicularly downwards by means of a plumb-line, and having a conſiderable magnifying power, the objects upon the ſurface of the earth were ſeen moving acroſs its field of view, and their direction would moſt clearly aſcertain that of the car itſelf, and alſo its velocity. If, for example, the magnifying power were fifty times and the field of view one degree, the viſible ſpace included in that field from an elevation of two miles would be about 180 feet in diameter, in which objects of ſix or ſeven inches broad might be very well diſtinguiſhed through a favourable atmoſphere; and at ſo low a velocity as one mile an hour the whole field of view would be paſſed over in about twelve ſeconds. Hence we ſee that the method affords a conſiderable degree of accuracy, and will not in general require any great power of magnifying or delicacy of obſervation. The computation would be founded on the following problem, which will not preſent any difficulty to thoſe who are acquainted with theſe ſubjects, if the phyſical allowances for temperature in barometrical admeaſurements be admitted to be correct enough for this purpoſe.

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† Or rather, perhaps, becauſe not exactly in the ſame current of the atmoſphere. N.

Problem for computing the velocity of a balloon by obſ. through a teleſcope.

Given the temperature on the earth and in the car, the height of the barometrical column, and the time employed in the apparent tranſit of an object on the earth through a given angle or field of view; to find the velocity of the obſerver.

Tabulated reſults for practice.

For practice it would perhaps be ſufficiently exact and convenient to compute a ſmall table, in which, neglecting the temperature, the velocity in miles per hour might be had by inſpection, when the height of the mercury, the time of tranſit, and the magnifying power were known.

The obſerved courſe, &c.

A pigeon let go.

The dip of the magnetic needle is leſs at great heights in the air,

This may afford means of ſhewing the heights, &c.

The aeronauts having noticed by their inſtruments what were the direction of the currents of air at different heights, found themſelves in one which carried them directly towards the Baltic. They therefore deſcended till they ſaw by the barometer that they had returned to a current which carried them inland; and afterwards again roſe much higher, and ſaw with great preciſion by their teleſcope the inſtant of their quitting the gulph. When the barometer ſtood at 24 inches they let go a pigeon, who flew with difficulty and would not quit the balloon; but upon being precipitated he in vain endeavoured to regain it, and at length deſcended rapidly towards the earth. At ten at night the balloon had riſen to an height indicated by 22 inches of the mercurial column, the thermometer ſtanding at 4½ degrees (I ſuppoſe centigrade). Here it was that M. Sacharoff carefully obſerved a phenomenon which had been before remarked by M. Robertſon in his aſcent from Hamburgh, but at a much greater elevation. Their dipping needle was deranged; but on inſpecting the common compaſs, its needle was found to be no longer horizontal, the north end being elevated near 10 degrees. On this phenomenon they remark that the magnetic attraction probably diminiſhing as the ſquare of the diſtance may afford additional means of directing future obſervers in the atmoſphere, and even determine the elevations independently of the barometer. From the preſent elevation a pigeon being thrown down, fell ſo directly that it was doubted whether he could have reached the earth alive.

Singular echo of the voice heard from the earth at the diſtance of two miles.

Darkneſs coming on, it became neceſſary to deſcend, during which the obſerver repeatedly made an experiment which alſo promiſes to be of great utility to voyagers in the air, as well as to enlighten our concluſions reſpecting the phenomena of ſound. When they ſpoke through a trumpet directed towards the earth, the voice was returned with extreme preciſion and without

No echo but when the trumpet was directed to the earth.

without ſeeming to have loſt any part of its intenſity. No repetition was made except when the trumpet was directed to the earth; and the intervals of reflection were different according to the elevation of the obſervers. The percuſſion impreſſed on the air by the ſound every time produced a ſlight undulation in the aeroſtat; whence they deduce an inference in favour of the ſuppoſed efficacy of cannon in partly modifying or averting the diſcharge of ſtormy clouds. In one of their experiments, the ſound employed ten ſeconds in its return, which would give a diſtance of about two miles out and home, if the ſame law of the velocity of ſound were ſuppoſed to prevail in the perpendicular courſe as along the ſurface of the earth, which however does not ſeem likely. The barometer ſtood then at 27 inches, and at their outſet it was at 30 inches on the ground. It would be eaſy, and it is ſurely deſirable to make experiments with cannon and ſtop watches on the velocity of aſcending, and if poſſible, deſcending ſound.

The ſtrength of the ſound perhaps depended on the ſtillneſs of the ſituation.

This reflection of ſound or echo is a ſubject of very great curioſity. There is perhaps no other inſtance in nature where ſo extended a wall of reflection can be had. I am diſpoſed to think that the apparent intenſity of the returned ſound may in ſome meaſure have depended on the perfect ſilence in which the ſpeakers were placed. In a ſtill night the centinels on the ramparts at Portſmouth may be heard at the Iſle of Wight, over a diſtance of five miles, and there are numerous inſtances of low ſounds, ſuch as the beating of a clock or watch, or the ſounds of footſteps being heard to conſiderable diſtances, when other ſounds do not act on the organ of ſenſe.*

Sudden riſe of the thermometer in deſcending.

In their deſcent to the earth they paſſed through various ſtrata of vapours, all of different temperatures, and at the inſtant the earth came in ſight, the thermometer ſtarted up through ſeveral degrees, probably becauſe they had quitted a cold maſs of vapour which obſcured their view, or perhaps becauſe the radiant heat of the earth's ſurface might at that moment have reached them unimpaired.

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* See a curious paper of M. Perrole on ſound, with the annotations thereon, in the firſt Vol. of our quarto Journal, page 411.—N.

Letter

XIII.

Letter from Mr. Cuthbertson, *containing Remarks on Mr.* Wm. Wilson's and *Mr.* Hauy's *Experiments on the Electricity of Metals.*

To Mr. NICHOLSON.

London, 54, *Poland-Street*,
Dear Sir, *April* 24, 1805.

Mr. Wilſon's experiments on electricity;

IN your valuable Journal for January, page 42, you have favoured us with a letter from Mr. Wilſon, containing ſome experiments which he calls, *exhibiting the electricity of metals.*

of copper ſifted through zinc,

controverted by another experiment of contact and ſeparation.

Mr. Wilſon ſays in his table of experiments, that copper filings ſifted through zinc is poſitively electrified: this is an *error* which I thought myſelf obliged to take notice of, as it is a direct contradiction to what I have aſſerted in *my examination of Sig. Volta's experiments, which he calls fundamental, and on which his theory of galvaniſm reſts; ſee this Journal, vol.* II. *page* 281; I have ſaid therein, that if a plate of zinc be ſeparated from a copper one, it will be poſitive, conſequently leave the copper negative if inſulated: whether this experiment be performed as I have therein mentioned, or according to Mr. Wilſon, the one reduced into filings, and afterwards ſifted through holes made in the other metal, the electricity excited muſt fall under the ſame denomination, the difference will be that the quantity of electricity excited will be more by Mr. W.'s method than mine, becauſe he has multiplied the ſeparations, a fact well worth notice.

Contact and ſeparation produce the electric ſtate;

Mr. W.'s chief object in view ſeemingly, is to prove that the ſeparation of metals is the cauſe of the electric fluid being excited and not touching. I cannot perceive that theſe experiments throw any light upon that ſubject, as both touching and ſeparating are here employed.

but neither act will do alone.

In my ſecond volume on electricity, publiſhed in Amſterdam, anno 1782, I have proved by experiment that neither *touching* nor *friction* ſeparately employed excites electric fluid, friction and ſeparating jointly employed, is a powerful exciter of electricity on glaſs; touching and ſeparating jointly employed on glaſs excite electric fluid in a ſlight degree, and only when the ſtate of the atmoſphere is favourable.

In

Hauy's obſ. on metallic ſubſtances, imperfect.

In the Philoſophical Magazine for Nov. 1804, page 120, we have Mr. Hauy's obſervations on the electricity of metallic ſubſtances: he does not inform us what ſhape his *ſilver* pieces were, or whether they were pure or with alloy; he however does not heſitate in pronouncing of it to be poſitively electrified by friction; ſo that it does not ſeem that he has entered very wide into the ſubject, or he would have perceived ſome remarkable changes to take place in that metal by friction, and particularly in coins. They will change from poſitive to negative, and *vice verſa*, without any viſible cauſe. If a dollar and a half-crown be ſtuck to the ends of two ſticks of ſealing-wax, and rubbed ſeparately upon woollen cloth, they will be found, after the friction, ſometimes poſitive and ſometimes negative, and ſometimes one poſitive and the other negative, without varying the manner of friction. If pure ſilver, or ſilver with different proportions of alloy, be melted down to a button, and uſed in that ſhape, or hammered flat, repreſenting coins, they are for the moſt part poſitive by friction. Theſe experiments upon metals are not new; I believe they were firſt begun by Mr. Henly, and inſerted in the Philoſophical Tranſactions; but I have not the data at hand, and I do not not remember that he had obſerved the above-mentioned property of this metal.

Singular effect of rubbing coins, &c.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Note on the New Planet Juno.

Planet Juno.

I HAVE not had any late account of the new planet announced at page 301 of our 9th volume (Dec. 1804) in a letter from M. Bode. The diſcoverer's name, who is Mr. Harding of Lilienthal, near Bremen, was not then mentioned.—For the preſent I give the following notes from the Journal de Phyſique, Thermidor laſt.

On the 5th of September, 1804, its right aſcenſion was 1° 52′; declination 0° 11′ north. M. Burckhardt obſerved it on the 23d of September at 359° 7′ and 4° 6′, whence he concludes that the duration of its revolution is five years and a half. Its inclination 21°; excentricity one quarter of its radius; mean diſtance from the ſun three times greater than that of the earth, Its diameter could not be meaſured, but it appeared like a ſtar of the eighth magnitude. It ſeems nearly equal to that of Ceres, or the planet diſcovered by Piazzi.

Diſcovery of Fluoric Acid in the Topaz.[*]

Saxon topaz analyſed by Vauquelin. Braſilian by Deſcotils.

IN the year 1797, M. Vauquelin analyſed the Saxon topaz, and found its conſtituent parts to be, ſilex 31, alumine 68. Mr. Deſcotils ſoon after examined the Braſilian topaz; but as there was a loſs of 18 *per cent.* in his firſt analyſis, and 12 in his ſecond, he did not think fit to publiſh the reſults of his labours at that time; and other circumſtances prevented him from purſuing the inquiry, as he had intended.

Klaproth diſcovered fluoric acid in the Saxon topaz.

Sulphuric acid incapable of expelling it.

Vauquelin treating the topaz with potaſh and ſulphuric acid expelled the fluoric.

Not long ago Mr. Klaproth wrote to Mr. Hauy, that he had found fluoric acid in the Saxon topaz. Mr. Laugier made ſeveral experiments to verify this diſcovery, but without ſucceſs. It is true in the analyſis with potaſh he found a deficiency of 16 per cent. but though he reduced the topaz to an impalpable powder, and did all he could to expel the fluoric acid from it by means of the ſulphuric, he was unſucceſsful. Mr. Vauquelin on his return applied himſelf to the ſame reſearch, and we here preſent the reſult of his labours. Not knowing what proceſs Mr. Klaproth had employed, he tried that which ſeemed to him moſt likely to ſucceed. He firſt heated the topaz with cauſtic potaſh in a ſilver crucible in the uſual way. After he had diluted the maſs with water, he introduced it into a retort, and poured on it ſulphuric acid. White fumes ſoon aroſe, and theſe, being collected, exhibited all the characteriſtics of fluoric acid combined with ſilex. The latter came almoſt wholly from the ſtone, as the retort was not perceptibly attacked by the acid.

The Braſilian topaz gives the ſame reſult.

Topaz a true alumino ſiliceous fluate.

The ſame experiment with the Braſilian topaz gave the ſame reſult; and there is every reaſon to believe, that the Siberian, which Mr. Vauquelin is now analyſing, will afford the ſame products. Thus we may now conſider this gem as a ſiliceous compound, conſiſting of fluoric acid, alumine, and ſilex, or a true alumino-ſiliceous fluate; and the diſcovery muſt be conſidered as of the higheſt importance in mineralogy.

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[*] Bulletin des Sciences, No. XC. p. 282, Sept. 1804.

Mr. Vauquelin next inquired what might be the circumſtance, that led him into an error when he firſt analyſed the topaz; and he imagines, that it was his treating the alcaline maſs with muriatic acid, inſtead of employing the ſulphuric; and that probably not heating it to a ſufficient degree to expel the fluoric acid, from fear of decompoſing the muriate of alumine, he precipitated the fluoric acid combined with the alumine, when he added ammonia to ſeparate the alumine from its muriatic ſolution.

Circumſtance that miſled Vauquelin in his firſt analyſis.

Examination of Crude Platina, by Meſſrs. Tennant *and* Wollaston.*

THE editors of the *Annales de Muſeum* obſerve, that platina, when taken from the mine, appears from the experiments of Deſcotils, Fourcroy, and Vauquelin, to be mixed with iron, chrome, and other metals, among which may be one or two that are new. Mr. Tennant has given names to two, and Dr. Wollaſton to two others; but the French journaliſts congratulate their countrymen for being leſs haſty, and waiting till they are aſſured theſe new metals really exiſt.

Platina.

Prize Queſtions in France.

The Society of Agriculture and the Arts, in the department of the North, propoſes for the ſubjects of two prizes, which ſhall be adjudged, the firſt in the firſt of Fructidor, in the year XIII. and the ſecond in the firſt fortnight of Fructidor, in the year XIV. the two following queſtions:

Prize queſtions reſpecting wool; and an inſect on the colſa.

Firſt Queſtion.—"What method of propagating, rearing, feeding, and houſing the ſheep of the race now exiſting in the department of the North, ought to be followed in this department, to obtain wool from theſe animals, equal in quality to the beſt wools from Engliſh ſheep?"

Second Queſtion.—An inſect known in the country under the improper denomination of *puceron*, has this and ſeveral preceding years attacked and deſtroyed the greater part of the flowers of the colza.—"What is this inſect? Under what generic and ſpecific name have the moſt celebrated naturaliſts deſcribed it? What is its life, either in the ſtate of coleoptera in which it is found on the colza, or in the ſtate of larva? What natural enemies, what artificial means of deſtruction can be oppoſed to it in either ſtate? Generally, what care can preſerve the colza from its ravages?"

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* Bulletin des Sciences, No. XC. p. 234, Sept. 1804.

Each of theſe two prizes will be a gold medal of the value of 150 francs.

Ruſſian Circumnavigation.

Ruſſian voyage round the world.

By a courier, expedited by the governor of Kamſchatka, news was received at Peterſburgh on the 21ſt of December, that the veſſels belonging to the expedition round the world under the orders of M. de Kruſenſtern, had arrived, on the 26th of July laſt, at the harbour of Peter and Paul, in Kamſchatka, and that, up to this time M. Kruſenſtern had not loſt a ſingle man of his retinue, nor had he any ſick in the ſquadron. In his voyage he had viſited the Marqueſas and Sandwich Iſlands, M. Kruſenſtern purpoſed ſailing for Japan, towards the end of Auguſt.

Europeans found at the Marqueſas, who had forgotten their own language.

At one of the Marqueſas, where the veſſels ſtopped a few days, he had taken on board a Frenchman and an Engliſhman, to bring them back to Europe. Captain Kruſenſtern ſays he had not yet been able to diſcover how theſe two individuals had come to this iſland; both of them having completely forgotten their original language. He thought however that he could comprehend that they had arrived there on board an American veſſel which had been ſhipwrecked on the coaſt. The Frenchman ſpeaks the language of the iſlanders very well, and has adopted all their cuſtoms, habits and manner. There is no doubt that they will ſoon recover the uſe of their language, in a daily intercourſe with Europeans; and that they will then be able to give an account of their adventures, as well as information reſpecting the iſlanders, among whom they have lived ſo long. At leaſt, this is expected with impatience.

Geological Journey from the Academy at Warſaw.

Journies of diſcovery in mineralogy and natural hiſtory.

The Society of the Friends of the Sciences at Warſaw have charged two of its members, M. Carteau and Stacio, to undertake a mineralogical and phyſical expedition into the Carpathian mountains. Another member of the ſociety has already examined the eaſtern part of them, with relation to mineralogy, geogony and oryctognoſy. At this time he is travelling over the mountains in the interior of Auſtria, from whence he will proceed to upper Italy and to the Alps of Switzerland. When this journey is completed, he will undertake another to Caucaſus.

Lalande's Propoſal of a new Scale of the Thermometer.

New ſcale of thermometer.

M. de Lalande propoſes to adopt a thermometer-ſcale, which ſhall remedy all the inconveniences of thoſe now in uſe. His mean point is taken from the natural ſtate of the globe, which he fixes at 9¼ degrees of Reaumur's thermometer, and he takes the 10 millionth part of the volume of mercury for the meaſure of a degree. Among the advantages of this inſtrument he reckons a ſimplification of expreſſion, which will give a facility to comprehending what was before without meaning to the public. For example, the degree of heat of common ſummers, and the degree of cold of our mean winters, will be both expreſſed by 30: The degree of 40 will indicate a hot ſummer and a ſevere winter, &c.; another advantage will be derived from the ſmaller interval of the degrees, which will remove the neceſſity of having recourſe to fractions in the greater number of obſervations. The boiling point of water is + 132°.8 of the propoſed thermometer, and — 74°.4 is the point of the congelation of mercury. Ice melts at — 17°.9 and — 44°.2 is the zero of Fahrenheit.

Two Kinds of Honey.

Prouſt on honey.

In a note to Dr. Delametherie, Prouſt announces the diſcovery of two kinds of honey; the one liquid, the other dry, not deliqueſcent, cryſtallizable in its manner, and leſs ſaccharine than ſugar: they are ſeparated by ſpirit of wine, to which end granulated honey muſt be operated on.

J. de Phyſique.

Palladium.

Attempts to form palladium with mercury and platina.

Experiments made by Meſſrs. Roſe and Gehlen, and others by Richter, to obtain palladium, are given at length in the Journal of Chemiſtry, publiſhed in German by Klaproth and Richter.

Theſe philoſophers followed Mr. Chenevix's proceſs with great care, but did not produce that metallic body. In the precipitation of muriate of mercury and muriate of platina, they had a black powder, which always afforded the metals ſeparate from each other.

Richter was not more ſucceſsful. He verified that the green ſulphate of iron does not decompoſe either the muriate of mercury or that of platina. The other facts he obſerved were to the ſame effect as thoſe of Roſe and Gehlen. He always found the mercury of his precipitate ſeparated from the platina by heat.

Traité élémentaire d'Hiſtoire Naturelle, &c. *An elementary Treatiſe on Natural Hiſtory, by* A. M. CONSTAT DUMÉRIL: *Compoſed by Order of Government for the Uſe of the National Lyceums,* 1 *Vol.* 8vo. *Paris* [*].

Elementary works on natural hiſtory very defective.

There is not one of the ſciences, the elementary works of which have been ſo long neglected as natural hiſtory. Sometimes this title has been given to collections of tales fit only to amuſe children, but not calculated to make them acquainted with nature as a whole, and with the progreſs of the ſcience: at other times authors have entered into diſcuſſions too abſtruſe, or contented themſelves with a mere nomenclature, always dry and ſterile to beginners, to whom names give no idea of objects with which they are unacquainted. Mr. D. has preſerved a juſt medium between theſe extremes, while he obſerves an accurate and methodical arrangement. He makes us acquainted with the whole of the productions of nature, and the method of ſtudying and claſſing them, chooſing for examples in every ſection ſuch as are moſt remarkable for their uſes or ſingularity; he continually excites the attention and curioſity of his pupils; and he preſents to them a number of facts neceſſary to be known.

Mr. D. has purſued a better method.

His arrangement proceeds from the moſt ſimple things to the more complex.

In this work Mr. D. has adopted an arrangement, the reverſe of what is uſually employed in books of natural hiſtory; that is, he always proceeds from the moſt ſimple to the moſt complex. He begins with unorganized ſubſtances, proceeds hence to plants, and laſtly to animals; and in theſe he commences

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[*] Bulletin des Sciences, No. 90, p. 236, Sept. 1804.

Its advantages.

with zoophytes, and ends with man. This arrangement has the advantage of inſtilling ideas gradually into the mind of the ſcholar, and avoiding a number of repetitions and anticipations. The hiſtory of organiſed ſubſtances gives him an idea of bodies unmixed with any other ideas: that of vegetables ſhows him organization and life in their moſt ſimple ſtate: and theſe he perceives gradually become more complicated as he aſcends through the different claſſes of the animal kingdom, ſo that the hiſtory of each claſs is but little more than an expoſition of the organs and faculties it enjoys beyond thoſe of the preceding.

Mr. D. has treated the inſects on a new plan.

Though the diſcuſſion of any new idea ſeems contrary to the eſſence of an elementary work, it is obvious, that ſuch a work cannot be well executed but by a man capable of conſidering the whole of a ſcience in its proper point of view. In this reſpect the naturaliſt will here read with pleaſure the article of general obſervations placed at the head of each part: he will diſtinguiſh the hiſtory of inſects, which Mr. D. has treated after a new plan: and he will notice the chapter on man, in which the author diſplays the phyſical characters that diſtinguiſh man from brutes, and the conſequences reſpecting his manners that ariſe from his very ſtructure. This chapter may be conſidered as the connecting link between phyſics and metaphyſics.

Elemens de l'Art de la Teinture, &c. *or Elements of the Art of Dying; with a Deſcription of the Proceſs of Bleaching by the oxigenated Muriatic Acid. Second Edition: by* C. L. *and* A. B. BERTHOLLET. *Paris.*

Berthollet on dying.

This new edition of a work of the firſt merit and celebrity is ſpoken of in the Foreign Journals, as being conſiderably improved by the former author and his ſon. The great perſpicuity and order which are ſeen in all the productions of this eminent chemiſt, and his own unremitted labours in the ſcience, are a ſure guarantee to the ſame effect.

Conſiderations on Organized Beings. By J. C. DELAMETHERIE.

Delametherie on organized beings.

The ſcience of natural hiſtory is indebted to M. Delametherie for ſeveral intereſting works which contain ideas of advantage to the progreſs of human knowledge. Beſides the

Journal de Phyſique, &c. of which he is the editor, he has publiſhed a Theory of the Earth ; a Treatiſe on Man ; different Phyſiological Views of the animal and vegetable Kingdoms, on Vital Air, &c. In this new work the author compares the ſtructure of animals and plants, and applies to vegetables the different ſyſtems or apparatuſes of organs and vital functions, which X. Bichat has propoſed in his General Anatomy. M. Delametherie has benefitted by the experiments and labours of ſeveral philoſophers of merit. All, perhaps, may not adopt ſome of the opinions he offers, the conqueſt of minds being ſtill more difficult than that of hearts ; but they, at leaſt, deſerve examination, and may lead to unexpected reſults.

Bibliotheque de Sonini.

To CORRESPONDENTS.

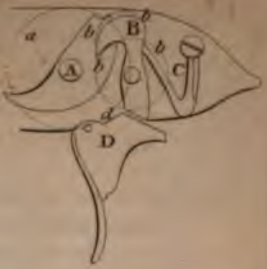
My beſt acknowledgements are due to Mr. D. *who has favoured this work with a preciſe ſtatement of the elucidation of Mr.* BOSWELL'S *ſecond propoſition ; but he will perceive that the paper of Mr.* GOUGH, *which was already printed when his letter came to hand, has rendered it unneceſſary.*

I believe the readers of this Journal will think with me, that the diſpute between C. L. *and Mr.* E. WALKER *has proceeded at leaſt as far as the intereſts of ſcience demand. In a letter from* C. L. *before me, I have ſuppoſed the following explanatory ſentences to afford no new ground for diſcuſſion, and therefore extract them, and hope the buſineſs will end here.* C. L. *ſays :*

" *I cannot conceive how expreſſions ſo plain could be miſunderſtood, and hope you will permit me to endeavour to make them plainer if poſſible.* I have denied Mr. WALKER'S facts ; *that is, I have denied the* truth *of his narrative reſpecting* certain ſuppoſed facts, *and I have pointed out an eaſy way of convincing me and the world that he is not deceived ; namely, that you ſhould repeat his experiments, and ſee if what he aſſerts be true. But Mr.* WALKER declines examining the remainder of my letter, *which is an indiſtinct, though ſufficiently clear way of ſaying, that he does not chooſe to riſk his ſuppoſed facts by putting them to ſuch a trial.*"

Mr. Dodd's Gun Lock.

Fig. 3.



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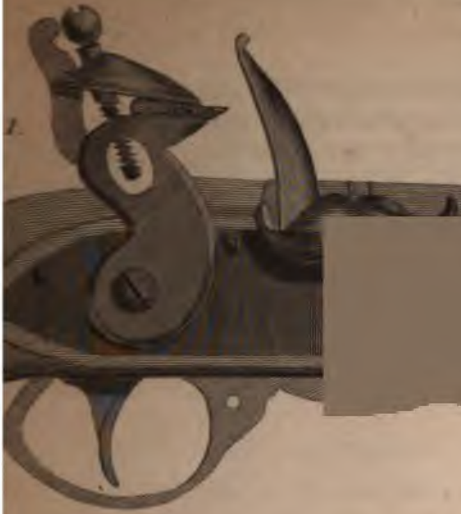

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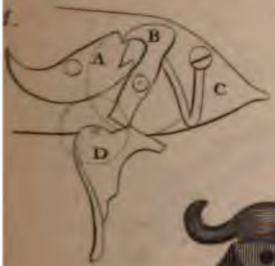
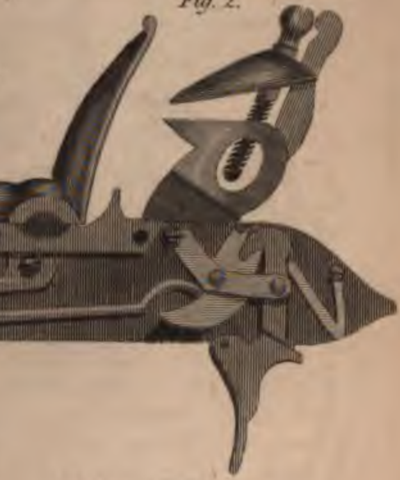


Fig. 2.



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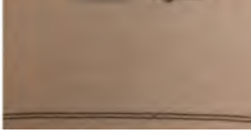
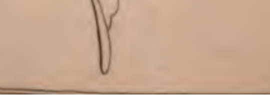
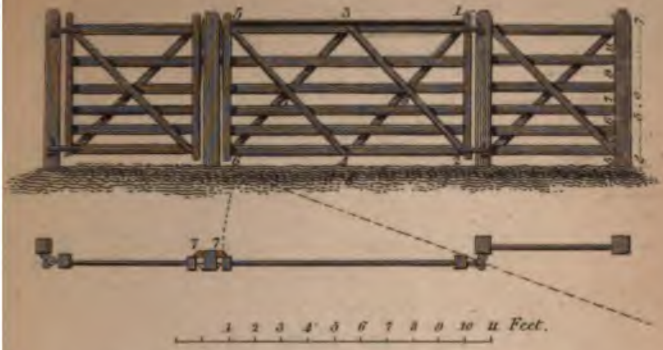


Fig. 5.



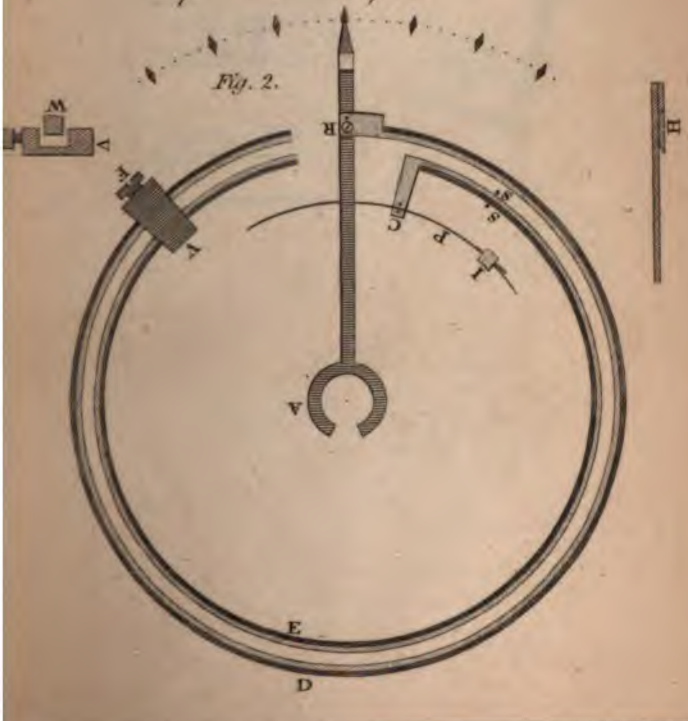
Mr. Waistell's improved Field Gate.

Fig. 1.



Mr. Scott's compensation Curb. for a Time piece.

Fig. 2.



Mutlow Sc. Russell Ct.

Nicholson's Philos. Journal. Vol. XI. Pl. 3.

Bavarian Method of evaporating Salt Waters

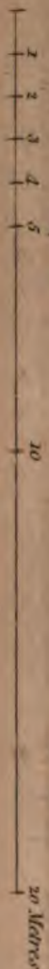


Fig. 3.

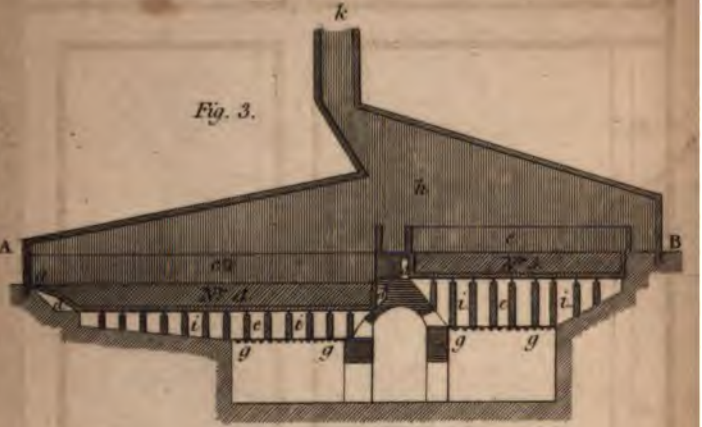


Fig. 4.

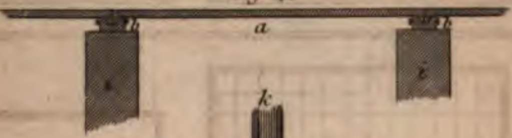


Fig. 2.

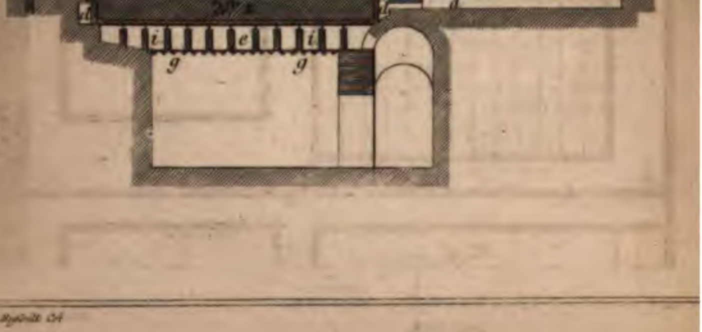


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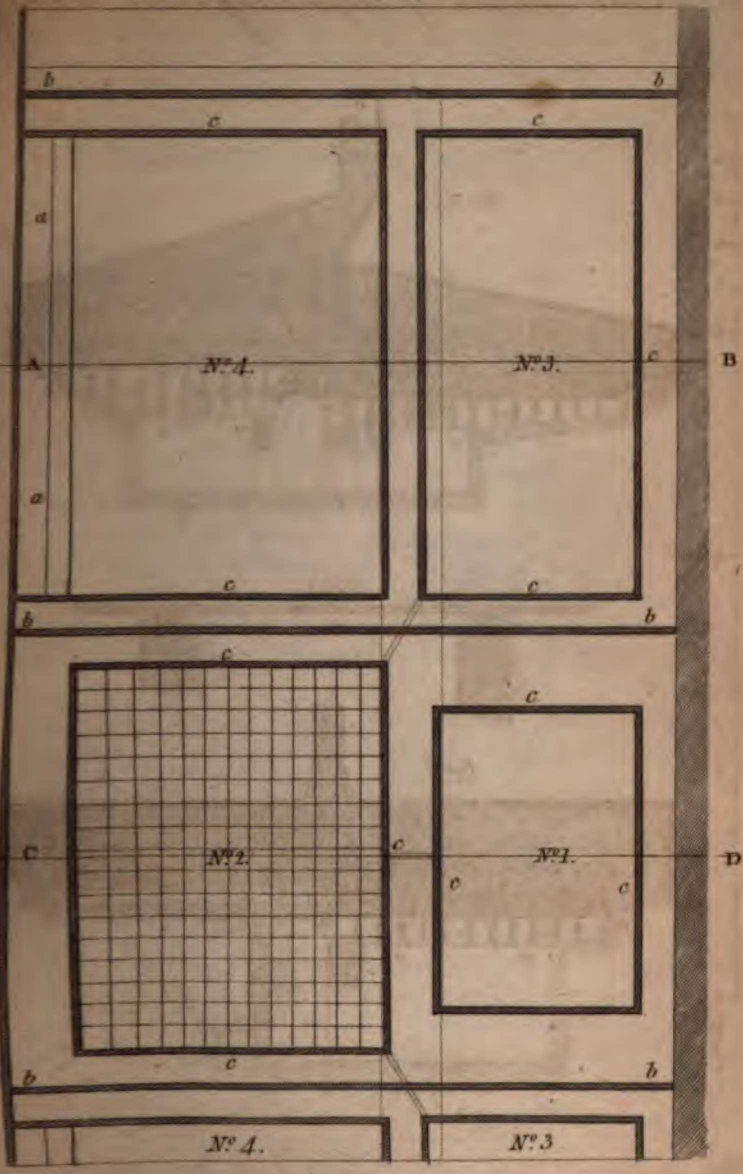
Fig. 1.



Mutlow Sc. Ruſsell Ct.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JUNE, 1805.

ARTICLE I.

Experiments on the Gaſes obtained by the deſtructive Diſtillation of Wood, Peat, Pit-Coal, Oil, Wax, &c. with a View to the Theory of their Combuſtion, when employed as Sources of artificial Light; and including Obſervations on Hydro-Carburets in general, and the Carbonic Oxide: By Mr. WILLIAM HENRY. *(Communicated by the Author.)*

Proceſs of Mr. Murdoch to illuminate by gas from coal.

THE gas obtained by the deſtructive diſtillation of pit-coal, has become an object of conſiderable intereſt and importance, in conſequence of its ſucceſsful application (by Mr. Murdoch, of Soho, near Birmingham *) to the purpoſe of affording light. Having conſtructed an Argand's lamp laſt winter, with the view of effecting the combuſtion of the gas on Mr. Murdoch's plan, I made previous trials with pure hydrogen gas, with carburetted hydrogen obtained by paſſing water over ignited charcoal, and with the carbonic oxide; but found that each of them burned with ſo trifling a production of light, as to be altogether unfit for the purpoſe of illumination; while the light evolved by the gas from coal was little, if at all, inferior to

Neither hidrogen nor carbonic oxide give any notable light; but the gas from coal is nearly equal to oil.

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* See the ſtatement of Mr. M's claim to the diſcovery in the poſtſcript.

This laſt gas burns with much (though diminiſhed) light after it has depoſited its condenſible matter.

that from good ſpermaceti oil. So eſſential a difference in the combuſtion of theſe gaſes, induced me at firſt to believe, that the gas from coal owes its illuminating property to ſomething mechanically ſuſpended in it; but I was afterwards ſatisfied, that though it contains, when recently prepared, much that is ſubſequently depoſited, yet that its quality of burning with a bright and compact flame belongs to it, though certainly with conſiderable diminution, as a permanent gas, after the ſeparation of all condenſible matter. It appeared, therefore, worthy of inveſtigation to determine, on what the ſuperior fitneſs of the gas from coal, for evolving light, depends; and to connect the theory of its combuſtion with that of other ſubſtances, commonly employed as ſources of artificial light.

Products by burning the gas from coal with oxigen and alſo other gaſes.

With this view, numerous comparative experiments were made on the rapid combuſtion of this gas with oxigenous gas in cloſe veſſels, and alſo on that of other inflammable gaſes; and their compoſition may be inferred from the products of theſe experiments, the principal reſults of which are contained in the following table:

Table of reſults.

Kind of Gas.	Oxigen Gas required to ſaturate 100 Meaſures.	Carbonic Acid produced.
Pure hydrogen,	50 to 54	
Gas from moiſt charcoal,	60	35
——— wood (oak)	54	33
——— dried peat,	68	43
——— coal, or cannel,	170	100
——— lamp-oil,	190	124
——— wax,	220	137
Pure olefiant gas,	284	179

If the meaſure of carbonic acid produced, be deducted from the whole oxigen employed, the remainder will expreſs the meaſure of oxigen which was employed in burning the hydrogen of the gas; and this laſt doubled will give the

Now if it be aſſumed (which I believe is as nearly as poſſible the fact) that in the formation of each meaſure of carbonic acid, in the above experiments, an equal volume of oxigen gas is employed*, we ſhall learn, by deducting the numbers in the

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* Mr. Cruickſhank takes it for granted, as the baſis of his calculations, that in forming ſix meaſures of carbonic acid, ſeven meaſures of oxigenous gas are employed. This proportion I believe to be over-eſtimated. Dr. Prieſtley obſerves, (on Air, 2d Edition, III. 377), "I heated 8¼ grains of perfect charcoal in 70 ounce meaſures of dephlogiſticated air, when it ſtill remained 70 oz.

m.½

the third column from the correſponding one in the ſecond, what proportion of the conſumed oxigen has been allotted to the ſaturation of the hydrogen of each hydro-carburet. Thus, for example, in the combuſtion of the gas from coal, 70 parts of oxigen have diſappeared, beſides that which has entered into the carbonic acid; and, ſince each meaſure of oxigen ſaturates two of hydrogen gas, the gas from coal muſt contain, in 100 meaſures, a quantity of hydrogen which, expanded to its uſual elaſticity, would occupy 140 meaſures. By a ſimilar mode of eſtimation, the quantity of hydrogen in other ſpecies of inflammable gas may be aſcertained; *viz.* by ſubtracting the number in the third from the correſponding one in the ſecond column, in each inſtance, and doubling the remainder.

meaſure of hydrogen originally preſent.

The above experiments ſufficiently explain why the gas from coal evolves ſo much more light, during combuſtion, than either hydrogen or the hydro-carburet from moiſt charcoal, becauſe, in an equal volume, it includes, in its compoſition, above thrice the quantity of inflammable matter preſent in hydrogen gas, and nearly thrice as much as is contained in the gas from moiſt charcoal. The appreciation of the degree of combuſtibility of each gas, by the quantity of oxigen required for its ſaturation, entirely agrees, as might naturally be expected, with that founded on the phenomena of ſilent combuſtion in an Argand's lamp; for each gas ſeemed to me to evolve light, as nearly as could be judged, in proportion to the quantity of oxigen conſumed by its detonation in a cloſe veſſel. Above all others, the olefiant gas [*] is decidedly entitled to rank, by the ſplendor and beauty of the light which it yields; and the violence of its detonation, when fired with a mixture of oxigen gas, alſo ſurpaſſes that of every other inflammable gas. By exploding only .03 of a cubic inch with .17 of oxigen gas, a ſtrong glaſs tube was

Theſe facts explain the greater illumination from the coal gas. The light is greater the more oxigen is required for the combuſtion.

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m.; but, after waſhing in water, was reduced to 40 oz. m." In this experiment one grain and ¼ of charcoal was conſumed, and 30 oz. m. of carbonic acid were generated, without any change in the volume of the oxigenous gas.

[*] A full abſtract of the memoir of Meſſrs. Deiman, &c. on this intereſting gas, may be ſeen in the 1ſt Vol. of the 4to Series of this Journal. Its characteriſtic property is that of being rapidly condenſed into oil, by contact with oxigenized muriatic acid gas.

ſhattered with violence; and a Volta's eudiometer, $\frac{1}{4}$ of an inch in thickneſs was burſt by leſs than a cubic inch of a mixture of the two gaſes.

Specific gravity of an inflammable gas (freed from carbonic acid) is a teſt of its fitneſs to afford light.

The ſpecific gravity of the inflammable gaſes, when perfectly freed from carbonic acid, is another competent teſt of their fitneſs as ſources of light. Thus the ſpecific gravity of the gas from moiſt charcoal, (common air being 1000) according to Cruickſhank is 480; of the hydrocarburet from alcohol 520, and of the olefiant gas, as determined by the Dutch chemiſts, 909.

It is probable that the inflammable gaſes are mixtures of few ſimple gaſes.

From the limitation to the proportions, in which bodies in general, having a ſuſceptibility of chemical union, are capable of combining, it ſeems to me reaſonable to infer, that carbon and hydrogen do not unite in all poſſible proportions, forming ſo many diſtinct compounds; but that the various inflammable gaſes are mixtures of a very few ſimple ones. Of thoſe at preſent known, pure hydrogen gas; the carburetted hydrogen, which by combuſtion affords an equal bulk of carbonic acid, and conſumes twice its bulk of oxigen; the carbonic oxide; and the olefiant gas, it will appear, may be traced in the mixed gaſes comprehended in the forgoing table.

Gas from coal appears to be hydro-carburet with perhaps ſome carbonic oxide.

The gas from coal I apprehend to be principally hydro-carburet, with perhaps ſome portion of carbonic oxide, the preſence of which laſt is rendered probable, becauſe the gas from coal is ſaturated by leſs than twice its bulk of oxigen, though it gives an equal volume of carbonic acid. Now the gas from marſhes, which, with Mr. Cruickſhank, Mr. Dalton finds to be hydrocarburet, contaminated with about 20 *per cent.* azotic gas, conſumes, making allowance for this adulteration, double its volume of oxigen gas; and ſince the gas from coal requires a leſs proportion than this of oxigen, and yet gives an equal product of carbonic acid with that from marſhes, it is fair to preſume, that it muſt previouſly have contained ſome oxigen, which, after waſhing it with lime-water, can ſubſiſt in no other ſtate than that of the carbonic oxide.

Gas from ignited charcoal and water is probably carbonic oxide with hydrogen and a little hydro-carburet.

The gas obtained by decompoſing water over ignited charcoal, is moſt probably a mixture of carbonic oxide with hydrogen gas, and perhaps a little hydro-carburet. On no other preſumption can the reſults of its combuſtion be explained; ſince the quantity of oxigen required in ſaturating 100 meaſures is only ten more than are conſumed by 100 meaſures of pure hydrogen, though 35 m. of carbonic acid, containing at leaſt

35 of oxigen, are found after combuſtion. Now, according to Mr. Cruickſhank, theſe 35 m. of carbonic acid, if formed from carbonic oxide, would require only 15 additional meaſures of oxigen gas, which is not very remote from the truth. Reaſoning in the ſame mode, the gaſes from wood and from peat will appear to be mixtures in different proportions of two at leaſt of the above-mentioned, viz. hydrogen, and the carbonic oxide. The reſults of the combuſtion of the gaſes from wood and peat evince that they differ conſiderably from that obtained from coal; and contain much leſs uncombined inflammable matter. Another circumſtance of diſtinction alſo is, that, before being waſhed with lime-liquor, the gas recently prepared from wood or from peat, contains from $\frac{1}{4}$ to $\frac{1}{3}$ its bulk of carbonic acid; whereas the gas from coal loſes by this ablution only from $\frac{1}{27}$ to $\frac{1}{13}$.* In my firſt experiment, I found a large admixture of azote in all theſe gaſes; but this afterwards proved to be accidental and not eſſential; ſince by careful diſtillation in glaſs retorts, of the ſubſtances that afforded them, the gaſes were obtained entirely free from this contamination.

Gaſes from wood and from peat contain leſs of uncombined inflammable matter than the coal gas.

The gaſes obtained by the deſtructive diſtillation of oil and of wax, it may be obſerved in the table, conſume conſiderably more oxigen than the gas from coal. This circumſtance firſt led me to ſuſpect that they might poſſibly be mixtures of the olefiant gas with carburetted hydrogen; and on applying the oxygenized muriatic acid gas, this ſuſpicion was fully verified. One meaſure of the gas from oil with one of the oxygenized gas, were reduced ſpeedily to 1¼; a like diminution was produced in the gas from tallow; and that from wax had its bulk ſtill farther contracted, only 1½ m. being left by ſimilar proportions.

The gaſes from oil and from wax are pure hydro-carburets, except that the firſt contains one-eighth and the latter one-fourth of olefiant gas.

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* The condenſible products alſo of coal probably differ from thoſe of wood and peat. If an intermediate veſſel be placed for their reception, it emits, after the diſtillation, a ſtrong ſmell of ammonia. This was long ago obſerved by Lord Dundonald, who enumerates, among other products of coal, the volatile alkali (ſee a pamphlet "on the Uſes and Qualities of Coal Tar," publiſhed by his Lordſhip in 1785.) This production of ammonia I have not obſerved from peat or wood; nor do I find it mentioned in a Hiſtory of Peat, including the reſults of its diſtillation, &c. publiſhed in the ſecond vol. of the Edinburgh "Eſſays Phyſical and Literary."

In order to aſcertain how much of the diminution was owing to the condenſation of olefiant gas, the proportion of oxygenized acid, required for the ſaturation of that gas, was carefully aſcertained. After ſeveral trials, it was found that 3 m. of the oxygenized acid with 2½ pure olefiant gas, prepared according to the Dutch chemiſts, left only 0.15 m. of common air derived from the veſſels. It appears, therefore, that the gas from oil and from tallow contains about ⅛, and that from wax ¼ olefiant gas, the reſt being pure hydro-carburet.

Hence it is ſeen why waſhing diminiſhes the product of carbonic acid afforded by burning an hydrocarburet. For it takes away the olefiant gas.

The reſults of theſe experiments, in connection with a fact communicated to me by Mr. Dalton, explain ſome circumſtances obſerved by Mr. Cruickſhank, for which that ingenious chemiſt was at a loſs to account, viz. the great variation in the products of carbonic acid, obtained by burning the ſame hydrocarburet when waſhed and when unwaſhed, or when long kept in contact with water; though the gas, when originally procured, was perfectly free from carbonic acid. The olefiant gas, Mr. Dalton has aſcertained, is far more abſorbable by water than other ſpecies of hydrocarburet, viz. in the proportion nearly of ⅛. Now the gas from camphor, I find to contain much olefiant gas, and indeed this might have been inferred from Mr. Cruickſhank's own ſtatement, who obſerves that this gas, by admixture with oxygenized muriatic acid, undergoes a conſiderable diminution of bulk. The pure hydrocarburet, on the contrary, I have never ſeen at all condenſed by contact with this gas, in the rapid manner obſervable in olefiant gas or mixtures containing it; though, by confinement together for ſome hours, they are mutually decompoſed into common muriatic acid, carbonic acid, and water. The hydrocarburets from ether and alcohol alſo contain olefiant gas; and this, I apprehend, will be found to be the fact with all inflammable gaſes, which by combuſtion give more than their own bulk of carbonic acid. The variable products of carbonic acid, obtained in Mr. Cruikſhank's experiments, from equal quantities of different hydrocarburets, cannot, therefore, be conſidered as denoting ſo many diſtinct ſpecies of carburetted hydrogen; but as owing to the admixture with this of various proportions of olefiant gas.

Hydrocarburets from ether and alcohol alſo contain ol. gas; which is the general cauſe of the difference in carb. acid products by Mr. Cruikſhank.

Error of that author reſpecting the conſtitution of carburetted hydrogen gaſes.

It will not, I am perſuaded, be regarded as indicating a wiſh to detract, in the ſmalleſt degree, from the credit due to Mr. Cruikſhank, whoſe memoirs on the hydrocarburets and carbonic oxide I eſtimate among the moſt ingenious, and generally ſpeaking, the moſt ſatisfactory examples of chemical reſearch, if I obſerve that the part of his table (vol. V. p. 8 of the 4to ſeries of this Journal) which relates to the conſtitution of the carburetted hydrogen gaſes, I conſider as entirely erroneous. To excite ſtrong ſuſpicion of the accuracy of the proportions aſſigned to theſe gaſes, it is ſurely ſufficient that one of them (the gas from moiſt charcoal) is ſtated to contain in 100 cub. in. = 14½ grains, no leſs than 9 grains of water, a proportion abſolutely inconceivable; and the ſame objection applies, in a leſs degree, to the other caſes. Now in 100 cubic inches of the muriatic acid gas, I found the abſolute quantity of combined water to be only 1.4 gr. (Phil. Tranſ. 1800); and it is rendered highly probable, by the experiments of Clément and Deſorme (*Ann. de Chim.* XLII. 121) that all gaſes contain the ſame quantity of water. In the inſtance of the gas from charcoal, Mr. C. was moſt probably miſled, by not having ſuſpected the preſence of the carbonic oxide; and the correction is to be made as follows. One hundred cubic inches (=14½ grains) combined with the proper quantity of oxigen, gave 19 grains of carbonic acid, containing very nearly 4 grains of carbon; and ſuppoſing the carbon in the gas before combuſtion to have been in the ſtate of carbonic oxide, it would be combined with about 9 grains of oxygen, and would conſtitute 13 gr. or 43½ cub. in. of carbonic oxide.— There remains then only 1½ grain, of the 14½ ſubmitted to experiment to be accounted for, which is very exactly made up by the reſiduary 57½ cub. in. of hydrogen gas, taking the weight of 100 cub. in. of hidrogen to be 2.6 gr. The water contained in the gas may, I think, be ſet out of the queſtion; for it muſt be recollected that the product of the combuſtion is in part aeriform; and it may be conſidered as a tolerable approximation to the truth, that the gas from charcoal contains, in 100 inches, 43 of carbonic oxide, the remainder being principally hydrogen gas.

Carbonic oxide does not contain combined hidrogen, &c.

With reſpect to the preſence of hydrogen in the carbonic oxide, which has been a topic of controverſy, neither the fact nor the negative can, I think, be at preſent with certainty affirmed. If however any hydrogen be contained in it, I ſhould deem it an accidental and not an eſſential ingredient, and am of opinion that, if preſent at all, it exiſts in the ſtate of hydrogen gas; for I find that the carbonic oxide is not expanded by electrical diſcharges, which would aſſuredly happen if the carburetted hydrogen were one of its conſtituents, or accidentally mixed with it.

Elucidation of the theory of lamps, &c.

The oil, &c. is decompoſed in the wick,

—into olefiant and carburetted hydrogen gaſes, which are then burned. Whence the fitneſs of any fuel to give luminous flame may be known from its deſtructive diſtillation.

To return to the theory of lamps, &c. it is proved by the preceding experiments, that the ſubſtances ordinarily employed as ſources of artificial light, viz. oil, tallow, and wax, afford when ſubmitted to an increaſed temperature, much olefiant gas; and it has been juſtly obſerved by the editor of this Journal (4to ſeries I. 71) "that the wick of a lamp or candle ſurrounded by flame is exactly in the ſituation of a body expoſed to deſtructive diſtillation in a cloſe veſſel." In this caſe the ſeries of capillary tubes compoſing the wick, ſerve perhaps preciſely the ſame office as a tube horizontally diſpoſed in a heated furnace, through which an inflammable liquid is tranſmitted. The fuel previouſly melted, is drawn up into theſe ignited capillary tubes, and there reſolved into olefiant and carburetted hydrogen gaſes, from the combuſtion of which gaſes, and not merely of a condenſible vapour, it appears to me that the illumination chiefly proceeds. Hence it is not improbable, that the proportion of olefiant gas and hydrocarburet, obtained by the diſtillation of any ſubſtance, will be a tolerable meaſure of its fitneſs for affording light. In diſtillations of this kind, however, the degree of heat is of conſiderable moment, for I have found that the olefiant gas may be obtained or not, at pleaſure, during the decompoſition of ether, alcohol, oil, &c. by varying the temperature to which the containing veſſels are expoſed.

The gaſes from coal, &c. though they offered no olefiant gas, afford much light by an inflammable vapour when recent.

In the gaſes from coal, peat, and wood, though theſe ſubſtances yield no olefiant gas, the defect is compenſated by an inflammable vapour diffuſed through them when recent, and which is even not removed by paſſing through a ſmall quantity of water. Gas from coal, however, which had ſtood over water upwards of a month, I have found burns with conſiderably impaired brilliancy, though ſtill with a far more denſe and bright flame than hydrogen gas, or the gas from charcoal.

Mancheſter, May 4, 1805.

POSTSCRIPT.

The gas from coal appears by exp. with ox. m. acid gas to contain a ſmall portion of ol. gas.

Since the preceding pages were written, I have examined a freſh portion of the gas from coal, obtained by very cautious diſtillation, with a view to aſcertain whether any olefiant gas can be procured from that ſubſtance. Of this gas five meaſures, mixed with five of oxygenized muriatic acid, were reduced to nine; from which it ſhould appear that the gas from coal may poſſibly contain $\frac{1}{10}$ of olefiant gas. The production of oil, however, was not ſo manifeſt as in other inſtances; and I judged it to have happened chiefly becauſe an irideſcent film was viſible on the ſurface of the water when held between the eye and the light.

Hiſtory of Mr. Murdoch's experiments for giving light by gas from pit-coal.

I am enabled alſo, by a letter received this morning from a friend who is well acquainted with the progreſs of Mr. Murdoch's experiments, in anſwer to ſome queries from me, to ſtate ſpecifically the grounds of that gentleman's claim to the important application of coal as a ſource of artificial light.—This I cannot do better than by an extract from the letter.

“In the year 1792, at which time Mr. Murdoch reſided at Redruth in Cornwall, as Boulton and Watts principal agent and manager of engines in that county, he commenced a ſeries of experiments upon the quantity and quality of the gaſes contained in different ſubſtances. In the courſe of theſe, he remarked, that the gas obtained by diſtillation from coal, peat, wood, and other inflammable ſubſtances, burnt with great brilliancy upon being ſet fire to; and it occurred to him, that by confining and conducting it through tubes, it might be employed as an economical ſubſtitute for lamps and candles. The diſtillation was performed in iron retorts, and the gas conducted through tinned iron and copper tubes, to the diſtance of 70 feet. At this termination, as well as at intermediate points, the gas was ſet fire to, as it paſſed through apertures of different diameters and forms, purpoſely varied with a view of aſcertaining which would anſwer beſt. In ſome, the gas iſſued through a number of ſmall holes, like the head of a watering pan; in others it was thrown out in thin long ſheets, and again in others in circular ones, upon the principle of Argand's lamp. Bags of leather and of varniſhed ſilk, bladders, and veſſels of tinned iron were filled with the gas, which was ſet fire to, and carried about from room to room, with a view of

aſcer-

Hiſtory of Mr. Murdoch's experiments for giving light by gas from pit coal.

aſcertaining how far it could be made to anſwer the purpoſe of a moveable or transferable light. Trials were likewiſe made of the different quantities and qualities of gas produced by coals of various deſcriptions, ſuch as the Swanſea, Haverfordweſt, Newcaſtle, Shropſhire, Staffordſhire, and ſome kinds of Scotch coals."

"Mr. Murdoch's conſtant occupations prevented his giving farther attention to the ſubject at that time; but he again availed himſelf of a moment of leiſure to repeat his experiments upon coal and peat, at Old Cumnock in Ayrſhire in 1797; and it may be proper to notice that both theſe, and the former ones, were exhibited to numerous ſpectators, who, if neceſſary, can atteſt them. In 1798, he conſtructed an apparatus at Soho Foundry, which was applied during many ſucceſſive nights, to the lighting of the building; when the experiments upon different apertures were repeated and extended upon a large ſcale. Various methods were alſo practiſed of waſhing and purifying the air, to get rid of the ſmoke and ſmell. Theſe experiments were continued, with occaſional interruptions, until the epoch of the peace in the ſpring of 1802, when the illumination of the Soho manufactory afforded an opportunity of making a public diſplay of the new lights; and they were made to conſtitute a principal feature in that exhibition. I do not know exactly at what time the firſt trials were made, or publiſhed in France. The firſt notice we received of them here, was in a letter from a friend at Paris, dated the 8th of Nov. 1801, in which he deſires me to inform Mr. Murdoch, that a perſon had lighted up his houſe and gardens with the gas obtained from wood and coal, and had it in contemplation to light up the city of Paris."

"After mentioning the above, I think it is proper to ſtate alſo, that in the ovens conſtructed upon Lord Dundonald's plan, at Calcutts in Shropſhire, for the purpoſe of ſaving the tar, &c. which eſcapes during the coaking of coal, it has been uſual for a number of years paſt to ſet fire to the large current of gas as it flies off, and thus procure a bright illumination. This however was not known to Mr. Murdoch, and was never ſeen by him."

II.

Experiments on the Analyſis of Goulard's Extract, or the Aqua Lithargyri acetati. By JOHN BOSTOCK, *M. D. Communicated by the Author.*

To Mr. NICHOLSON.

DEAR SIR.

I HAVE the pleaſure to tranſmit to you ſome experiments on the analyſis of Goulard's extract, which I hope you may think not unworthy of a place in your Journal.

I am, Sir,
Your obedient Servant,
JOHN BOSTOCK.

Liverpool,
May 5, 1805.

The extract of Goulard coagulates mucilage more than the acetate of lead.

During the courſe of ſome experiments on the analyſis of animal fluids, I was led to obſerve the effect of the *aqua lithargyri acetati*, or the extract of Goulard, as a coagulator of mucus, and particularly to notice the ſuperiority of its power over that of the acetate of lead. From theſe circumſtances I was induced to examine into the opinions that had been entertained reſpecting its compoſition, but was not able to obtain any ſatisfactory information. Although it is a compound ſo well known, and ſo frequently employed, it appears never to have been made the ſubject of chemical analyſis. In Dr. Thomſom's Syſtem of Chemiſtry it is not diſtinguiſhed from the common acetate of lead;* Dr. Murray informs us, that "it is merely a ſolution of acetate of lead in water, with an "exceſs of acid;†" and Dr. Duncan, Jun. conceives, that it does not differ from a ſolution of the acetate of lead of the ſame ſtrength.‡ We meet with nothing ſpecific reſpecting its conſtitution in M. Fourcroy's "*Syſteme*", § nor is there any light thrown upon it by his acute commentator Prouſt.|| In this dearth of information, I proceeded to make the following experiments.

It has not been examined.

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* Chem. III. 52. † Mat. Med. II. 223. ‡ Edin. Diſp. p. 506
§ VIII. 203. || Journ. Phyſ. LVI. 207.

To

Experiments on its compoſition. The common acetate of lead was diſſolved in water.

1. To 200 grains of diſtilled water were added 60 grains of acetate of lead, in its uſual cryſtalline ſtate; the fluid was kept for about an hour at the boiling temperature, and was afterwards filtered. The reſidue, when dried, did not amount to more than 2 grains; this was boiled in a freſh quantity of water, when about half of it was diſſolved, but one grain ſtill remained not acted upon by the water. It appeared, therefore, that a ſaturated ſolution of the acetate of lead was formed; it was tranſparent and colourleſs; it ſlightly reddened paper ſtained with litmus.

This was precipitated by carbonate of potaſh. It gave eleven parts carbonate of lead.

2. A ſolution of the carbonate of potaſh was prepared, in the proportion of 11.25 grains of potaſh to 100 grains of water. To 40 grains of the ſolution of the acetate of lead from No. 1, a quantity of this ſolution of potaſh was gradually added; a copious precipitate of the carbonate of lead was produced; after the addition of 60 grains of the alcaline ſolution there was no farther precipitation, and the fluid ſlightly affected a paper ſoaked in the infuſion of the mallow flower. The precipitate was carefully collected, and being dried by a gentle heat, afforded eight grains of carbonate of lead.

Goulard's extract gave eleven parts carbonate of lead.

3. Forty grains of *aqua lithargyri acetati* were treated in the ſame manner with the carbonate of potaſh; the precipitate formed appeared more copious than in the former experiment, and after the addition of 40 grains only of the alkali, no farther effect ſeemed to be produced; the fluid affected the mallow-paper in the ſame degree as in the former experiment. The precipitate, being collected and dried, weighed 11 grains.

It did not redden litmus paper. Blue precipitate with litmus infuſion.

4. The *aqua lithargyri acetati* did not in any degree redden litmus paper; a few drops of it were added to half an ounce of the infuſion of litmus; a precipitate of a beautiful light blue was immediately formed, while the fluid was left tranſparent and nearly colourleſs.

The ſolution of acetate gave 4 gr. brittle clear reſidue by evap.

5. Twenty grains of the ſolution of acetate of lead, No. 1, were ſlowly evaporated; the fluid became extremely viſcid, and at length, in ſome degree, brittle and tranſparent, and aſſumed the appearance of dried gum. It weighed about four grains.

Aqua lith. acet. gave 5 gr. white opake reſidue.

6. Twenty grains of the *aqua lithargyri acetati* were evaporated in the ſame manner; it became white and opake, and when the proceſs was completed, it exhibited the appearance of a number of ſcales of a pearl colour. It weighed a little more than five grains.

7. A

Gum precipitated by Goulard's extract.

7. A ſolution of gum Arabic was formed, in the proportion of one part of gum to 100 parts of water. One grain of this ſolution was added to 39 grains of water, ſo that the gum only conſtituted $\frac{1}{4000}$ part of the ſolution; a ſingle grain of Goulard dropped into it produced a perceptible opacity.

—but ſcarcely by acetate of lead.

8. Twenty grains of the ſaturated ſolution of the acetate of lead, had one grain of the ſolution of gum added; the effect was barely viſible, certainly leſs than in the former experiment.

I am far from conſidering theſe experiments as ſufficient to afford a complete inveſtigation of the ſubject; but I think they may enable us to make ſome advances towards the truth.

Forty grains acetate contain ſix grains metallic lead.

The 40 grains of the ſolution No. 1. contain 11.6 grains of acetate of lead; by the addition of the alcali, this was converted into eight grains of the carbonate of lead. Theſe eight grains of carbonate conſiſt of 6.72 grains of the yellow oxide, and 1.28 grains of carbonic acid.* The 6.72 grains of yellow oxide conſiſt of 6.12 grains of pure lead, and .6 grains of oxigen, † ſo that the 40 grains of the ſaturated ſolution contain a little more than 6 grains of pure lead.

Forty grains Goulard contain 8½ of lead.

By employing the ſame reaſoning to the analogous experiment with Goulard, we may conclude, that the 11 grains of carbonate produced in this caſe, conſiſt of 9.24 grains of the yellow oxide of lead, and 1.76 grains of carbonic acid; the 9.24 grains of oxide will be compoſed of 8.4 grains of pure lead, and .84 grains of oxigen, ſo that the 40 grains of the *aqua lithargyri acetati* contain nearly 8½ grains of pure lead.

Deduction of the reſpective quantities of acid.

We ſhall not find it ſo eaſy to aſcertain preciſely the quantity of acetic acid which enters into the compoſition of the acetate of lead, and the *aqua lithargyri acetati* reſpectively; but if we truſt to the experiments of M. Thenard ‡, we muſt conclude that 11.6 grains of acetate of lead, contain about three grains of the acetic acid. The quantity of acid in the *aq. lith. acet.* is leſs than that in the acetate of lead, in the proportion of 40 to 60; therefore the 40 grains of *aq. lith. acet.* will only contain two grains of acid.

Component parts of each.

Hence it follows, that 40 grains of the ſolution of acetate of lead conſiſt of,

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* Thomſon's Chem. III. 50. † Prouſt, Journ. de Phyſ. LVI. 206. ‡ Nich. Journ. VI. 223.

Lead

	Grs.			Grs.
Lead - -	6.12	The ſame quantity of Goulard will conſiſt of - -	Lead	8.4
Acetic Acid	3.		Acid	2.
Oxygene	.6		Oxygene	.84
Water	30.28		Water	28.76
	40.00			40.00

converting theſe proportions into quantities of 100 grains each they will be as follows,

	Sol. acet. lead.	Aq. lith. acet.
Lead - - -	15.3	21.
Acetic acid -	7.5	5.
Oxygene -	1.5	2.1
Water - -	75.7	71.9
	100.0	100.0

The experiments ſhew that Goulard and the acetate are different ſalts.

From this ſtatement it appears, that in the *aq. lith. acet.* the oxide of lead and the acid exiſt to each other in the proportion of 23 to 5, or of 100 to 21.74, and we find that M. Thenard has deſcribed the perfect acetate of lead, as a ſalt in which the oxide and the acid exiſt in the proportion of 100 to 21.79. So near a coincidence between theſe two proportions can ſcarcely be regarded as the mere effect of accident; but muſt rather be conſidered as a proof that the ſubſtances operated upon were nearly, if not altogether, identical. Admitting this to be the caſe, we muſt conclude that the *aqua lithargyri acetati* is a ſaturated ſolution of the proper acetate of lead, that it is an eſſentially different ſalt from the ſuper-acetate of lead, and that it is not, as has been imagined, an accidental compound, but an exactly neutralized ſalt, the conſtituents of which exiſt in a conſtant ratio to each other.

The former being at ſaturation,

—and the latter a ſuper-acetate.

The neutral compound is moſt eaſily decompoſed,

—and is therefore a better teſt of mucus.

It happens in this, as in other inſtances, that the ingredients compoſing the completely ſaturated compound, poſſeſs a weaker affinity for each other than when they exiſt in a different proportion. To this circumſtance muſt be attributed the ſuperior delicacy which Goulard poſſeſſes, as a teſt of animal and vegetable mucus, over the ſuper-acetate of lead, or the common *ceruſſa acetata.* The *aqua lithargyri acetati* is ſpeedily decompoſed by the action of the atmoſphere, in conſequence of the oxide of lead which enters into its compoſition having a ſtronger affinity for carbonic than for acetic acid; this effect takes place in a leſs degree, in a ſaturated ſolution of ſuper-acetate of lead.

From

Super-acetate of lead more ſoluble than generally ſuppoſed.

From the firſt experiment we learn, that the ſuper-acetate of lead is more ſoluble in water than is generally imagined; Dr. Thomſon obſerves, that it is diſſolved only ſparingly [*]; yet we find that 100 parts of water retain in ſolution 27 parts of the ſalt.

III.

A conciſe View of the Theory of Reſpiration. By W. Brande', *Eſq. (From the Author.)*

Reſpiration,

THE term reſpiration implies the reception of atmoſpheric air into the lungs, and its ſubſequent emiſſion, after having produced changes in the blood neceſſary to the continuance of life [†].

could not be explained till the atmoſphere was analyſed.

No other gaſeous body being capable of producing theſe changes, it was natural to ſuppoſe, that until we became acquainted with the component parts of the atmoſphere, very little of the true nature of reſpiration could be underſtood.

Diſcoveries of Prieſtley, and Lavoiſier.

The firſt great ſtep towards the analyſis of the air was made by Dr. Prieſtley, who in the year 1774 diſcovered oxigen gas, called by him dephlogiſticated air. But we are indebted to Lavoiſier for the moſt accurate inveſtigation on this ſubject; who from many experiments, which it is not neceſſary here to relate, concluded that atmoſpheric air was compoſed of oxigen and azot, in the proportion of about 27 parts of the former to 73 of the latter. The air alſo contains a ſmall quantity of carbonic acid, and a conſiderable quantity of water (ſubject however to much variation) is always ſuſpended by it.

Gas which cannot be reſpired, or admitted into the lungs.

Some of the gaſes are totally unreſpirable, that is to ſay, incapable of being taken into the lungs; for whenever this is attempted, a ſpaſmodic affection of the epiglottis takes place, which by cloſing on the larynx, ſhuts up all communication with the organs of reſpiration. To this claſs belong all thoſe gaſeous

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[*] Thomſon's Chemiſtry, III. 53.

[†] Reſpiration has been divided into, 1. Inſpiration, or the ingreſs of air into the cells of the lungs, cauſed by the enlargement of the cavity of the cheſt; 2. Into expiration, or the egreſs of air from the lungs, cauſed by the contraction of the cheſt.

Carbonic acid. Its effects; as deſcribed by Pilatre de Rozier.

gaſeous bodies poſſeſſed of acid properties. The effects of carbonic acid are deſcribed as follows by Pilatre de Rozier:—He went into a brewer's tub which was full of carbonic acid gas; he at firſt felt a ſlight heat throughout his whole body, which produced a gentle perſpiration; an itching ſenſation frequently obliged him to cloſe his eyes, and on attempting to breathe, he was prevented by a very violent ſenſe of ſuffocation. He wiſhed to get out of the tub, but being unable to find the ladder, the neceſſity of breathing increaſed, he was ſeized with a violent giddineſs, and felt a tingling ſenſation in his ears. He at length contrived to extricate himſelf, and although he then experienced no difficulty in breathing, he was unable to diſtinguiſh the objects around him; his hearing was alſo much impaired. On repeating the experiment he found, that as long as he remained without attempting to breathe, he could readily move or even ſpeak, but whenever he tried inſpiration, a violent ſenſe of ſuffocation came on.

Gaſes which can be admitted into the lungs.

Hydrogen and azot kill by mere ſuffocation.

Gaſes which kill by a ſpeedy poſitive action.

Nitrous oxide and oxigen kill by a ſlower action.

Atmoſpheric air maintains life.

But there are certain gaſeous bodies which may be drawn into the lungs, meeting with no oppoſition from the organs of reſpiration. Dr. Thomſon has divided theſe into four claſſes. The firſt ſet, he obſerves, occaſion immediate death, but produce no viſible change in the blood; they occaſion the animal's death, merely by depriving him of air, in the ſame manner as were he immerſed in water: the only gaſes belonging to this claſs are hidrogen and azot. The ſecond ſet occaſion immediate death alſo, but at the ſame time produce certain alterations in the blood; and therefore kill, not only by depriving the animal of air, but by certain ſpecific properties: the gaſes belonging to this claſs are, carburetted hydrogen, ſulphuretted hydrogen, carbonic oxide, and perhaps alſo nitrous gas. The third ſet of gaſes may be breathed for ſome time without injury, but death enſues at laſt, provided their action be long enough continued: to this claſs belong the nitrous oxide and oxigen gas. The fourth ſet may be breathed any length of time without injury: the only gaſeous body belonging to this claſs, is the air of the atmoſphere, that compound of oxigen and azot every where ſurrounding the globe.

This compound fluid loſes its oxigen by that proceſs, and carbonic acid gas is produced.

After an animal has breathed a certain quantity of air for a given time, it becomes totally unfit for reſpiration; and if the air thus reſpired be chemically examined, we ſhall find that the oxigen is greatly diminiſhed, and that a conſiderable quantity of carbonic acid gas has been produced.

It appears from a number of experiments made by Dr. Hales, Dr. Menzies, and Mr. Davy, that the number of reſpirations made in a given time, as well as the quantity of air taken into the lungs, are liable to conſiderable variations in different people. Some have calculated the number of reſpirations at 14 only in a minute, others at 20; Mr. Davy informs us that he makes 26 or 27 in a minute; but having frequently endeavoured to count the reſpirations made by different people, in a given time, and without their knowledge, I have found them vary from 18 to 26 in a minute, moſt commonly, however, 20 or 21; and 21 in a minute make 30,240 in 24 hours.

The number of reſpirations made in a minute vary in different perſons from 18 to 26; but are commonly about 20.

The quantity of air taken in at each reſpiration, muſt be in proportion to the ſize of the perſon and the capacity of his lungs. About 41 cubic inches of air are taken in at every natural inſpiration.

Each reſpiration being about 41 cubic inches of air.

We now come to conſider the changes which are produced, both in the air and blood, by reſpiration. 1. On the changes effected in the air. Dr. Prieſtley, M. Lavoiſier, and Mr. Davy, have furniſhed us with many intereſting and inſtructive experiments on this ſubject. The changes are, 1. That a portion of the air diſappears; 2. That the air expired differs from that firſt taken into the lungs, in containing carbonic acid, and water in the ſtate of vapour. Dr. Menzies has ſhewn, that $\frac{1}{36}$th of the air inſpired diſappears in the lungs, and the experiments of Lavoiſier, which were made with much preciſion, differ but little from the above ſtatement. I never knew the quantity of air which diſappears to be leſs than $\frac{1}{19}$th part of the whole taken into the lungs; this may however be liable to variation in different people.

Phenomena of reſpiration.

Some air diſappears; about one-nineteenth; and the expired air is altered.

It has hitherto been ſuppoſed that the portion of air which diſappears, conſiſts of the oxigen only: Mr. Davy has, however, given ſome very ſtrong reaſons for ſuppoſing that part of the azote likewiſe diſappears during reſpiration. He ſuppoſes that the average quantity of air which is abſorbed at every reſpiration, amounts to 1.4 cubic inches, of which 0.2 are azot and 1.2 oxigen.

The abſorbed air was thought to be oxigen only: but appears to be in part azot.

Lime-water detects carbonic acid gas in the air emitted from the lungs, and the quantity of this gas may eaſily be eſtimated, by receiving the air expired into a graduated glaſs jar, ſtanding over mercury; a little cauſtic ſoda being introduced, the abſorption which takes place denotes the quantity

The emitted air contains carbonic acid;

about one cubic inch each expiration.

But this varies.

of carbonic acid. Lavoiſier has eſtimated the quantity of this gas thrown out from the lungs in 24 hours, at about 15.5 ounces troy. Mr. Davy makes the quantity thrown out in the ſame time, amount to 37 ounces, which is about 1 cubic inch at every expiration[*]. The quantity however varies from 0.5 cubic inch to 1.5 at different times in the ſame perſon, ſo that this accounts for the great variation in the above-mentioned experiments. Moreover the proportion varies, in the ſame individual, during the 24 hours; for I have found the quantity of carbonic acid gas emitted from my own lungs, to be rather leſs in the morning than towards the evening; but this alſo varies in different people.

Aqueous vapour alſo emitted;

But watery vapour is alſo emitted in reſpiration, the greateſt part of which is probably given off by the exhalent arteries, which are ſo copiouſly diſperſed on the ſurface of the lungs. A part is alſo emitted from the blood in the pulmonary veſſels.

in quantity variable.

The eſtimation of its quantity is attended with ſome difficulty; acccording to Dr. Hales it amounts in a day to 20 ounces: this is however but of little conſequence, for it is liable to much alteration.

The blood undergoes important changes during reſpiration.

It abſorbs air; becomes florid red; emits carbonic acid; and water; and perhaps hydrogen.

Various theories.

More important changes however than thoſe juſt mentioned are produced by reſpiration, namely, the alterations produced in the blood; which fluid, returning from every part of the body by the veins, is poured into the heart; from whence, being propelled through the lungs, it is brought into contact with the air, undergoing certain changes which render it fit for the nouriſhment and ſupport of the body. Theſe changes, which are of a very complicated nature, have engaged the attention of ſeveral learned and ingenious philoſophers. Dr. Thomſon has enumerated them as follows: 1. The blood abſorbs air. 2. It acquires a florid red colour, and the chyle diſappears. 3. It emits carbonic acid, and perhaps carbon. 4. It emits water, and perhaps hydrogen. Dr. Prieſtley, M. Lavoiſier, and Lagrange, have each adopted a different theory, by which they endeavour to explain and account for theſe changes produced by reſpiration: they are all however liable to conſiderable objections, and reſt merely on the ſuppoſition that the oxigen is alone abſorbed. Now Mr. Davy has ſhewn, that at leaſt a portion of the azot diſappears in the lungs: he has even rendered it very probable that the air is not decompoſed, but that it is abſorbed unaltered by the blood; that it is decompoſed during circulation; and that the uſeleſs portion of azot is again given out. The following facts are in ſupport of this opinion: "When the gaſeous oxide of azot is reſpired, its quantity is diminiſhed, carbonic acid gas is evolved as uſual, and a quantity of azot makes its appearance. Now as this azot did not exiſt ſeparately, it muſt have been produced by the decompoſition of the gaſeous oxide of azot; but its quantity being much leſs than the azot contained in the oxide of azot which had diſappeared, it follows that a part of this laſt gas had been abſorbed unaltered; and if a part, why not the whole? In that caſe the azotic gas muſt have been ſeparated from the blood by the ſubſequent decompoſition of the oxide of azot abſorbed *." Atmoſpheric air is compoſed of exactly the ſame ingredients as the oxide of azot, merely in different proportions, and in a ſtate of leſs intimate chemical combination. It is moreover natural to aſk, that if oxigen were alone abſorbed by the blood, why ſhould it not anſwer the ſame purpoſes as air? It is well known that this gas cannot be reſpired for a length of time without producing fatal conſequences; but even when it is reſpired, the quantity (of oxigen) which diſappears is much ſmaller than when a like quantity of atmoſpheric air is breathed for the ſame time. Mr. Davy has given the following experiment in proof of this fact: He breathed 182 cubic inches of oxigen gas for half a minute, 11.4 c. inches diſappeared; whereas when the experiment was repeated under the ſame circumſtances with atmoſpheric air, the quantity abſorbed amounted to 15.6 cubic inches.

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[*] Davy's Reſearches, page 433.

Mr. Davy apprehends that azot is abſorbed with the oxigen, &c.

Facts from the reſpiration of oxide of azot.

Oxigen alone is not proper for reſpiration:

Much leſs is abſorbed than when a like quantity of atmoſpheric air is breathed.

It was firſt obſerved by Lower, that the colour of venous blood, which is dark reddiſh purple, was converted into the florid ſcarlet colour of arterial blood, in its paſſage through the lungs. The phenomena of reſpiration, however, ſtill remained unexplained, until Dr. Prieſtley publiſhed his experiments on the changes produced in venous and arterial blood when put in contact with certain gaſeous bodies. "† He obſerves, that having introduced pieces of the craſſamentum of

Lower firſt obſerved the change of colour in venous blood by reſpiration.

Prieſtley ſhewed the cauſe; viz. the abſorption of oxigen.

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* Thomſon's Syſtem of Chemiſtry, Vol. IV. page 719.

† Prieſtley on Air, Vol. III. page 71.

coagulated ſheep's blood into dephlogiſticated air (oxigen gas); the blackeſt parts aſſumed a florid red colour, and that more readily than they would have done if common air only had been made uſe of: Whereas the brighteſt red blood became preſently black in any kind of air unfit for reſpiration, as in fixed air, &c.; and after having become black in phlogiſticated air (azot), it regained its red colour on being brought into contact with common air, the ſame blood becoming alternately black and ſcarlet, by being transferred from phlogiſticated into dephlogiſticated air, and *vice verſâ.*"

Theſe then may be regarded as the experiments which gave origin to all ſubſequent enquiries.

Deſcription of the proceſs of nutrition.

The food which is taken into the body is converted into chyle and excrement *; the former of which is abſorbed by a ſet of veſſels termed lacteals, which convey their fluid into the thoracic duct. The term lymph has been applied to that fluid which lubricates the ſurfaces of all the circumſcribed cavities of the body: This fluid is abſorbed by a ſet of veſſels termed lymphatics, which of courſe originate in every part of the body; they likewiſe terminate in the thoracic duct, which therefore is the great reſervoir of the abſorbent ſyſtem; it receives the chyle and lymph, and conveys them to the blood; they are here decompoſed, and converted into new ſubſtances neceſſary to the ſupport of the body.

Formation of fibrina.

Now the coagulable lymph, or fibrina, appears to be the moſt eſſential part of the blood, for it is employed to ſupply the waſte of the muſcles, &c. and Dr. Thomſon has accounted for its formation in the following mannner †: "It follows," ſays he, "from the experiments of Fourcroy, that fibrina contains more azot and leſs hydrogen and carbon than any of the ingredients of the blood, and conſequently alſo than any of the ingredients of the chyle. In what manner the chyle, or a part of it, is converted into fibrina, it is impoſſible to ſay: We are not ſufficiently acquainted with the ſubject to be able to explain the proceſs. But we can ſee at leaſt, that carbon and hydrogen muſt be abſtracted from that part of the chyle which is to be converted into fibrina, and we know that theſe ſubſtances are actually thrown out in reſpiration. We may conclude then that *one* uſe of the air abſorbed is to abſtract a quantity of carbon and hydrogen from a part of the chyle by compound affinity, in ſuch a manner that the remainder becomes fibrina: Therefore one end of reſpiration is to form fibrina.

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* The food, on being received into the ſtomach, is converted into a pulpy ſubſtance termed chyme. This alteration is effected by a peculiar fluid called *gaſtric juice*, which is ſecreted by the internal coats of the ſtomach. The chyme thus formed is propelled into the duodenum, where it meets with the bile, which converts it into a fluid much reſembling milk, termed chyle, and into excrement.

† Thomſon's Chemiſtry, 2d Edit. Vol. IV. page 725.

Uſe of reſpiration in that connexion.

It appears then, from the above-mentioned facts, that the perfection of the blood is almoſt totally dependent on reſpiration; whenever therefore this function is ſuſpended but for a very ſhort time, death is the conſequence.

Life cannot ſubſiſt without it.

It is well known that all the more perfect animals poſſeſs a temperature conſiderably higher than the ſurrounding atmoſphere: the cauſe however of this increaſed temperature, remained unexplained for a conſiderable time. At length Dr. Black's theory of latent heat became known, when ſeveral attempts were made to explain the cauſe of the increaſe of temperature, or ſtandard heat of the body, but none of them were ſatisfactory. Dr. Thomſon has however given us the following ingenious theory: As the air is abſorbed unaltered by the blood, it is evident that it will give out the greateſt portion of its caloric during circulation; that portion therefore which is emitted at the inſtant that the air combines with the blood, is united to the carbonic acid, converting it into the ſtate of gas, and the water into vapour. It appears moreover, that the heat of the blood is ſomewhat raiſed during circulation; for Mr. John Hunter found that the blood in the heart was a degree higher than in any other part of the body.

The elevated temperature of the more perfect animals is cauſed by the combination and condenſation of air.

From the facts which have now been alluded to, it appears that the following changes are produced by reſpiration: The blood is propelled, by the contraction of the heart, into the pulmonary artery, which, by its numerous ramifications, conveys the blood into the ſmall branches of the air-cells of the lungs, which are of ſo fine a texture as to admit the abſorption of a portion of air. The blood having undergone this alteration, is returned into the heart by the pulmonary veins, from whence it is circulated over the whole body. During the circulation, the air which has been abſorbed undergoes a gradual decompoſition; carbonic acid and water are formed, which, together

Recapitulation.

together with a portion of azot, are returned by the veins, and thrown out as the blood paſſes through the lungs. A freſh portion of air is at the ſame time abſorbed, and the above changes repeated.

Theſe then are the effects of reſpiration, as far as we are at preſent acquainted with them; but this important branch of phyſiology ſtill remains in conſiderable obſcurity.

Arlington Street,
April 29, 1805.

WILLIAM BRANDE.

IV.

Inſtruction on the Proceſſes diſcovered by M. BRALLE, *of Amiens, for watering Hemp in Two Hours Time, in all Seaſons, without injuring its Quality* *. *Publiſhed by Order of the Miniſter of the Interior of France.*

Experiments on hemp, by order of the French government.

IN the month of Fructidor in the year XI. (September 1803.) the government called to Paris M. Bralle, of Amiens, the inventor of new proceſſes for watering hemp. This diſcovery, which is intereſting to agriculture, manufactures, commerce, and the marine, had engaged its attention; orders were given to make the experiments requiſite to aſcertain its value.

Every thing which could elucidate the principles and practice of M. Bralle's proceſſes, which could prove and inſure their ſucceſs, was put in practice. Numerous and varied trials were made in the preſence of M. M. Monge and Berthollet, ſenators, and Teiſſier, member of the Inſtitute. M. Molard, adminiſtrator of the conſervatory of arts and manufactures, directed theſe trials, and carefully purſued them for ſix months. The reſults were equal to the expectations that had been formed.

From the account rendered to his imperial Majeſty, it was judged that the knowledge of a more expeditious method of watering hemp, than thoſe employed at preſent, which is practicable at all ſeaſons, and is in no reſpect injurious to health, by means of which a greater produce can be obtained from an equal quantity of the materials; and, which muſt extend and multiply the culture of an extremely valuable plant, could not be too extenſively publiſhed. In conformity to this deſire, we ſhall briefly deſcribe M. Bralle's proceſſes, relate the experiments which have been made, and offer ſome obſervations on the utility and advantages which are promiſed by this new diſcovery.

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* From Bibliotheque Phyſico Economique, Brumaire, An. XIII.

§ I.

The Proceſſes of M. Bralle.

M. Bralle's proceſs. The hemp is ſteeped in hot water with ſoap.

The means uſed by M. Bralle for watering hemp, are

1ſt. Water is heated in a veſſel to the temperature of from 72° to 75° of Reaumur's thermometer; (200° fahr.)

2nd. A quantity of green ſoap (*ſavon verd*) is added proportional to the quantity of the hemp to be ſteeped.

3rd. The hemp is then immerſed ſo that it ſhall be covered by the fluid, after which the veſſel is cloſed, and the fire put out.

4th. The hemp is left in this ſtate of maceration for two hours, and then taken out.

Proportions of the articles.

The weight of ſoap required for a complete ſteeping, is to that of hemp-ſtalks as 1 to 48; and the weight of the hemp to that of the water as 48 to 650.

The proceſs may be repeated with other hemp in the ſame water.

Several ſteepings may be made one after the other. It is ſufficient, before each new immerſion, to add a quantity of ſoap water to replace what was abſorbed by the preceding, and to raiſe the temperature of the bath to the above degree. The ſame water may be thus employed for fifteen ſucceſſive days.

When the bundles are taken out of the ſteeping veſſel, they are covered with a layer of ſtraw, that they may cool gradually, without loſing their humidity.

Breaking the ſtalk,

Next day they are ſpread on a floor, puſhing the bands towards the top of the ſtems, and a roller of ſtone or wood, loaded with a weight, is paſſed ſeveral times over them, to cruſh them, and diſpoſe the tow to be eaſily ſeparated from the reed, which is effected by beating. Whether the hemp be wet or dry, it peels completely in either ſtate.

beating.

After having tied the handfulls of the tow peeled off while wet, at the top, they are ſpread on the graſs, turned, and, after five or ſix days, carried to the warehouſe.

The handfulls of ſteeped and cruſhed hemp which are intended to be beaten and ſtripped dry muſt alſo be expoſed on the graſs; this operation being abſolutely neceſſary to whiten the tow, and facilitate the ſeparation of the reed.

§ II.

Recapitulation of the Experiments.

The experiments were varied to aſcertain the requiſite

By means of a portable ſteeping veſſel, different quantities of hemp were ſteeped, the temperature of the ſoapy liquor was varied at pleaſure, and the ſtate of the hemp was obſerved during the courſe of each operation, of which the duration was more or leſs prolonged, in order to aſcertain;

Temperature,

1ſt. The temperature which the ſoapy liquor ought to have before the immerſion of the hemp;

time, and

2nd. The time neceſſary for a complete ſteeping, at a determinate temperature;

proportion of ſoap.

3rd. The quantity of ſoap abſolutely neceſſary for a given weight of hemp-ſtalks, weighed before the immerſion, &c.

Reſults or general obſervations on the proceſs.

From a great number of experiments made in the months of January, February, and March laſt, it was found,

1ſt. That water containing the quantity of green ſoap directed by M. Bralle, for a given weight of hemp, effects the ſteeping completely;

2nd. That the ſteeping is ſo much the more ſpeedy as the temperature of the fluid is nearer to ebullition, at the time of the immerſion of the hemp;

3rd. That if the hemp be kept more than two hours in the ſteeping veſſel, the time preſcribed by M. Bralle for obtaining a complete watering, the tow ſeparates equally well from the reed, but it acquires a deeper colour, and loſes part of its ſtrength;

4th. That if the hemp be immerſed in a cold ſoapy liquor, and heat be then applied, the ſteeping is not accompliſhed ſo perfectly, whatever degree of temperature may be given to the liquor, and however long the immerſion may be continued*;

5th. That the bundles of hemp immerſed and kept vertically in the veſſel, are ſteeped more uniformly than if they were laid horizontally; and this poſition alſo facilitates the manipulation.

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* Probably the fluid between the fibres is not heated, becauſe its conducting power is bad, and it is prevented from circulating. N.

§ III.

§ III.

Obſervations on the Utility and Advantages of the New Diſcovery.

Deſcription of the methods of ſteeping hemp, as heretofore practiſed.

Expoſure on the graſs.

Two methods only of ſteeping hemp are generally practiſed. The firſt conſiſts in ſpreading the plant on the graſs, and turning it two or three times a week, until the air, the light, the dews, or the rains, have diſpoſed the tow to ſeparate eaſily from the reed. The reſult is obtained in a longer or ſhorter time, according to the weather and the ſtate of the air; and frequently, in certain countries, the operation is not finiſhed in leſs than forty days.

2. Steeping.

The ſecond conſiſts in immerſing the bundles of hemp in rivers, brooks, ditches, or pools, and keeping them there for eight, fifteen, twenty, or even thirty days, according to the degree of the heat of the water, or of the atmoſphere.

Theſe proceſſes very defective.

The maceration effected by both theſe proceſſes is frequently incomplete, and always unequal. By following the firſt, the cultivator is liable to have his crop diſperſed by the winds, or injured by long rains: if he adopts the ſecond, he runs the riſk of loſing a part of it by the overflowing of the rivers, or of its being covered with mud. The firſt method in particular, is liable to the ſerious inconvenience of depriving the national marine of part of the hemp produced by our territory: it is known that the tow produced from the hemp which has been expoſed on the graſs is not uſed by the government.

Superiority of the new proceſs.

The ſteeping of hemp according to M. Bralle's proceſs, requires only a copper cylindrical veſſel, placed on a ſmall furnace of bricks.

A ſteeping veſſel of this kind, containing 240 litres of water, (52 ale gallons) is ſufficient to ſteep 18 kilogrammes of hemp-ſtalks, (about 40lb.) and as the operation is completed in two hours, 100 kilogrammes (221lb.) may be eaſily ſteeped in a day.

This method appears to deſerve the preference over the former ones, on many accounts.

It is practicable all the year.

1ſt. The ſteeping is practicable all the year, except during very hard froſts, when it is difficult to dry the hemp. But when it is to be peeled wet, the cold is no longer an obſtacle; it is then only neceſſary to take proper precautions for preventing the tow from freezing in its humid ſtate.

ſaves time,

2nd. The time of ſteeping being only two hours, affords a ſaving of time of great value to the cultivator, particularly during the ſeaſon of harveſt.

and is not injurious to health.

3rd. The workman has no cauſe to fear any injury of his health: it is ſufficient to keep up a current of air while the bundles are plunged into and taken out of the ſteeping veſſel; the handfulls of ſtalks or tow, which are afterwards expoſed on the graſs, do not emit any bad ſmell, or vitiate the purity of the air, whatever may be the quantity of hemp dried at once in the ſame place.

Every one knows, that when the bundles of hemp ſteeped in water in the old method, are taken out and waſhed, they emit an infectious odour which becomes inſupportable during the heats, and to which ſerious diſorders are aſcribed. The valley of the department of the Somme, and many others in which hemp is ſteeped, afford too convincing proofs. The waters are rendered unfit for the uſe of cattle, and the fiſh contained in them are frequently deſtroyed.

Apparatus on a larger ſcale.

To accelerate the operation of ſteeping by the new proceſs, in countries where there is an extenſive culture of it, inſtead of the portable ſteeping veſſel which was made uſe of in the experiments, the following apparatus may be adopted, conſiſting of a boiler and four wooden tubs, ſerving for ſteeping veſſels.

After having heated the ſoapy matter to ebullition, it is ſuffered to flow through a cock, into two of theſe tubs filled with bundles of hemp, and cloſed by a cover: while the ſteeping is going on in the two firſt tubs, the neceſſary quantity of liquor is heated, to be conveyed into the other two, which are alſo filled with bundles of hemp, and cloſed with lids.

By means of this very ſimple apparatus, a conſiderable quantity of hemp may be ſteeped in a day without interruption.

Comparative expence of the two proceſſes.

4th. The expence of ſteeping in water, compared with that required by the method of M. Bralle, is nearly the ſame, when the ſmall ſteeping veſſel is made uſe of; but if a cauldron, rather large, and the ſteeping tubs which have been mentioned are employed, the coſt will be diminiſhed more than a half.

In fact, the expence of the firſt includes the conveyance of the hemp to be ſteeped, the time employed in forming the bundles of hemp into a ſort of rafts, that they may be ſunk by loading them with ſtones, turf, clods of earth, and even mud; in fixing and ſecuring theſe rafts by driving in ſtakes; a tedious work, and the more troubleſome, becauſe 10 kilogrammes of hemp-ſtalks cannot be immerſed without a weight of 15 or 20 kilogrammes, and, after the ſteeping, all this maſs muſt be removed, to take the bundles out of the water, and waſh them.

The coſt of the new proceſs conſiſts principally in the price of the ſolvent made uſe of, which amounts to about eight centimes for a kilogramme of tow. To this ſhould be added the price of combuſtible neceſſary for heating the liquor, if this combuſtible was not afforded by the reeds of the bundles, whether they are peeled wet or dry.

At an equal expence, the new proceſs is ſtill preferable to the old, becauſe, from what has been ſaid, it renders the manipulation more expeditious and more eaſy.

Comparative produce.

5th. Eight kilogrammes of hemp-ſtalks ſteeped by the new proceſs, commonly produce two kilogrammes of pure tow, by peeling when wet; whereas hemp ſteeped in water by the old proceſs, and beaten, does not yield more from eight kilogrammes than one and a half.

The dry peeling of hemp ſteeped in the old way does not produce the ſame quantity as that which is peeled when wet: the breaking of the reed in many places occaſions a greater loſs of tow.

The hemp being waſhed, beaten and combed in the old method, a kilogramme of long tow is obtained from four kilogrammes of the rough tow; the remainder is ſhort ſtuff, hards and duſt.

The ſame quantity of hemp, manipulated in the new way, yields two kilogrammes of long tow, one kilogramme of ſecond tow, and about a kilogramme of ſhort ſtuff and hards.

Thus from eight kilogrammes of hemp-ſtalks, two kilogrammes are obtained in rough tow by the new proceſs, and from this quantity is obtained one killogramme of the firſt tow, which does not exiſt in any known manipulation.

Extenſion of the culture of hemp.

6th. The inhabitants of the banks of rivers and of the valleys, are almoſt the only perſons who cultivate hemp; they owe this privilege to the vicinity of the waters, and the humidity of the ſoil. By the new proceſs the culture of hemp will be extended to all places, and procure a new and very advantageous occupation to the inhabitants of the plains, the land of which is much more vegetative than that of the marſhes.

It is an error to ſuppoſe that hemp cannot grow to a great height in the plains; it is a fact, that it riſes to the height of two yards, in land which has been well ploughed and manured, when mild rains have promoted germinanation and growth.

It is equally a fact, that there is every where a ſufficient quantity of ſpring or ciſtern water to ſteep the hemp by the new proceſs: if droughts ſhould ſupervene, which beſides are only accidental, the ſteeping may be deferred.

It will, therefore, be poſſible to cultivate hemp in the plains, and in low lands, which are always rich and fertile, though frequently without ſprings of water, and to augment not only the maſs of our products, but alſo our riches of this deſcription, ſince one acre of good hemp yields as much profit as two acres of wheat.

Summary of the advantages ariſing from this proceſs.

Such are the effects which may be expected from M. Bralle's new method of ſteeping hemp. It is, as was obſerved at the commencement of this inſtruction, more expeditious than thoſe hitherto employed; it perfectly completes the ſteeping; it may be uſed at all ſeaſons; it does not affect the purity of the air; from an equal quantity of materials, it procures a more abundant produce; and laſtly, it is well calculated to extend the cultivation of the plant itſelf. The enlightened lovers of agriculture, and well-informed proprietors, who live upon and cultivate their own eſtates, without being ſlaves to the cuſtomary practices, will adopt it, and ſecure its advantages, by repeating the experiments which have aſcertained its merit, and alſo by making trials on a more extenſive ſcale than thoſe which took place in the conſervatory of arts and manufactures. Their example will be followed, the proceſs of M. Bralle will be extended, and we ſhall ſee portable ſteeping veſſels, ſimilar to thoſe uſed by M. Molard, multiplied; a cheap apparatus which requires very little repairs, and by means of which the hemp grown through the extent of one or of ſeveral communes may be ſteeped even in the field on which it grew.

V.

Deſcription of a Portable Steam Engine. By Mr. MATTHEW MURRAY.

To Mr. NICHOLSON.

SIR,

I TAKE the liberty of handing you the deſcription of a portable ſteam engine of my conſtruction, which you will have the goodneſs to inſert in your Philoſophical Journal. I will juſt obſerve it is reduced to the feweſt parts that practical utility will admit, which muſt neceſſarily render it of great advantage; as the ſimplicity of its parts make it nearly impoſſible to be out of order with a very moderate degree of management. The following deſcription and reference to the plate will explain the nature of this engine.

I am, Sir,
Your much obliged humble ſervant,
MATTHEW MURRAY.

Leeds, May 7th, 1805.

Deſcription of a Portable Steam Engine.——Plate VII.

AA Repreſents the ground or floor on which the engine ſtands.

Deſcription of a portable ſteam engine.

B Section of a receſs made in the ground for the beam O to work in.

C. Iron ciſtern reſting upon the ground or floor covering the receſs for the beam.

D An opening in the floor to admit a boy to oil the centers of the beam.

E A double ſteam cylinder, having an upright pipe in the intermediate ſpace, which effects a communication between the top and bottom and the valve box G.

F A ſteam pipe that communicates with the boiler through which all the ſteam paſſes and ſurrounds the inner cylinder in its way to the valve box, prior to its application againſt the piſton.

G The valve box fixed upon a projection from the cylinder bottom, having an opening or connection with the interval between the two cylinders. In this opening is a regulating valve

Deſcription of a portable ſteam engine.

valve for adjuſting the quantity of ſteam (that acts againſt the piſton) in its paſſage through the valve box. There are alſo three other openings in the bottom of this valve box, one of which connects with the top of the cylinder by the pipe in the intermediate ſpace, the ſecond with the bottom, and the third with the eduction pipe that leads to the condenſor. Two of theſe openings are alternately connected together by a ſlide valve,* while the third is left open for the admiſſion of ſteam to the piſton, this valve changes its poſition at the end of each ſtroke of the piſton, and performs all the purpoſes of the moſt complicated machine.

H The air-pump connected with a condenſer at the bottom of the eduction pipe.

I. The fly wheel fixed upon an axis which receives its motion from a crank connected with the beam by the rod K.

L L Two rods for connecting the motion of the piſton to the beam, theſe rods move perpendicularly by a motion which could not be conveniently ſhewn in this view without rendering it confuſed.

M A ſpherical triangle turn'd by the crank for moving the ſlide valve by the horizontal rod N that connects them together. This motion has the advantage of preventing the engine from ever turning the contrary way round from that which it is wanted to go, and prevents the noiſe that is uſually heard in engines.

O The beam attached to the bottom of the ciſtern C by means of the hanging carriages P.

Q A reſt or fixture in a wall for the end of the fly wheel ſhaft; this will vary according to the ſituation where the engine is to be fixed, or it may be ſupported by a metal ſtandard.

R. Index to the injection cock that admits water to the condenſer. *Note,* The ciſtern is to be kept nearly full of water during the time the engine is at work.

The cylinder G and valve box E muſt be ſurrounded on all ſides by a caſe (not ſhewn in this view) the ſpace between filled with charcoal to prevent the tranſmiſſion of heat, which if effectually done will work with the leaſt poſſible quantity of coals, as it combines the advantages of every other engine hitherto known. By detaching the air-pump and condenſer

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* For theſe valves I took out a patent in 1802.

(which may be done in half an hour) and where water cannot be had for condenſation, this engine may be worked by the preſſure of ſtrong ſteam alone, as the internal cylinder is kept as hot as the ſteam in the boiler.* This dangerous plan never ought to be reſorted to but in caſes of neceſſity, as it is no ſaving of coals, and as there can be no certain rule when to diſcontinue the uſe of the boiler, the weakneſs of which is not prevented by putting the fire in a tube in the inſide of it. This engine requires no framing nor mill-wright work in the fixing, but merely bolting down to the floor it ſtands upon. It takes up very little room, and all its parts are within reach, without the neceſſity of upper floors or ſtages, which would be the caſe if the beam was above; but by being fixed below and alone, it has no tendency to move from its ſituation.

Deſcription of a portable ſteam engine.

VI.

Letter from Mr. J. C. HORNBLOWER, *Engineer, on the Meaſure of Force by Horſe Powers.*

To Mr. NICHOLSON.

DEAR SIR,

I AM induced to trouble you on account of the preſent unſettled ſtate of things reſpecting what is uſually called *the power of a horſe*. I do not know why a matter of this ſort ſhould remain ſo diſregarded, eſpecially as it has ſo long become one of our data, comprehending the unities of weight, ſpace and time, by which we are to be underſtood in our communications on the ſubject, and by which we are to aſcertain the pre-

Uncertainty of what is meant by the power of a horſe.

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* Many engines are at preſent worked in London and elſewhere by the mere force of ſteam, without condenſation, under Trevithick's patent. The force is from 45 to 60 lb. on the round inch; a preſſure equal to about 25 fathoms of water at the moſt. Various aſſertions and reports concerning the ſafety, the economy, and the other effects of theſe engines have paſſed under my notice; but the intereſted ſituation of ſome of the narrators on both ſides, and the ſhort time of trial, have induced me to wait for more facts before I ſhould give any account of the engine in this Journal. I hope to do this a few months hence.—W. N.

ciſe value or effect of any mill or engine in and about London. Indeed I do not know why it was adopted for the purpoſes intended, it being ſo indefinite.

This unity probably aroſe from ſteam engines being ſubſtituted for horſes.

I can eaſily conceive it probable that ſomebody who has employed horſes for ſome time in mill-work, may have applied to an engineer, and ſaid "I have a mind to have my work done by a ſteam engine inſtead of horſes, for I am to a point that I ſhall ſave money by it, and pleaſe to give me an eſtimate of the coſt of an engine that will do the work of my horſes;" and then the engineer ſets about getting information as to what may be deemed the effect a horſe can produce, and calls it the *horſe power;* and perhaps having Deſagulier's Experimental Philoſophy at hand, applies to him, and there he finds that a horſe will raiſe a hogſhead of water 50 feet high in a minute; then what is the weight of a hogſhead of water, and he finds from ſome particulars related by him, 2nd vol. page 505, that a hogſhead of water is equal to 550 lb. but of what meaſure is uncertain, for the ale hogſhead, 51 gallons, is 540 lb. and the wine hogſhead, 63 gallons, is 504 lb. reckoning the cubic foot at 1000 ounces avoirdupois.

Deſaguliers conſiders it as 550 lb. raiſed 50 feet per minute.

Another eſtimate; nearly double that of Deſaguliers.

Some engineers who have very unceremoniouſly taken the lead in this affair, have adopted I do not know what for a *datum*, but the reſult is this: An engine by calculating 10 lb. on the ſquare inch, making the whole preſſure = 1000, moving through 200 feet per minute, is called a *four-horſe engine.*— Let us ſee then what will be the effect of one horſe according to this fact.

$1000^{\text{lb.}} \times 200^{\text{ft.}} = 200000 =$ the whole effect, then $\frac{200000}{4}$ = the effect of one horſe. Now compare this with the eſtimate of Dr. Deſaguliers, which is a hogſhead of water at 550 lb. 50 feet high in a minute; 550 lb. × 50 ft. $= 27500$ and ; $50000 - 27500 = 22500$ = the difference between one of Mr. Watt's horſes and one of the doctor's, on the former of which I make no comment.

Smeaton's eſtimate one-fifth leſs than Deſaguliers.

Mr. Smeaton, whom I hold as having ſuperior claim to precedency on ſubjects of this nature, has utterly diſapproved of Deſagulier's experiment by the moſt powerful conviction of its fallibility, formed by concluſions drawn from ſterling experience in the accompliſhment of works on a large ſcale; and he ſtates the greateſt effect to be 40 feet high in a minute; but as this is ſtill in the commonly received opinion as to the weight of the hogſhead, I would rather turn to thoſe who have made their experiments on a weight of ſolid matter, expreſſed in terms which cannot be miſtaken.

Mention of experiments on the reaction againſt horſes, by the late Sam. More, Eſq. with an inſtrument. *It was a ſpring with graduation.*

I remember to have had ſome converſation on this ſubject many years ago, with the late Samuel More, at that time ſecretary to the Society for the Encouragement of Arts, &c. when he ſhewed an inſtrument conſtructed on purpoſe to determine the reſiſtance againſt horſes at plough. I do not recollect that I made any minutes on the reſult of our converſation, ſo can only ſay that his relation of the fact ſurpriſed me, until I came to compare it with the effect of horſes actually applied to overcome a load drawn up a ſhaft in a mine; but I had not the ſame means of determining the re-action that he had; however, the reſult of his experiment may be ſeen in the Tranſactions of that Society; ſome obſervations on it may be ſeen in the 3d vol. quarto, of the Philoſophical Journal, page 136, only there ſeems to be a miſtake in the deduction in the note: it ſhould be 264 : 10 : : 1375 : 52 +

Propoſed experiment.

I much wiſh to have an experiment like Mr. More's made by a ſledge drawn forwards and backwards on a level road, with Mr. More's inſtrument placed between it and the horſes; ſuch an experiment would be very practicable, and the ſmall deviation from the true level of a road would be compenſated by alternately going firſt one way and then the other.

Uncertainty of the meaſure of a horſe power from grinding or pumping, &c.

It is true that we are become pretty well acquainted with what may be done by horſes in grinding malt, pumping liquor and worts in breweries; but there are ſo many fortuitous circumſtances to be regarded, even here, that nothing decided can reſult from the cloſeſt inveſtigation. For inſtance, ſome brewers chuſe to have their malt ground much lower than others; the pump-work is executed in ſome breweries under very different advantages, and from local circumſtances may be retarded by the inertia neceſſary to communicate motion from the wheel to the work, adding the different condition of valves, buckets, &c. All theſe conſiderations demand ſome *invariable* reſiſtance to be overcome by the exertion of the horſe, and I know of nothing ſo appropriate as that I have juſt mentioned.

Difficulty of this ſubject, not only from the work, but the

The power of a horſe (by which I mean the mechanic power) is not eaſily aſcertained. It has ſcarce any analogy with claſs of horſes employed.

with a weight deſcending through a given ſpace or a quantity of water falling a given height, and therefore is better expreſſed by the terms effect, reſiſtance, re-action, &c. and even then, to be any thing like preciſe, we ought to diſcriminate whether brewers' horſes, or higlers' horſes, waggon horſes or coach horſes, heavy horſes or light, and if you will go into the country among the coal-mines, you will have another claſs of theſe animals, which I know not what to call unleſs it be *poor horſes*, full worked and half ſtarved; in ſhort, I mean that neither one or the other ought to be taken into the account as the *meaſure of a mechanic power*.

Mr. More's eſtimate of 80lb. 3 miles an hour. This is nearly three fourths of Deſagulier's rate.

Nevertheleſs it ſeems deſirable to have ſome popular expreſſion for the application of whatever may be ſubſtituted in the place of horſes, whether ſteam, water or wind; nor can there be any objection to ſaying, " equal to the work of ſo many horſes," provided we can attain to a clear, unequivocal and ſomewhat exact value, attributable to that power, and if I may give my own opinion, I think Mr. More has ſtated the utmoſt effect to be 80 lb. three miles per hour, in ſuch horſes as are proper for giving motion to mill-work, and at ſuch ſpells as will not exhauſt the breath or ſtrength of the animal.

Remarks.

I am ſurpriſed to find this mode of calculation has obtained ſo far as to determine the power engines employed purely as pumping engines, as lately at the Tunnel, the New Docks, &c. but I am glad it reaches no further than the bills of mortality, and I hear that the Dutch method of hoiſting goods to warehouſes has lately been adopted at ſome of our new docks. *O tempora, O mores!* While other countries are availing themſelves of the application of the ſteam engine in place of animal labour, we are taking up the expedients of thoſe who have ſcarce heard there is any ſuch thing as a ſteam engine, or who cannot appreciate its value on that degree of evidence we have in our own country.

Whether the horſe power or unity be true or not, it ought ſurely to be free from ambiguity.

It may be objected to by ſome to alter the preſent data, however erroneous, as we ſhall be obliged to require 20 horſe engines inſtead of 10 (for it appears the eſtimate is nearly, if not quite *cent. per cent.* more than it ſhould be); but even that can make no difference in any reſpect than as making a rent in an egregious error; the coſt of an engine cannot be altered by it, nor the conſumption of fuel, but a material convenience would be the reſult of ſuch a regulation, conſidering the advantage of a coincidence in this point throughout the kingdom, and as partaking of the nature of a unity of weights and meaſures, it ought to be paramount to every ſubordinate conſideration.*

Your much obliged obedient ſervant,

J. C. HORNBLOWER.

VII.

Letter from Mr. A. F. THOELDEN, *communicating three manuſcript Tables from Mr.* BODE, *of Berlin, of the geocentric Places of the new Planets Ceres, Pallas, and Juno, for twelve Months to come.*

To Mr. NICHOLSON.

SIR,

THE three planets, or aſteroids (according to Dr. Herſchel), lately diſcovered, being ſo very ſmall, are not eaſily found, unleſs the obſerver is acquainted with the place where he is to look for them. This uncertainty induced ſome aſtronomical gentlemen to deſire me to inquire, if there were not any ephemeris of their motions publiſhed abroad. I complied with their requeſt, and Mr. Bode has very obligingly communicated to me the following written account of their reſpective ſituations, calculated for the Obſervatory at Berlin, (that of Juno, according to a table of his own calculation). If you think this communication may be intereſting to the aſtronomical readers of your Philoſophical Journal, I beg you will make uſe of it.

Tables obtained from Mr. Bode of the places of the new planets.

I am, Sir,

Your moſt obedient humble ſervant,

A. F. THOELDEN.

10, *St. Alban's Street,*
May 13, 1805.

P. S. A new edition of Mr. Bode's ſmall Celeſtial Atlas has juſt been publiſhed; with a Catalogue of 6500 Stars, after Piazzi's obſervations. This laſt work can be had ſeparate.

Any gentleman who may be deſirous of one or both theſe works, will be ſupplied in a reaſonable time after ſending an order to me.

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* For a very clear and rational report of a ſteam-engine in horſe-powers, ſee our Journal, IX. p. 215.—W. N.

TABLE I.

GEOCENTRIC MOTION OF CERES.

1805.	A. R.	Decl.	1806.	A. R.	Decl.
Oct. 2.	104° 34′	22° 51′N.	Jan. 9.	103° 26′	29° 46′N.
11.	106 53	23 3	18.	101 11	30 25
20.	108 49	23 18	27.	99 18	30 53
29.	110 21	23 39	Feb. 5.	97 56	31 11
Nov. 7.	111 24	24 5	14.	97 13	31 22
16.	111 54	24 40	23.	97 10	31 26
25.	111 49	25 22	Mar. 4.	97 46	31 25
Dec. 4.	111 7	26 12	13.	98 58	31 20
13.	109 48	27 7	22.	100 43	31 11
22.	107 59	28 3	31.	102 57	30 58
31.	105 46	28 58	Apr. 9.	105 32	30 40
			18.	108 28	30 18
			27.	111 40	29 51
			May 6.	115 6	29 19
			15.	118 42	28 40
			24.	122 27	27 56

TABLE

TABLE II.

GEOCENTRIC MOTION OF PALLAS.

1805.	A. R.	Decl.	1806.	A. R.	Decl.
Aug. 3.	59° 29′	3° 4′ S.	Jan. 3.	68° 34′	31° 16′ S.
12.	62 47	4 22	12.	67 51	29 27
21.	65 54	5 56	21.	67 48	27 12
30.	68 52	7 47	30.	68 24	24 39
Sep. 8.	71 34	9 53	Feb. 8.	69 37	21 54
17.	73 58	12 15	17.	71 26	19 3
26.	75 59	14 50	26.	73 45	16 10
Oct. 5.	77 35	17 37	Mar. 7.	76 33	13 20
14.	78 40	20 30	16.	79 44	10 35
23.	79 12	23 23	25.	83 16	7 59
Nov. 1.	79 8	26 8	Apr. 3.	87 6	5 34
10.	78 26	28 38	12.	91 11	3 21
19.	77 11	30 43	21.	95 28	1 22
28.	75 29	32 13	30.	99 57	0 19 N.
Dec. 7.	73 33	33 3			
16.	71 35	33 9			
25.	69 51	32 32			

TABLE III.

GEOCENTRIC MOTION OF JUNO.

1805.	Longitude.			Latitude.		1806.	Longitude.			Latitude.	
Oct. 1.	4ˢ	28°	22′	6°	21′ S.	Jan. 1.	5ˢ	26°	39′	4°	18′ S.
11.	5	2	28	6	12	11.	5	27	25	3	56
21.	5	6	18	6	1	21.	5	27	29	3	31
Nov. 1.	5	10	23	5	47	Feb. 1.	5	26	45	3	0
11.	5	13	54	5	33	11.	5	25	19	2	22
21.	5	16	44	5	21	21.	5	23	20	1	44
Dec. 1.	5	19	23	5	8	Mar. 1.	5	21	33	1	15
11.	5	21	34	4	54	11.	5	19	0	0	33
21.	5	23	27	4	38	21.	5	16	41	0	10N.
						Apr. 1.	5	14	27	0	50
						11.	5	12	56	1	26
						21.	5	12	5	1	57
						May 1.	5	11	46	2	19

VIII.

Letter from Profeſſor Pini, *Inſpector of Mines to the Italian Republic, to* J. C. Delametherie, *on Corindon found in Italy.*[*]

Beautiful red adamantine ſpar found in Italy.

THE intereſt you take in publiſhing diſcoveries in natural hiſtory in your excellent Journal, induces me to communicate to you a mineralogical rarity lately found on a mountain of the Italian republic: it is a very fine corindon, or adamantine ſpar, of a deep ruby colour. I ſaw it for the firſt time among the minerals which the learned Brochi, profeſſor of natural hiſtory at Breſcia, had made a ſhort time before in the department of Serio. At the firſt view he conſidered it as a feldſpar, of which it has all the appearance; and, in fact, the corindon being a ſubſtance which hitherto has only been furniſhed by countries far diſtant from us, it would have been imprudent to have judged otherwiſe at firſt: but the colour of ſtone, exactly reſembling that of a red corindon which I brought from Paris, given to me as coming from Madras, led me to ſuppoſe that it did not differ from it.

But as Profeſſor Brochi purpoſed meeting me in a ſhort time at Milan, to which place I was going, we poſtponed the verification of this ſuſpicion. When he ſaw the red corindon from Madras, in my poſſeſſion, he no longer doubted the identity of its ſpecies with that of our ſample. I afterwards diſcovered the ſame identity in the trials to which I ſubmitted it: the following are the reſults:

Examination. 1. Hardneſs. 2. Infuſibility. 3. Laminar texture. 4. Fiſſure and angle. 5. Reflection of light. 6. Sp. gravity.

1ſt. The corindon of Italy ſcratches the hardeſt rock-cryſtal. 2d. It does not melt before the blow-pipe, either alone or with the addition of borax. 3. Its texture is in laminæ, which follow different directions. 4th. Its fiſſure is triple, and when it is cut in the three directions, it offers a rhomboid, the acute angle of which is $64\frac{1}{3}^{\circ}$. 5th. Its croſs fracture ſhews the ſplendor of the diamond, and reflects the light, the flaſhes of which are almoſt the colour of ſilver, 6th. Its ſpecific gravity is 3.87, which is the mean of that of the true corindon.

Is found on a mountain of micaceous ſchiſtus.

Hitherto it has been met with in a mountain of micaceous ſchiſtus, in pieces of ſeveral inches in length, which are amorphous and opake, but ſemi-tranſparent on the thin edges. Profeſſor Brochi and I purpoſe making new reſearches there, which may lead to ſome more intereſting diſcovery.

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[*] From Journal de Phyſique, Vendemiaire, An XIII.

Countries where corindon is found.

In the mean time it will no longer be doubted, that the corindon is a product of Europe. M. de Bournon, with whoſe memoir on corindon, inſerted in the Journal des Mines, Vol. XIV. you are well acquainted, has noticed the different countries which furniſh this ſubſtance; they are the iſland of Ceylon, the peninſula of India, and in particular Madras, the Carnatic, and China. He concludes his intereſting details by enquiring whether this ſubſtance exiſts in other countries, except thoſe acknowledged to be the chief, if not the excluſive ſituations of this ſpecies. This queſtion ariſes from ſeveral ſtones found in Europe having been given as corindons. In fact, thoſe collected in Germany were found to be ſometimes feldſpars, and ſometimes the *ſchorlartiger-beryll* of Werner, your leucolite. That mentioned in the Muſeum Brittanicum as coming from Tyrie, on the eaſtern coaſt of Scotland, was far from having the hardneſs belonging to this ſpecies; that from Cheſnut-hill near Philadelphia, announced by Mr. Smith, was diſcovered by Mr. Richard Phillips to be a fragment of badly cryſtallized quartz. It only remained to decide on the feldſpar found by Bournon in France, in the province of Forez, the deſcription of which he ſent to you in a letter inſerted in the Journal de Phyſique for June 1789, and which he ſtill conſiders as a true corindon.

The ſpecimens of European ſtones called corindon do not belong to that ſpecies.

This ſubſtance appears to be the ſame as that you have called andalouſite, and which ſome dealers in natural hiſtory have circulated in commerce by the name of adamantine ſpar, from the kingdom of Caſtile: it has been placed by Abbé Hauy in the appendix, which contains thoſe ſubſtances, the nature of which did not appear to be ſufficiently known to permit him to aſſign them a place in his method. He calls it apyrous feldſpar. Thus we may be ſatisfied that hitherto there is no certainty of corindon having been found in Europe. I flatter myſelf that now there will be no doubt on the corindon of Italy which I have the honour to announce; it is not even deficient in the ſpecific gravity belonging to this ſubſtance; a defect which induced Profeſſor Hauy not to acknowledge the feldſpar of Forez to be a corindon.

Probability that Europe may afford the precious gems.

If rubies and ſapphires be claſſed with corindon, as many mineralogiſts ſeem diſpoſed to do, Europe will probably have mines of theſe precious ſtones as it already has mines of emeralds, ſuch as thoſe of Limoges, diſcovered by the learned Lelievre, counſellor of the mines. But theſe ſtones will be rubies, ſapphires, and emeralds of mineralogiſts, but not of the jewellers, until they ſhall be found very tranſparent, which I hope will be the fruit of new reſearches, followed with perſeverance.

HERMENEGILDE PINI.

IX.

On diſcloſing the Proceſs of Manufactories. In a Letter from Mr. JOHN CLENNELL.

To Mr. NICHOLSON.

MY DEAR SIR, *Newcaſtle, Feb.* 17, 1805.

Intereſting queſtion reſpecting the diſcloſure of manufacturing proceſſes.

PERMIT me to intreat the attention of ſome of your numerous correſpondents towards a queſtion which muſt certainly be intereſting to every manufacturer, but of which no regular diſcuſſion has yet been offered—Is it proper or improper to lay before the public in reſpectable periodical works, a full and impartial ſtatement of the various proceſſes of our manufactories? I ſhall ſtate ſuch reaſons as have offered themſelves to me why they ſhould be diſplayed, but I am principally anxious to receive further information on a ſubject that appears to me peculiarly intereſting.

Argument of *Boyle*,

The firſt argument I ſhall adduce is that of Mr. Boyle, as quoted by Dr. Johnſon in the 201ſt number of the Rambler. "The excellency of manufactures, and the facility of labour, would be much promoted, if the various expedients and contrivances which lie concealed in private hands, were, by reciprocal communications, made generally known; for there are few operations that are not performed by one or another with ſome peculiar advantages, which, though *ſingly* of little importance, would, by *conjunction and concurrence*, open new inlets to knowledge, and give new powers to diligence."

The

confirmed by experience.

The ſecond is the very conſiderable improvements that have taken place in thoſe few manufactories which have yet been under the influence of chemical enquiry; thus realizing, but on a very extenſive ſcale, the ſuggeſtions of Mr. Boyle: ſo far therefore as we are to be guided on the one hand by experience, and on the other by the influence of ſcientific enquiry on liberal diſplay, will the argument be in our favour.

Accidental diſcoveries improved by diſcloſure.

In the third place I would obſerve, that as many very valuable diſcoveries are owing to accident, thoſe with whom they happen are frequently perhaps incapable of improving them to the extent they would admit of in the hands of men of ſcience, and thus, by a ſpirit of monopoly, preclude even themſelves from the advantageous cultivation of ſuch diſcoveries, merely leſt others might enjoy it alſo. If, again, we conſider the rapid progreſs that has been made of late years in every department of uſeful and practical knowledge, we muſt attribute it entirely to thoſe liberal communications that have been made by men whoſe attention has been immediately directed to the promotion and improvement of every thing valuable to the public. Again, the profits of every buſineſs depend on the regularity and knowledge with which it is conducted; but how is the laſt to be enjoyed without reſources to apply to, and how much more eaſily would it be obtained if ſcience could regulate and ſimplify the combinations of the manufacturer? If to accompliſh by every thing employed (and even in many caſes the refuſe) in each proceſs its utmoſt poſſible uſe, is a favourite principle with manufacturers; to take the moſt accurate and beſt adapted means to effect it, ought certainly to be as powerful with them. Is it not alſo obvious, that, to diſcard all myſtery and quackery, and fairly to diſcloſe each proceſs, is to invite the attention of men of ſcience and reſearch to extend any advantages gained by chance or otherwiſe, and to diſcover greater utility in the various ſubſtances employed. The origin, progreſs; preſent ſtate, and hints for the improvement of the "arts of life," would certainly be worthy the contemplation of our firſt chemiſts, and are ſubjects that have appeared of ſuch importance to a neighbouring nation, that many of their moſt eminent men have been employed in them; and ſome volumes of the *Encyclopedie Methodique* are dedicated to ſuch information, with plates too, in ſeveral inſtances diſplaying even the moſt minute work-tools employed in each.

Science would be thus introduced into workſhops, &c.

The

Some objections anſwered.

The hiſtory and detail of manufactories conducted in each place ought, I preſume, to form a principal object with the writers of local hiſtories; yet very few of thoſe gentlemen are enabled to obtain ſuch accounts as they can depend on, from the ſelfiſh and monopolizing jealouſy of manufacturers in general. To theſe various advantages an objection may be offered, "That diſplay is placing objects of taxation in the hands of miniſters: be it ſo; diſplay will make it eaſier to collect the tax, will make it more certain, and it may be, *leſs oppreſſive:* if to theſe be added the above advantages, it may fairly be preſumed, that diſcovery and conſequent improvement is the moſt advantageous track to be purſued; but, my dear Sir, I beg your pardon, on this ſubject I did not mean to offer my own opinion ſo much as to ſolicit information from that of others.

I am truly your's,

JOHN CLENNELL.

Literary purſuits are compatible with the duties of merchants and manufacturers.

How far literary purſuits are compatible with the duties of the commercial man, or the manufacturer, ſeems a queſtion ſo completely decided in the affirmative, in the firſt volume of the Mancheſter Memoirs, by Mr. Henry; in the ſecond volume of the ſame work, by Dr. Barnes; and in the hundredth number of the Lounger,—that the above paper aſſumes the principle as being fully eſtabliſhed. J. C.

X.

Queſtion reſpecting the Purification of Copper. By J. P. *With a conciſe Reply.* W. N.

Queſtion reſpecting the purification of copper.

As copper in its pureſt ſtate (eſpecially out of London) for manufacturing different articles, cannot be obtained without a very tedious proceſs; as it forms the principal ingredient in mirrors for reflecting teleſcopes, and likewiſe is much uſed as an alloy for gold; if it is impure, it never fails to render the gold ſo alloyed brittle, and not to be reſtored to its ductility until the impure alloy be waſted by ſubſequent meltings, to the loſs and diſappointment of the workman. Required, therefore, a method of purifying the copper, particularly for the latter purpoſe?

Your's reſpectfully,

Morley Street, J. P. *Jun.*
Newcaſtle-upon-Tyne.

Reply.

As the proceſſes for refining copper in the large way are grounded upon its property of reſiſting oxidation more than the other metals which are uſually combined with it, it may be adviſeable to adopt the proceſs of Pelletier, with a due attention to the manipulation and the proportion of manganeſe to be made uſe of. The very intereſting letter of Mr. Thomſon in the preſent Number, will indicate the principles of operation. Mr. Hatchett's excellent papers in the Philoſophical Tranſactions, of which a correct abridgement is given in our V. and VI. Volumes, ſhew the miſchievous conſequences of impurity in the copper for alloying the precious metals; and it is but too well known, that it is difficult to be procured, or even to be made pure, upon a ſcale of extenſive magnitude.

W. N.

XI.

Deſcription of a Method of connecting Iron Bars, and coating them with Lead, ſo as to form ſolid Pillars for Light-houſes on Rocks covered at High-water, and to defend them from Corroſion. By Capt. JOSEPH BRODIE, *of the Royal Navy.**

Deſcription of the means of coating iron bars: by reference to the drawing.

FIG. 1, *Plate V.* A ſhews four rods of caſt iron, compoſed of a number of pieces two feet long, rivetted together, in a manner explained by the plate, ſo as to produce the effect of one bar of the thickneſs of the whole. B. A tube of caſt iron, formed from a number of ſeparate pieces, each about ten inches long, and which, when placed round the iron rods above-mentioned, and then ſcrewed together, form a mould, into which the melted lead is to be poured, to coat the iron rods. C. A portion of the rods covered with the melted lead, ſo as to form a cylindrical pillar apparently of lead, the iron being perfectly coated therewith.

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* Communicated to the Society of Arts (Memoirs, MDCCCIV. 258.) who voted him the gold medal.

Fig. 2. D ſhews the manner in which the hollow cylinder is formed to any length required, by the junction of a number of ſemi-cylinders rivetted together and fitting each other. E, the ſide flanges ſcrewed cloſe together. F, the end flanges alſo ſcrewed together, as prepared for the melted lead.

After a certain portion of the iron rods are coated with lead, the lower parts of the tube are taken off and placed higher up; by which repeated changes, a few tubes will anſwer the purpoſe to coat any length of the iron rods.

XII.

Reply to Mr. ACCUM'S *laſt Letter on the Production of Nitrous Acid. By* W. F. S.*

To Mr. NICHOLSON.

SIR,

Remarks on Mr. Accum's letter.

I THINK Mr. Accum by no means throws off the charge of cenſure with which he is accuſed, upon ſo material a part of the modern theory. He ought to have been more explicit, and in ſo nice and ſo diſputed an experiment, to have given us a minute detail.

Unſucceſsful attempts to repeat the experiment of forming nitrous acid.

I have performed the experiment which he ſpeaks of, but I could detect no nitrous acid after the proceſs. The air gradually diminiſhes by the electric ſpark, but this diminution is owing to the oxigen gas producing a calcination of the metals employed, for the pureſt oxigen gas anſwers better than when mixed with nitrogen. Therefore I hope, if Mr. Accum poſſeſſes a more accurate experiment, he will give it through your Journal: certainly the preſent ſtate of modern chemiſtry requires a very minute inveſtigation. I hope, Mr. Nicholſon, you will not refuſe inſerting this in your Journal.

London, May 2.

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* See our Vol. X. p. 109 and 214.

XIII.

Experiments on the Electricity of Metallic Filings ſifted through Metal; with Remarks in anſwer to a Letter of Mr. Cuthbertſon. By *Mr.* Wm. Wilson. *(From the Author.)*

To Mr. NICHOLSON.

SIR,

An error pointed out by Mr. Cuthbertſon admitted.

IN the laſt number of your Philoſophical Journal, I find a letter from Mr. Cuthbertſon, containing ſome remarks on my letter on the electricity of metals, in which he notices an error in the table of reſults of experiments, that had eſcaped my notice till I read his letter. In that table I have by miſtake put P againſt copper filings ſifted through zinc inſtead of N.

Copper filings when ſifted through zinc are electrified ſtrongly with negative electricity.

Mr. C.'s objection to the production of electricity by mere ſeparation of metals, conſidered.

The metallic filings *touch* the ſieve, are *ſeparated*, and *touch* the receiving metal.

As the el. varies with the ſieve it does not depend on the *latter contact*,

—and in the *firſt contact* there is no inſulation,

—conſequently it depends on the *ſeparation*.

Mr. C. in his other remarks does not ſeem to admit that the ſeparating the metals from contact is the cauſe of the electrical fluid being excited and not touching, becauſe both touching and ſeparating are employed. In the way the experiments were made touching takes place two ways, viz. the filings come in contact with the metal plate they are ſifted into, and they are in contact with the ſieve before they are ſifted through it; but I cannot conceive how either of theſe contacts could cauſe the excitation of the electricity in the experiments. If the contact of the filings with the plate they were ſifted into was the cauſe, we ſhould have had the ſame effect with the ſame filings, whatever metal the ſieve was made of they were ſifted through; becauſe as they were always ſifted into the ſame plate, the ſame metal filings always came in contact with the ſame metal plate, and conſequently we ſhould have had the ſame effects in all caſes with the ſame filings; whereas out of the ten ſorts of filings that were uſed there was only one (ſteel) that produced the ſame effect with the different ſieves; and if the contact of the filings with the ſieve excited any electric fluid, it would be diſſipated as faſt as excited, becauſe neither the ſieve nor the filings were inſulated; conſequently it could have no effect on the reſults of the experiments.—Therefore ſince neither of theſe contacts excited the fluid, it muſt have been excited by the ſeparation.

I think the following experiments will put this beyond a doubt: 1. I faſtened a piece of card into a ſtick of glaſs, and then rubbed it over with ſtrong gum water and covered it with filings of zinc; ſo that when it was dry, it had a ſurface of filings of zinc. From a heap of the ſame filings I took up as much as I could on this little ſhovel without touching it with any thing elſe, and let them fall very ſlowly upon a piece of bright ſheet copper faſtened in an inclined poſition upon the cap of an electrometer, and formed into a receptacle at its lower part to contain the filings. In this operation almoſt every particle of filings neceſſarily came in contact with the face of the copper. After letting fall upon the copper about an ounce and a half of filings there was not the leaſt ſenſible effect on the electrometer. 2. I then took a piece of the ſame ſheet copper, which was pierced full of ſmall holes, and ſifted through it the ſame filings I had uſed in the above experiment, upon the ſame copper on the electrometer, and the gold leaves diverged with poſitive electricity and diſcharged themſelves againſt the ſlips of tin foil on the inſide of the glaſs ten times before the whole of the ounce and half of filings were ſifted into it.

An inſulated ſhovel of card, faced with zinc filings was uſed to pour zinc filings on a bright plate of copper.

The copper plate was not electrified.

A copper ſieve was then uſed inſtead of the ſhovel.

The copper plate was ſtrongly poſitive.

Now, ſince the ſeparating the two metals from contact in the ſecond of the above experiments is the only difference between them, and as the electric fluid was excited only in the ſecond, I think we may ſafely conclude that that ſeparation was the cauſe of the excitation, and not touching.

Separation was alone the cauſe of this electricity.

I am very much inclined to believe that the excitation that takes place in friction is cauſed by the ſame circumſtance, and that the friction does nothing more towards the excitation than bring the different parts of the ſubſtance that are rubbed together into contact, and ſeparate them from it.

Electric excitation is probably a fact of this kind.

I am your obedient humble ſervant,

WILLIAM WILSON.

Reply

XIV.

Reply to Mr. Boſwell. By An Old Correspondent.

To Mr. NICHOLSON.

SIR,

Explanatory remarks on Mr. Boſwell's letter.

WHEN I began to read your correſpondent, Mr. Boſwell's anſwer to my obſervations on his geometrical propoſitions, and found myſelf accuſed of having "thrown ſome very undeſerved reflections" on his communication, I could not help feeling a degree of apprehenſion leſt I ſhould inadvertently have made ſome miſtake or other, for which I muſt have been obliged to apologize to him and to the public; but on peruſing the letter through, I was not a little ſurpriſed to have found but *one* reflection pointed out, and that applying not to the *matter* but *manner* of my obſervations; it ſeems I have accuſed him of being *too confident* in one of his aſſertions; a literary crime, of which he exculpates himſelf by proving from a quotation, certainly very much to the purpoſe if we make no diſtinction between *doubt* and *diffidence*, that he is, on the contrary, a very *diffident writer*, which quotation, to be ſure, contraſted with an expreſſion that fell from my pen, might be concluſive, if the two paſſages were applicable to the *ſame thing*; but unfortunately it turns out, on cloſer examination, that the accuſation applies *excluſively* to Mr. B's *ſecond* propoſition, and his exculpatory quotation *excluſively* to his *firſt*.

Had I committed myſelf ſo far as to ſay that he announced his firſt propoſition with confidence, I muſt have ſtood clearly convicted of having done him injuſtice; but as it was his ſecond propoſition, or, in other words, his *other fact* in geometry, to which my objectionable obſervations *ſolely* applied, he ought to have quoted what he has ſaid about *it*, and about *it only*, as evidence againſt me; he has, however, directed his arrow at a wrong mark, on which account I claim the reader's indulgence to repeat the paſſage alluded to, which is the *only* paſſage in Mr. B's paper that relates to his ſecond propoſition: "The diſcovery of a fact in geometry often leads to *another*; one of this kind *I have here to add, which is,* that a right line (B E) drawn from the extremity B of the line I B, at right angles through the oppoſite diameter (I F) to the circumference, Explanatory remarks on Mr. Boſwell's letter.

ence, *will be equal* to a fourth of the circumference." In this innunciation, which is an appendage to the main ſubject or firſt propoſition, and evidently not included in the prefatory apology, I ſee nothing like a ſuſpicion of inaccuracy expreſſed, or even hinted at, and conſequently can trace no mark of *diffidence*, now that I come to examine it again; on the contrary, I repeat, that a fact is confidently aſſerted to exiſt, which has been proved *not to be a fact*, both by myſelf and by Mr. Gough, to whoſe teſtimony probably the reader will pay ſome deference, particularly as this gentleman has ingeniouſly ſhown that *another line* in the circle poſſeſſes the identical property erroneouſly attributed to the line B E, and that previouſly to Mr. B.'s complaint being made public: to the reader, therefore, the falſeneſs or juſtice of my obſervation or "reflection" muſt be referred; and it will anſwer the purpoſe of both Mr. B. and myſelf, if he will let the affair drop here; for *he* will then ſtand a good chance of being reputed what is his principal aim, a *diffident writer*, and I ſhall ceaſe to be, what ſome of your readers, beſides Mr. B. may poſſibly be diſpoſed to think me, a *caviller*.

I am, Sir, once more,
AN OLD CORRESPONDENT.

May 17, 1805.

P. S. The mathematical portion of your readers need not be informed, that the *forgetfulneſs* imputed to me, reſpecting the aſſumed *approximation* ſubſtituted for the *exact ratio* of the diameter to the circumference of a circle, is a charge applying with equal propriety to every mathematician who has deduced calculations depending on the area of a circle: even Mr. Gough, whoſe mathematical ſkill is juſtly the admiration of thouſands, whom he has never ſeen, and is doomed never to ſee, has ſomehow been *obliged* to be guilty of the ſame want of recollection, though his calculations may be conſidered as the reſult of *demonſtrative truth*.

It remains now for Mr. Boſwell to ſhow the ſcientific world, by an example or two, how he applies his diſcovery to practical meaſurements, which it is preſumed, will prove a communication of general intereſt.

XV.

Deſcription of an accurate Method of banking the Balance of a Time-keeper. By Mr. WILLIAM HARDY. *Extracted from his Letter to* CHAS. TAYLOR, *Eſq. Sec. to the Society of Arts.**

SIR,

Importance of the ſtop or banking piece uſed to prevent extreme vibrations in time-pieces.

THIS letter is accompanied with a drawing, a deſcription, and a model, of a more perfect mode of banking the balance of a time-keeper, than any that has yet appeared; and its application to a time-keeper is a matter of ſuch real importance, that the moſt accurate, without this moſt neceſſary appendage, is liable to ſuch derangement, that from the moſt trivial cauſe it is in one moment rendered uſeleſs.

The author's invention is confirmed by long trial,

and teſtimony.

To preſerve the good qualities of the time-keeper, on which often the ſtrength, the wealth, the grandeur, and ſafety of this great empire depend, I deem it neceſſary that my invention ſhould be laid before the Society of Arts, as the means of its being more generally known; and I hope that I ſhew proper reſpect to the Society, when I aſſure you that I do not offer any crude idea, neither could I think of giving you any trouble, until I had fully verified the utility of my contrivance by ſeveral years trial. As I can produce the teſtimony of ſome of the moſt eminent watchmakers in favour of my invention, I look forward with ſome degree of confidence, in expectation of obtaining the approbation of the Society.

The banking is required in watches which have a vibration through very great arcs.

It was at firſt imagined, that a banking to a watch with a free eſcapement was quite unneceſſary, as the limits of banking were ſo great as to admit of almoſt twice 360, or 720 degrees; but, on trial, the balance was frequently found to exceed this quantity, and that a very ſlight motion given to the time-keeper (particularly when the axis of the balance became the axis of that motion), was ſufficient to alter the ſtrength and figure of the pendulum-ſpring, and poſition of the pieces in reſpect of the balance-wheel, ſo as to change the rate of the time-keeper; and, what was worſe, require a new adjuſtment of the balance, to accommodate itſelf to the changes made in the ſpring, and other parts connected with it. Hence it became neceſſary, that ſome means ſhould be uſed to ſtop the balance at certain limits beyond its natural arch of vibration; and various attempts have been made to effect it. One way is, by a moveable piece on the axis of the balance, which banks againſt a pin, yet ſo as to ſuffer the balance to vibrate more than 360 degrees. Another method is to have a piece moveable on a centre in one of the arms of the balance, and applying itſelf as a tangent to the pendulum-ſpring, which paſſes through a hole in the piece. It has alſo a knee, which almoſt touches the plate, and juſt paſſes free of a pin placed in it. But when the balance vibrates ſo as to approach its utmoſt limits, the action of the ſpring, while in a ſtate of unwinding, throws the piece outward, ſo as to fall in the way of the pin, and ſtop the balance from proceeding farther. Another mode is by a ſtraight ſpring, ſcrewed upon the plate, having a hook at the end of it, into which a pin placed in the balance ſtrikes, when, as before, the pendulum-ſpring, in unwinding, touches the ſtraight ſpring, and moves it a little outwards. There is alſo a way of banking by means of a bolt, which is thrown back by the pendulum-ſpring, and made to fall in the way of a pin placed in the rim of the balance. Theſe are the principal modes of banking now in uſe, and they do not differ materially from one another in principle. But the weight and friction of ſo many pieces, on ſo delicate an organ as that of a pendulum-ſpring, are perhaps nearly as hurtful to the time-keeper as the injury it may ſuſtain when it is left without any banking whatever.

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* In their Memoirs for 1804. A premium of 30 guineas was awarded for this invention.

Former methods of banking.

They are objected to from weight and friction.

In *Figures* 1 and 2, *Plate VI.* the ſame letters are placed, to ſignify the ſame things. A A is the balance to which the pendulum-ſpring is faſtened in the uſual way. In one of the croſſes of the balance is placed a pin P, which ſtands a little way above its ſurface; and when the balance is cauſed to vibrate a complete circle, the pin in its motion will deſcribe the dotted circle P O Q, and juſt paſs clear of the inſide of a projection formed on a cock B, which is faſtened on the plate by means of a ſcrew. At about one-fourth of a turn of the pendulum-ſpring, reckoned from its ſtud E, is placed a very delicate tapering piece of ſteel S, having a ſmall hole in

Deſcription, with reference to the engraving.

it,

Deſcription, with reference to the engraving.

it, through which the pendulum-ſpring paſſes; and it is faſtened to it by means of a pin, and ſtands perpendicular to the curve of the ſpring. Let the balance be at reſt, as repreſented in *Fig.* 1, the banking-pin at P, and the banking-piece at *s*. Suppoſe the balance is made to vibrate from P towards O, when P arrives at the banking-piece *s*, it will paſs it without touching, becauſe its extremity *s* lies wholly within the circle traced out by the banking-pin. But when the banking-pin P has arrived at Q, the banking-piece *s* will have advanced to *t*, by the pendulum-ſpring winding itſelf up into the figure repreſented by the dotted curve; and when the banking-pin P (now at Q) returns back to P, and paſſes on from P towards Q, to approach B, and ſo complete the other half-arch of its vibration, before P can arrive at the banking-cock B, the pendulum-ſpring will have unwound itſelf into the figure deſcribed by the dotted curve, and the banking-piece *s* will have advanced into the poſition at *r* juſt touching the banking-cock. Its extremity *r*, however, being thrown beyond the dotted circle, muſt neceſſarily fall in the way of the banking-pin, which arrives there almoſt at the ſame moment, and is oppoſed by it, without the ſlighteſt ſhock to the pendulum-ſpring. The model* renders any farther explanation unneceſſary.

WILLIAM HARDY.

No. 61, *Chapel-Street, near White-Conduit-Houſe,*
Jan. 18, 1804.

XVI.

Deſcription of a new Apparatus for making the gaſiform Oxide of Carbon. Communicated by Mr. DEYEUX.†

The progreſs of chemiſtry owing chiefly to improved apparatus.

IT is generally admitted, that chemiſtry is indebted to the invention of various kinds of apparatus, and the perfection to which they have been brought, for much of the progreſs it has made within theſe laſt thirty years.

Inſtance in that of Woulfe.

For inſtance, before Woulfe made known his apparatus for obtaining the aeriform fluids evolved from various ſubſtances, either when expoſed to the action of fire, or when brought into contact with matters capable of combining with them, the operator was obliged to employ bulky veſſels, difficult to manage, and ſo inconvenient, that he did not wiſh to apply them; and when he did, it was impoſſible for him to collect the aeriform fluids with any accuracy; for when they were rarefied to a conſiderable degree, they eſcaped through apertures left purpoſely to preſerve the veſſels from burſting.

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* Which is preſerved in the collection of the Society.

† Annales de Chimie, Vol. LIII. p. 76.

Inconvenience of the old inſtruments.

Contraſted advantages of the modern arrangement of veſſels.

Theſe inconveniences are now removed by Woulfe's apparatus, ſo that the operations in which gaſes are evolved may eaſily be performed in veſſels of ſmall bulk; the gaſes may be ſubjected to calculation; their quantity as well as quality aſcertained with the utmoſt preciſion; and operations, which were formerly conſidered as very hazardous to the operator, may now be continued for hours together without the leaſt fear of injury.

With theſe advantages many others are connected; and it is known to every one, that they are owing to the degree of perfection to which chemiſts have brought Woulfe's apparatus, and particularly to their happy application of it on various occaſions.

Improvements yet remain to be made.

Yet, notwithſtanding theſe diſcoveries have been carried a great way, it is more than probable that many remain to be made: too much praiſe therefore cannot be beſtowed on thoſe who turn their attention to this important object, ſince the apparatuſes they invent are ſo many new means afforded chemiſts of collecting an infinite number of products which frequently eſcape them, and the knowledge of which may have great influence on the improvement of chemical ſcience.

Invention of Mr. Baruel;

From theſe motives I have thought it may be of uſe to make known an apparatus juſt invented by Mr. Baruel, operator to the chemical lectures of the Medical School at Paris.

for decompoſing gaſes, or combining them with other ſubſtances.

This young chemiſt, who had often noticed the difficulties and even dangers incurred when it is neceſſary to decompoſe gaſes, or to combine them with different ſubſtances, attempted to make ſome alteration in the proceſſes commonly employed in the laboratory for operations of this ſort; and after ſeveral trials he invented an apparatus, which ſucceeded beyond his hopes.

Gaſiform oxide of carbon formerly procured with difficulty,

I have ſeen this apparatus employed with the greateſt ſucceſs for the fabrication of the gaſiform oxide of carbon. This is now eaſily made.

gas, which formerly was to be procured only with difficulty and in ſmall quantity, may now be obtained eaſily, readily, and without much expenſe, which affords the advantage of ſubjecting it to many more experiments than has hitherto been done.

Other uſes of the apparatus.

The apparatus in queſtion may be of uſe likewiſe for the preparation of ſulphurated hydrogen gas, carbonated hydrogen gas, and phoſphorated hydrogen gas: it may likewiſe be employed for ſaturating a gas with any ſubſtance whatever, particularly when a high temperature is requiſite for this ſaturation.

The better to make known the apparatus of Mr. Baruel, I will give the deſcription of it communicated to me by the author, and add a ſketch of it, which will give a more complete idea of all its parts. *Plate VIII,*

Deſcription of the apparatus, and method of procuring gaſiform oxide of carbon.

Suppoſe it is required to make gaſiform oxide of carbon: ſome charcoal is to be taken very dry and carefully choſen, broken into ſmall pieces, and introduced into the three gun-barrels, B, C, D. The charcoal is to be preſſed lightly together with an iron rod, ſo as to occupy only the part of each barrel that is to be heated, but it muſt not be rammed hard. The three gun-barrels are then to be placed horizontally ſide by ſide in a reverberatory furnace, A, leaving about two inches diſtance between them; ſecured in their places with moiſtened brick earth, and covered with the dome of the furnace.

This done, fix in the mouth of the barrel B the glaſs tube E, which is curved ſo as to admit its other extremity to be inſerted into the neck of the bottle F; the neck of this bottle being large enough to hold likewiſe the pipe of the curved funnel G. Into the oppoſite end of the barrel B one of the ends of the curved tube H is to be introduced, the other end of the tube being inſerted into the opening of the barrel D, ſo as to form a communication between B and D. A ſimilar tube I is fitted to the other extremity of D and the adjacent end of C, forming a communication between theſe two barrels. Laſtly, From the oppoſite extremity of the barrel C iſſues the tube K, which is bent at right angles, ſo that its ſecond curvature paſſes under the receiver M, placed on the ſhelf of the pneumatic tub with water L.

Every thing being thus arranged, carbonate of lime diluted with a ſmall quantity of water is to be poured into the bottle F, and after all the joints of the tubes have been luted with great care, a fire is to be kindled in the furnace. As ſoon as this fire is ſufficiently ſtrong to make the gun-barrels red-hot, ſulphuric acid is to be poured into the funnel G, and, when it comes into contact with the carbonate of lime in the bottle F, it will expel a large quantity of carbonic acid. This acid preſently paſſes through the tube E into the barrel B, is conveyed from B to D through the tube H, from D to C through the tube I, and thence iſſuing by the tube K, comes out beneath the receiver M, placed on the ſhelf of the pneumatic tub.

Deſcription of the apparatus, and method of procuring gaſiform oxide of carbon.

The object propoſed by Mr. Baruel in this arrangement of his apparatus, was to oblige the carbonic acid gas evolved from the carbonate of lime, to traverſe the charcoal contained in the three gun-barrels, and thus ſaturate itſelf with all the carbon it could take up.

In fact it is eaſy to conceive, that this method muſt be more certain and expeditious than that formerly employed, when the operator was ſatisfied with paſſing the gas through a ſingle barrel. It is true it was collected, and ſubjected to a ſecond operation, or even to a third; but this mode was tedious, and much of the gas was always loſt. In this new method on the contrary nothing is loſt, and a product is ſeparated at once, which poſſeſſes all the properties that characteriſe the gaſiform oxide of carbon, and which may be uſed unſparingly, ſince it is always obtainable in large quantity.

This method more certain and expeditious than the old;

for nothing is loſt, and the product is obtained at once.

XVII.

Deſcription of an improved Mill for levigating Painters Colours. By Mr. JAMES RAWLINSON, *of Derby.*[*]

THE hitherto very unmechanical, inconvenient, and highly injurious method of grinding poiſonous and noxious colours, led me firſt to imagine a better might eaſily be contrived for that purpoſe. It muſt be obvious to every perſon, that the method

Great inconveniences and unwholeſome effects of grinding colours on the ſtone.

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[*] From the Memoirs of the Society of Arts for 1804 who awarded him the ſilver medal.

method hitherto adopted of grinding colours on an horizontal marble ſlab, with a ſmall pebble muller, requires the body of the perſon who grinds to bend over that ſlab, and conſequently his head; which cauſes him conſtantly to inhale the noxious and poiſonous volatile parts of the paint, which is not unfrequently ground with oil ſaturated with litharge of lead; and if we may judge from the very unhealthy appearance of theſe men, accuſtomed to much colour-grinding, it ſhould ſeem the bad effects of this employment require a ſpeedy remedy.

Machine by which the work is much better performed, and without inconvenience.

The machine, of which I now ſend the Society a model, has not only the advantage of being an effectual remedy of this extenſive and ſevere evil to recommend it, but it grinds the colour much eaſier, much finer, and much quicker, than any method hitherto adopted. Having occaſion for a conſiderable quantity of colour-grinding in the profeſſion in which I am engaged, and that in the fineſt ſtate poſſible, and having made uſe of this machine for ſeveral years, and being more and more convinced of its utility, I thought it my duty to preſent it to the Society of Arts, hoping that it might not be altogether unworthy of their attention. The roller of the machine that I uſe is ſixteen inches and a half in diameter, and four inches and a half in breadth. The concave muller that it works againſt covers one-third of that roller: it is therefore evident, that with this machine I have ſeventy-two ſquare inches of the concave marble muller in conſtant work on the paint, and that I can bring the paint much oftener under this muller in a given ſpace of time, than I could by the uſual method with the pebble muller, which is ſeldom more than four inches in diameter, and conſequently has ſcarcely ſixteen ſquare inches at work on the paint, when my concave muller has ſeventy-two. I do not mean to ſay that a roller, the ſize of that which I now uſe, is the largeſt which might be employed; for truly I believe that a roller two feet in diameter, with a concave muller in proportion, would not be hard work for a man; and then the advantage to the public would be ſtill farther increaſed.

It works with five times the ſurface of the muller,

and is applicable to water-colours as well as thoſe in oil.

This machine will be found equally uſeful for the colours ground in water, as for thoſe ground in oils; and I doubt not but the great importance of this ſimple machine will be very ſoon generally experienced in all manufactories where colours are uſed. The labour neceſſary with this machine, in grinding

ing colours exceedingly fine, is very eaſy. It is uſeleſs to enter into any minute deſcription in this place, as a bare inſpection of the machine muſt ſufficiently explain itſelf.

To the colourman it would evidently be an eſſential ſaving of labour, and conſequently of expenſe, which will probably have ſome weight as a recommendation; and the advantages to the colour-grinder have been already ſtated.

Deſcription, by reference to the drawing.

Plate V. Fig. 3. A is a roller or cylinder made of any kind of marble; black marble is eſteemed the beſt, becauſe it is the hardeſt, and takes the beſt poliſh. B is a concave muller covering one-third of the roller, of the ſame kind of marble, and fixed in a wooden frame *b*, which is hung to the frame E at *i i*. C is a ſtrong piece of iron, about an inch broad, to keep the muller ſteady, and is fixed to the frame with a joint at *f*. The ſmall binding-ſcrew, with the fly nut, that paſſes through the centre of the iron-plate at *c*, is for the purpoſe of laying more preſſure on the muller, if required, as well as to keep it ſteady. D is a taker-off, made of a clock-ſpring about half an inch broad, and fixed in the manner of a frame-ſaw in an iron frame *k*, in an inclined poſition to the roller, and turning on pivots at *d d*. G is a ſlide-board to draw out occaſionally, to clean, &c. if any particles of paint ſhould fall from the roller, and which alſo forms itſelf for the plate H, to catch the colour on as it falls from the taker-off. F is a drawer, for the purpoſe of containing curriers ſhavings, which are the beſt things for cleaning paint-mills.—E is the frame.

Pulverization, or dry grinding, previous to levigation.

Previous to the colour being applied to the mill, I ſhould recommend it to be finely pulverized in a mortar, covered in the manner of the chemiſts when they levigate poiſonous drugs[*]. This proceſs of dry-grinding is equally neceſſary for the marble ſlab now in uſe; after which it ſhould be mixed with oil or water, and with a ſpatula or pallet-knife put on the roller, near to the top of the concave muller, and the roller turned round, which takes the colour under the muller without any difficulty, and very few turns of the roller ſpread it equally over its ſurface. When it is perceived ſufficiently fine for the purpoſe required, it is very eaſily taken off by means of the taker-off deſcribed, which muſt be held againſt the roller, and the roller turned the reverſe way, which cleans it very quick and very completely; and the muller will only require to be cleaned when you deſiſt or change the colour. It is then turned back, being hung on pivots to the frame at *i i*, and cleaned with a pallet-knife or ſpatula very conveniently. Afterwards, a handful of curriers ſhavings held on the roller, with two or three revolutions cleans it effectually; and there is leſs waſte with this machine than with any marble ſlab.

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[*] Or rather in an improved mill, uſed at Mancheſter by Mr. Charles Taylor, for grinding indigo in a dry ſtate, of which I have annexed a drawing, and reference, to render the whole buſineſs of colour-grinding complete.—*Note of the Author.*

This is the ſame apparatus as was uſed under the name of *a philoſophical mill*, in the laboratory at Gottorp, about the beginning of the laſt century. See the memoir of Dr. Joel Langelot, with an engraving, in Lowthorp's Abridgment of the Philoſophical Tranſactions, III. 318.—W. N.

Method of working.

Quantity and fineneſs of grinding.

As to the quantity ground at once on this mill, it muſt be regulated by the ſtate of fineneſs to which it is required to be ground. If it is wanted to be very fine, a ſmaller quantity muſt be put on the roller at a time; and as to time requiſite for grinding a given quantity of colour, this will alſo depend on the ſtate of fineneſs to which it is ground. I have obſerved that my colour-grinder has ground the quantity of colour which uſed to ſerve him per day, with this machine, in three hours, and, as he ſaid, with eaſe. The colour alſo was much more to my ſatisfaction than in the former way, and attended with leſs waſte.

I have mentioned the pulverizing the colours in a covered mortar, which would prevent waſte, and prevent the duſt and fineſt parts of noxious colours from being injurious to the grinder. In ſome manufactories, where large quantities of colours, prepared from lead, copper, and arſenic, are uſed, this precaution is particularly neceſſary. I do not mean to ſay that my machine is intended to ſuperſede the paint-mill now in uſe for coarſe common colours. It is intended for no ſuch purpoſe; but to ſuperſede the uſe of the very awkward and unmechanical marble ſlab now in uſe, and on which all the colours for china manufactories, coach-painters, japanners, and colour-manufacturers for artiſts, &c. &c. are now ground.

This mill is not a crude project, but has been uſed ſeveral years.

Several of the colour-manufacturers have expreſſed to me their great want of ſuch a machine; and that I had no deſire of troubling the public with a machine that would not anſwer, is evident, from my having uſed it ſeveral years before I preſumed to recommend it to their attention. Being therefore now completely convinced of its utility, and hoping that it might relieve a number of my fellow-creatures from a dangerous employment, I have ventured to commit it to the protection of the Society of Arts; hoping, through their means, to ſee its ultimate ſucceſs. And, farther to give the Society the moſt complete aſſurance in my power, I have annexed the opinion of a very ingenious and mechanical friend of mine who has frequently ſeen it work. If any other queſtions ſhould occur to the Committee, that may be in my power to explain, I ſhall gladly do ſo.

I am, Sir,
Your moſt obedient ſervant
JAMES RAWLINSON.

Derby, Feb. 6, 1804.
Charles Taylor, Eſq.

Inſtructions for tying up the bladders of colour.

P. S. When the colour is ground, I recommend the following mode of tying it up in bladders, in preference to the uſual method. Inſtead of drawing the neck of the bladder cloſe, in the act of tying it, inſert a ſlender cylindrical ſtick, and bind the bladder cloſe around it. This, when dry, will form a tube or pipe, through which, when the ſtick is withdrawn, the colour may be ſqueezed as wanted, and the neck again cloſed by replacing the ſtick. This is not only a neater and much more cleanly mode than the uſual one of perforating the bladder; and ſtopping the hole with a nail, or more commonly leaving it open, to the prejudice of the colour; but the bladder, being uninjured, may be uſed repeatedly for freſh quantities of colour.

N. B. The barrel of a quill may be tied, in place of the ſtick, into the neck of the bladder, with its cloſed end outwards, which will keep the colour ſecure in travelling, and when uſed, the end of the quill being cut off, it may afterwards be cloſed by a ſtick.*

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* A certificate from Mr. Thomas Swanwick, of Derby, and alſo from Mr. John Middleton, of St. Martin's Lane, confirming the above ſtatement, accompanied theſe papers.

Reference

Reference to the improved Mill for grinding Indigo, or other dry Colours.

Deſcription of the mill for dry grinding.

Plate V. Fig. 4. L repreſents a mortar made of marble or hard ſtone; one made in the common way will anſwer. M, A muller or grinder, nearly in the form of a pear, in the upper part of which an iron axis is firmly fixed, which axis, at the parts N N, turns in grooves or ſlits, cut in two pieces of oak projecting horizontally from a wall, and when the axis is at work, are ſecured in the grooves by iron pins, O O. P, the handle, which forms a part of the axis, and by which the grinder is worked. Q, the wall in which the oak pieces N N are fixed. R, a weight which may occaſionally be added, if more power is wanted.

Fig. 5. ſhews the muller or grinder, with its axis ſeparate from the other machinery; its bottom ſhould be made to fit the mortar. S is a groove cut through the ſtone.

On grinding indigo, or ſuch ſubſtance, in a dry ſtate, in this mill, the muller being placed in the mortar, and ſecured in the oak pieces by the pins, the indigo to be ground is thrown above the muller into the mortar; on turning the handle of the axis, the indigo in lumps falls into the groove cut through the muller, and is from thence drawn under the action of the muller, and propelled to its outer edge within the mortar, from whence the coarſer particles again fall into the groove of the muller, and are again ground under it; which operation is continued, till the whole of it is ground to an impalpable powder; the muller is then eaſily removed, and the colour taken out.

A wood cover, in two halves, with a hole for the axis, is uſually placed upon the mortar, during the operation, to prevent any loſs to the colour, or bad effect to the operator.

A new

Mr. Murray's Portable Steam Engine.

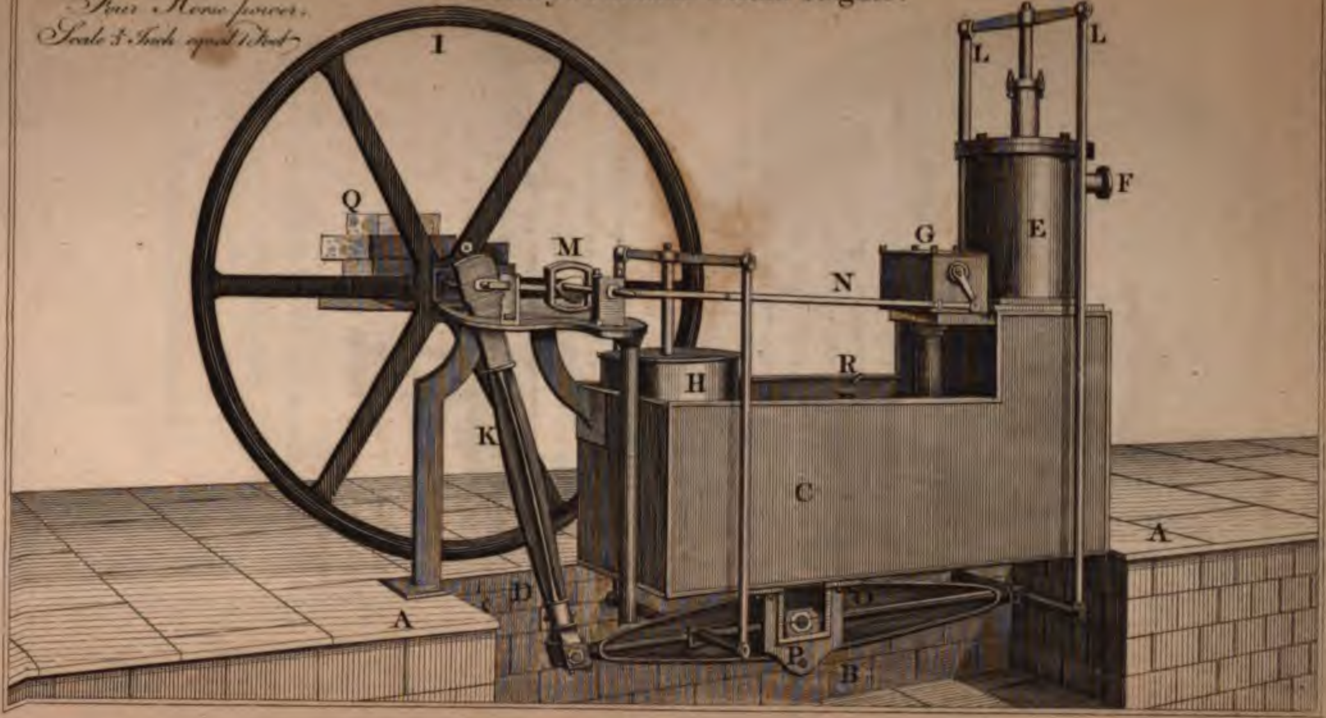
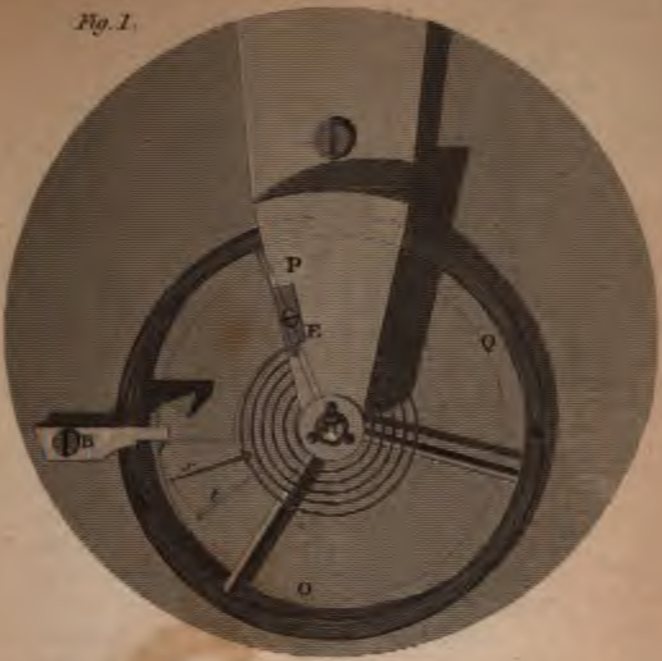
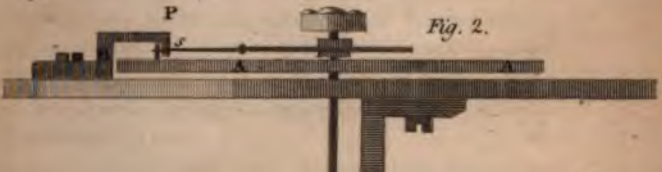


Fig. 1.



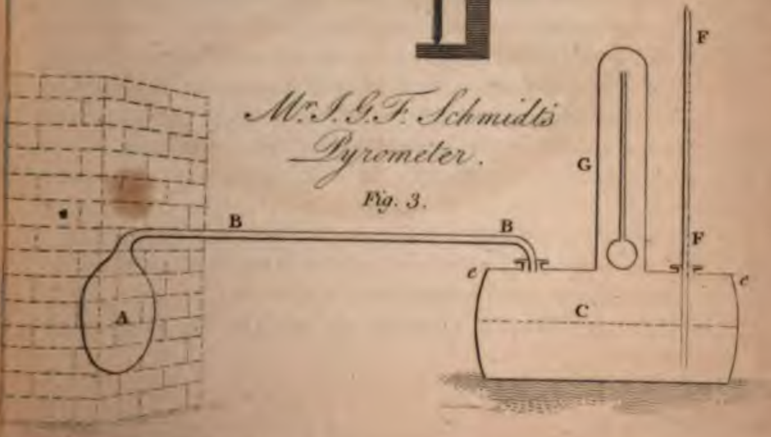
Mr. Hardy's method of banking the balance of a Time keepe

Fig. 2.



Mr. J. G. F. Schmidt's Pyrometer.

Fig. 3.



THE
PUBLIC [illegible]
ASTOR [illegible]
TILDEN [illegible]

XVIII.

A new and cheap Method of purifying Gold and Silver. By ANDREW THOMSON, *Eſq. of Banchory, near Aberdeen. In a Letter from the Diſcoverer.*

To Mr. NICHOLSON.

SIR,

I INTENDED to have deferred the preſent communication till ſuch time as I ſhould have it in my power to lay before the public the complete ſeries of experiments in which I have been engaged with regard to the purification of gold and ſilver. But unluckily I mentioned a few particular circumſtances with regard to them, to a man who took it upon him, without my knowledge, to ſend an account of them for publication to a periodical work. As I underſtand that work will not appear ſo ſoon as your next number, I beg, if you think it worthy of a place, that you will inſert the following account of ſome attempts I have been making to purify the precious metals.

This publication is prematurely made, becauſe the inventor might elſe be anticipated.

Being much at a loſs for want of a crucible of pure ſilver for the analyſis of ſome minerals, and as all the uſual methods practiced for purifying that metal are very troubleſome, I ſet myſelf to conſider the various operations on metals, in hopes of falling on a more ſimple way of accompliſhing my purpoſe. At length, I found a proceſs of Pelletier's, which promiſed to ſucceed, and mine is merely extending his idea a little further than he did himſelf.

Proceſs of Pelletier for extracting copper from bell-metal.

He was, I believe, employed by the French government to diſcover an eaſy way of ſeparating the tin from copper on bell-metal, and the proceſs he gave, is this. Upon the melted bell-metal project black oxide of manganeſe in powder, frequently ſtirring the metal till all the tin becomes oxidated by the manganeſe. He adds a caution, not to add too much manganeſe, otherwiſe part of the copper alſo will be deſtroyed.

He oxides the tin by manganeſe; in ſuch proportion as not to affect the copper.

It immediately ſtruck me, that in this way I might be able to oxidate the copper which alloys our ſilver, and upon making the trial I ſucceeded completely; I had ſome impure ſilver rolled out to about the thickneſs of a ſhilling, this I coiled

The author's improved proceſs. Coarſe ſilver was rolled out; then coiled up; bedded in manganeſe; and

Strongly ignited for a quarter of an hour. All the metal was oxided.

coiled up ſpirally, and put into a crucible, the bottom of which was covered with black oxide of manganeſe. I then added more oxide till the ſilver was covered, and all the ſpace between the coils completely filled. A cover was then luted to the crucible, and a ſmall hole left for the eſcape of oxigen gaſs. When this had been expoſed for a quarter of an hour to a heat ſufficient to melt ſilver, I found the ſurface of the manganeſe brown from the loſs of oxigen; but, where the ſilver had been, the whole was one uniform black powder, without the leaſt appearance of metallic luſtre, ſo that I had no doubt, that even the ſilver was become an oxide.

The whole contents were then put into a larger crucible with thrice its bulk of green glaſs.

I then put the whole contents of the firſt crucible into a ſecond of a larger ſize, into the bottom of which I put a quantity of pounded green glaſs, about three times the bulk of the contents of the firſt crucible, and luted on a cover as before, to prevent the acceſs of any inflammable ſubſtance.

Strong heat fuſed the glaſs, and reduced the ſilver pure, and alone in a button.

The crucible was then expoſed to a heat ſufficiently ſtrong to melt the glaſs very fluid. Upon cooling and breaking the crucible, I found the ſilver at the bottom perfectly pure, as its oxide alone could part from its oxigen without the acceſs of ſome inflammable ſubſtance. I find this proceſs anſwers equally well for purifying gold, and to me it ſeems to poſſeſs ſome advantages over all the former methods. The materials uſed are cheap, and a large quantity can be refined as ſoon, and as eaſily as a ſmall quantity, by merely altering the capacity of the crucible you uſe.

This proceſs anſwers equally with gold.

The metal muſt be in thin or ſmall maſſes.

I tried the ſame operation on gold and ſilver in round maſſes, but found it went on very ſlowly, and what I ſcarcely expected, in the firſt part of the proceſs of oxidating the metals, the remaining metal continued uniformly impure or nearly ſo, until the whole was oxidated.

The proportions, &c. are not here given, becauſe the author has haſtened to communicate his proceſs.

I regret that I have been forced to make this matter public, before I could do it in a manner ſatisfactory to myſelf. I wiſhed to have given the exact proportions of alloy, manganeſe, and glaſs to be generally uſed, and to have aſcertained if there is any truth in the old opinion, that ſaltpetre melted with gold deſtroys a part of it. I ſuppoſe that idea may have arizen from the oxigen given out by the nitre in a high heat, oxigenating the copper contained in the impure gold, which has been the ſubject of the experiment.

Since the above was written, I have been informed that this matter has actually been publiſhed, but know not in what work. I hope you will ſtill have the goodneſs to inſert this as an *original communication*, as I do not think the perſon who has publiſhed it will have the impudence to call it his own, and as Mr. Kirwan, and other celebrated chemiſts long ago adviſed me to publiſh it, I have already ſtated my reaſons for not following ſuch good advice.

He vindicates his claim.

As I have now been forced to appear before the public, I have hopes I ſhall be able to prevail on ſome of my friends to commit themſelves in the ſame way, in the confidence that their labours will be found uſeful to the public.

I am, Sir,
Your's truly,
ANDREW THOMSON.

Banchory, by Aberdeen,
May 5th, 1805.

XIX.

Memoir on the Propagation of Sound. By M. HASSENFRATZ.*

THE production of ſound is aſcribed by all natural philoſophers to the vibration of the molecules of bodies.

Sound produced by vibrations,

The vibration of theſe molecules admits of two kinds of modification; 1ſt, in velocity; 2ly, in magnitude. The firſt of theſe determines the nature of tones; the ſecond, their force or intenſity.

which differ in velocity and in magnitude.

Sound is tranſmitted to the ear by the molecules that fill the medium or interval between the ſonorous body and the organ of the hearing. The movement of the ſonorous body impreſſes on the molecules of the medium an impulſe, which they tranſmit from one to another, till it reaches the ear, with a greater or leſs velocity. In this tranſmiſſion the vibration may undergo two kinds of alteration: 1ſt, in its velocity; 2dly, in its intenſity. In this memoir I ſhall only tranſcribe ſome experiments relative to the velocity of ſound.

The ſonorous body impreſſes its vibrations on ſome medium, the vibrations of which are continued to the ear.

Philoſophers have long been engaged in determining the velocity of ſound, but conſidering the air as the chief medium

The velocity of ſound hitherto examined only in air.

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* Annales de Chimie, Vol. LIII. p. 64.

by which it is tranſmitted to the ear, they have attended only to its velocity in air, and have employed two different methods to determine this, theory and experiment.

This velocity uniform in a given medium,

and the denſity of the medium is one of its elements.

Not affected by the height of the barometer.

These two methods have led to the following remarkable reſults: 1ſt, that the velocity of ſound in a given medium, is uniform, whatever its diſtance from the phonic centre, and whatever its intenſity: 2dly, that the denſity of the medium at equal preſſures is one of the elements of the velocity; for it has been found by theory, that the velocity of ſound is the ſame as that of a body falling from half the height of an atmoſphere ſuppoſed to be of equal denſity with the air in the place where the ſound is tranſmitted; and by experiment, that, all other circumſtances being equal, the velocity of ſound is the ſame at different preſſures of the barometer; ſo that it is equal on the ſummit of a mountain and on the ſea-ſhore. In fact, the denſity of the air being proportional to the compreſſing weight, the height of the column of mercury in the barometer, divided by the denſity occaſioned by this preſſure, is a conſtant quantity; and the height of the atmoſphere of a uniform denſity being equal to the total weight of the air divided by its denſity, it follows that the height of the barometer ought to make no difference in the velocity of ſound. Blanconi aſſerts (Comment. Bonon, vol. II. p. 365), that the velocity of ſound is leſs in winter than in ſummer, ſince, according to his experiments, it takes four ſeconds more in winter to traverſe a ſpace of ſixteen Italian miles. Derham affirms, that the velocity of ſound is the ſame whether the air be extremely hot or extremely cold, though his tables of experiments will be found on examination favourable to the opinion of Blanconi; for the greateſt velocity of ſound in them was on the 5th of April, at one o'clock in the afternoon, being three miles in 111 half-ſeconds, and the leaſt velocity on the 12th of February, at ſix o'clock in the evening, being three miles in 122 half-ſeconds. As the experiments on the velocity of ſound undertaken by the Academy of Sciences in 1737 were made at temperatures that exhibit only two or three degrees difference, perhaps it would be well, as Mr. Laplace thinks, if they were repeated at a time of the year when the temperature is very different; for experience has taught us, that this velocity is equal in rainy and in fine weather, ſo that nothing but change of temperature can produce any variation in this reſpect.

Blanconi aſſerts it to be leſs in winter than in ſummer.

Derham denies this, but is probably wrong.

Not affected by rain or fine weather.

Whatever theſe reſults may prove, as the experiments on the velocity and propagation of ſound have been hitherto made in the air alone, it was an intereſting enquiry to determine the velocity of ſound, when tranſmitted by other bodies, and particularly of different denſities with reſpect to air. Mr. Laplace, to whom branches of phyſical ſcience are indebted for improvement, invited me about eight months ago to make experiments on this ſubject, and particularly on the propagation of ſound through ſolid bodies; and the experiments of which I ſhall give an account in this paper were principally made in conſequence of that gentleman's ſuggeſtion.

Making experiments in the quarries beneath Paris on the tranſmiſſion of ſound through long galleries, I cauſed a perſon to ſtrike with a hammer againſt a maſs of ſtone, retiring at the ſame time by degrees from the place of ſtriking, in order to diſtinguiſh if poſſible the ſound tranſmitted by the ſtone from that tranſmitted through the air. Placing my ear againſt the maſs of calcareous ſtone through which theſe galleries are carried, at a ſhort diſtance I diſtinguiſhed two ſounds perfectly ſeparate, one tranſmitted by the air, the other by the ſtone. Both ſounds grew weaker in proportion as I retired from the ſtriking point; but that tranſmitted by the ſtone was weakened much more rapidly than that tranſmitted through the air. In a gallery excavated beneath Rue de la Harpe the ſound tranſmitted by the ſtone ceaſed to be audible at 134 paces diſtance; and in a gallery beneath Rue de St. Jaques at 140 paces.—— Through the air the ſound was tranſmitted to 400 or 440 paces diſtance. The ſound tranſmitted by the ſtone always reached the ear much ſooner than that tranſmitted by the air.

Experiments on the tranſmiſſion of ſound through the ſtone in quarries.

It was conveyed through the ſtone only 140 paces;

—but through the air 440.

It paſſes quickeſt through the ſtone.

Mr. Berthollet, to whom Mr. Laplace imparted theſe reſults, deſirous of being aſſured whether the ſound of a hammer could be tranſmitted through a maſs of ſtone 140 paces thick, requeſted Mr. Gay, by my deſire, to be preſent at my experiments. With this young chemiſt I repeated the experiment of the tranſmiſſion of ſound through ſtone on ſeveral ſeparate maſſes, and he convinced himſelf, that ſound was capable of being tranſmitted through a maſs 150 paces in length.

Experiment repeated, and the ſound conveyed 150 paces.

It was long ago obſerved in working mines, that the noiſe was propagated to a very great diſtance through maſſes of rock; and the line of the ſound heard through the ſtone ſerves on many occaſions to determine the direction in which the gal-

The propagation of ſound through ſtone long ago obſerved by mechanics.

Its velocity apparently equal to that of light.

leries are carried on; but no one had attempted to obſerve, whether the velocity of the ſound tranſmitted by the ſtone differed from that of the ſound tranſmitted by the air. My experiments in the quarries underneath Paris have taught me, that the difference is conſiderable; and when the gallery is ſufficiently ſtraight, to be able to diſcern the motion of the hammer with the eye, no calculable difference can be perceived between the conveyance of the motion to the eye and that of the ſound to the ear.

The differences of the diſtances to which ſound is tranſmitted through ſolid maſſes remarkable.

The diſtance which the ſound of the hammer can be conveyed to the ear varies conſiderably with the nature of the ſtone and the ſeparations or fiſſures in the maſs. Having cauſed a man to ſtrike with reiterated blows againſt an iſolated wall, built of common ſtone of the ſame kind as that in the quarries, and cemented with mortar, the ſound was tranſmitted only thirty paces. Striking in the ſame manner on a parapet of hewn ſtone raiſed on the borders of the Seine, the ſound was tranſmitted 46 paces. Theſe experiments were made in the open air by day, conſequently under circumſtances leſs favourable to the propagation of ſound than when on the calcareous maſſes in the quarries; but the difference between 30 and 46 paces, under the ſame circumſtances, on maſſes differing only in the dimenſions of the ſtones of which they were formed, is very remarkable.

Experiments repeated,

Encouraged by the ſucceſs of my experiments in the quarries, and by the invitation of Mr. Laplace, I attempted to repeat the ſame experiments on different ſubſtances.

on timber.

By the ſide of the high road that leads from the place *de la Concorde* to *Chaillot* along the bank of the Seine, on the ſtone wharf of St. Leir, oppoſite the ſteam-engine of Gros-Caillou, is placed a railing 210 paces in length, formed of 31 pieces of timber, ſeparated by four large poſts. The blow of a hammer at one extremity of this railing was heard diſtinctly at the other, though through the air it was audible only 120 paces. At the diſtance at which both the ſounds were audible, that through the wood was heard long before the other; and when, ſtanding at the greateſt diſtance from the place of the blow, I heard only the ſound tranſmitted through the timber, the velocity of its tranſmiſſion was ſo great, that it was difficult to diſtinguiſh any interval between the perception of the ſound by the ear, and of the motion of the hammer by the eye.

Conveyed farther than in the open air.

Its velocity apparently equal to that of light.

Having convinced myſelf, that the propagation of ſound through ſtone and through wood was effected with much greater velocity than through air, and that the time of its tranſmiſſion to ſuch ſhort diſtances as thoſe on which I was able to make experiments was too little for calculation, I was deſirous of knowing whether the velocity of its tranſmiſſion through metallic ſubſtances were the ſame.

Similar experiments and reſults with long bars of iron.

Several experiments on bars of iron fixed on ſolid maſſes, as the bars that hold together the ſtones of parapets, having given me uncertain reſults, I ſought for iſolated bars of ſufficient length to afford ſome certainty. My firſt experiment was made on the upper bars of an iron railing, 34 paces long, erected on one of the walls of the garden of the Legiſlative Body, adjoining to the Place des Invalides. On ſtriking one extremity of this aſſemblage of bars, two diſtinct ſounds were heard at the other end; that tranſmitted by the bars, and that by the air; the former being always heard firſt. The ſame experiment afterwards repeated on bars of different lengths, gave me the ſame reſult; and this reſult is ſuch, that it is impoſſible to diſtinguiſh, at the ſmall diſtances at which theſe experiments were made, any difference between the tranſmiſſion of the motion by light, and that of the ſound by the ſolid medium.

Mr. Gay ſuppoſed he heard two ſounds through the air, firſt a grave, then an acute.

Repeating my experiments on the velocity and propagation of ſound through the maſſes of ſtone in the quarries beneath Paris, in company with Mr. Gay, this young chemiſt imagined that he diſtinguiſhed two ſounds tranſmitted through the air, one grave and the other acute, which reached his ear in ſucceſſion, the graver ſound appearing to have the greater velocity.

Mairan and others had conjectured the ſame.

This reſult, though contrary to the theory of the propagation of ſound, according to which grave and acute ſounds ought to have the ſame velocity, had already been conjectured by ſeveral philoſophers, particularly by Mairan, and was therefore worth confirmation. For this purpoſe I ſtretched two ſtrings, one of braſs, the other of catgut, ſo as to make them emit two different tones, the firſt one more acute, the ſecond one more grave; then ſtriking both theſe ſtrings at once with the wood of a black lead pencil, the two ſounds, which were confounded together at firſt, appeared to ſeparate at the diſtance of 400 paces in a large gallery of the quarries, and we both imagined we could diſtinguiſh the graver ſound firſt.

Experiment to determine this.

Probable cauſe of fallacy in this experiment.

While obſerving the propagation of ſound in the galleries, I had ſeveral times occaſion to remark, that a ſound at a great diſtance was frequently repeated, either by reflection or by the vibration of the walls, ſo as to cauſe two differerent ſounds to be heard and diſtinguiſhed, which reached the ear in ſucceſſion. As it was poſſible that the difference which appeared to Mr. Gay and me might have been produced by the cauſe here mentioned, I determined to repeat the experiment in the open air.

The experiment repeated in the open air.

The double ſound produced even here by an echo.

Grave and acute ſounds have the ſame velocity.

For this purpoſe I took two glaſs bells, the tones of which were as an octave and a fifth, that is, the ratio of their vibration was as 1 to 3. A hammer was ſo adjuſted as to ſtrike both the bells at once, and make them ſound at the ſame inſtant. Carrying this inſtrument into the fields, I endeavoured to aſcertain whether the two ſounds reached the ear in unequal times. Several experiments repeated in various places, made me believe a long time, that their velocity was unequal; but having obſerved, that on ſome occaſions the ſounds reached the ear at the ſame time, I was led to remark, that whenever I imagined I diſtinguiſhed a difference of velocity, this difference had been occaſioned by a repetition of the ſound, and that frequently very trifling obſtacles, as trees or hedges, were ſufficient to produce this repetition. I repeated my experiments therefore anew in the midſt of plains of greater or leſs extent, as thoſe of Montronge, Grenelle, St. Denis, &c. and whenever I was remote from any obſtacle capable of producing a repetition, both ſounds were heard at the ſame time. With my inſtrument I could diſtinguiſh the ſound of the two bells at the diſtance of 700 paces or 631 yards; whence it follows, that both experiment and theory concur to demonſtrate, that grave and acute ſounds have the ſame velocity.

Concluſions from the experiments.

From the experiments related in this paper it follows: 1ſt, that the velocity of ſound differs according to the medium by which it is propagated: 2dly, that this velocity is much more conſiderable, when it is propagated by ſolid and very denſe bodies, than when by aeriform bodies, and of little denſity: 3dly, that both grave and acute ſounds have the ſame velocity; a reſult to which theory led us.

XX.

A very advantageous Mode of preparing Muriate of Barytes by the Mutual Decompoſition of ſulphurated Barytes and Muriate of Lime. By Mr. GOETTLING.[*]

Drieſſen firſt decompoſed ſulphate of barytes by muriate of lime; and next Trommſdorff.

MR. DRIESSEN of Groningen firſt obſerved, that muriate of lime and ſulphate of barytes decompoſe each other at a high degree of heat. Trommſdorff verified the experiment of the learned Dutch profeſſor, and applied the principle to the preparation of muriate of barytes. He perceived, however, that the whole of the ſulphate of barytes was not decompoſed in this way, and he thought, that equal parts of the two ſalts were the ſuitable proportion for decompoſing them as completely as poſſible. Mr. Goettling has deemed the ſubject worth farther inquiry, the reſult of which was the proceſs I ſhall now deſcribe.

Mr. Goetling's proceſs for preparing muriate of barytes in this way.

One part of native ſulphate of barytes in fine powder is to be mixed with half a part of muriate of lime.[†] This mixture is to be introduced into a Heſſian crucible, which is to be cloſely covered, and brought gradually to a red heat. The matter muſt be kept in a ſtate of incandeſcence a full half-hour, and frequently ſtirred. It is then to be poured out into an iron cone, and after being coarſely powdered, is to be thrown into three parts of boiling water. The veſſel being immediately taken from the fire, the mixture is to be ſtirred occaſionally with a glaſs ſpatula, and the undiſſolved matter is then to be left to ſubſide. The clear liquor being decanted off, the reſiduum is to be poured into a filter of linen of a cloſe texture, and the fluid lightly preſſed out. The reſiduum being again lixiviated with one part of water, is to be ſtrained as before. The liquors are then to be mixed together, and evaporated to a pellicle, to obtain the ſalt by cryſtallization. In this way we ſhall have five-eights of a part of muriate of barytes. The mother-water of the firſt cryſtallization, which is almoſt wholly muriate of lime, is to be ſet aſide for a freſh operation, or for any other purpoſe.

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[*] Van Mons's *Journal de Chimie*, Vol. VI. p. 92. Abridged from the *Taſchen-Buch fuer Scheidekuenſtler*.

[†] This muriate is obtained in abundance in our laboratories, as an adventitious product: it may be procured likewiſe at a very trifling expence by adding lime to the mother-water left after refining common ſalt.

Muriate of lime, how obtained.

The ſalt to be purified by re-cryſtalliſation.

As the inſoluble maſs of ſulphate of lime ſtill contains a large quantity of ſulphate of barytes, indeed about half the original quantity, it is to be treated afreſh as above, with one fourth part of muriate of lime; and the ſame proceſs is to be repeated with an eighth part of the ſame ſalt. Thus we obtain an addition from one to two eighth parts of muriate of barytes. The ſalts of the various cryſtallizations require to be diſſolved and re-cryſtallized anew, in order to free them from a little muriate of lime, which adheres to them in the firſt cryſtallization.

XXI.

Obſervations on the Rectification of Nitric Acid, by Mr. STEINACHER, *Druggiſt at Paris.**

Nitric acid contaminated in its firſt portions with muriatic. Why, according to Berthollet.

It has long been known, that the firſt portions of nitric acid diſtilled from litharge contain muriatic acid. Berthollet explains this phenomenon by ſaying, that the oxide of lead dividing its action between the two acids, both are ſubjected to the action of expanſibility produced by the heat. Meſſrs. Welter and Bonjour aſſert, that, if muriate of ſilver be employed, oxigenated muriatic acid is formed, which riſes with the firſt portions. If I may be allowed to give the reſults of my labours after thoſe of ſo many able chemiſts, I would ſay, that, when the nitric acid has been ſufficiently concentrated before being ſubjected to rectification on ſilver, or on oxide of lead, the firſt part of the rectified acid is found on trial to contain no muriatic acid, notwithſtanding the nitric acid contained much of it after its concentration; and hence I infer, that an exceſs of water is the true cauſe, that diminiſhes the attraction of the muriatic acid for the oxide of lead or of ſilver.

True reaſon is the preſence of an exceſs of water.

Concentrating the acid inſufficient, if the proportion of litharge be improper, and the diſtillation carried to dryneſs.

The operator however would in vain expect to ſucceed by merely concentrating his acid before rectifying it, if he uſed a determinate proportion of litharge, or diſtilled to dryneſs, as ſeveral authors direct. The quantity of litharge muſt vary from one to eight ſixteenths of the weight of the acid, according to its degree of impurity. On the other hand, if diſtilled to dryneſs, the latter portions of the nitric acid will carry over with them in ſolution muriate of lead, or of ſilver.

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* Van Mons's *Journal de Chimie*, Vol. VI. p. 88.

Mr. Steinacher's proceſs.

Four kilogrammes (10lb. 8oz. 16dwt. troy) of nitric acid of the ſhops, at 35°, containing muriatic acid, and a very little ſulphuric, are firſt diſtilled in a reverberatory furnace in a retort placed on an earthen veſſel filled with ſand. The fire muſt be ſo regulated that the drops ſucceed each other ſlowly, and half the acid is to be thus drawn off. It will then give 15° of Baumé's areometer. What remains in the retort is to be poured into a bottle. Its ſpecific gravity will be expreſſed by 4° of the areometer. Litharge being thrown into it in fine powder, and ſtirred with a glaſs rod, will be converted into a white powder in a few hours. More litharge is then to be added in the ſame manner; and this is to be continued, till the litharge retains its colour after ſeveral hours ſtanding. The muriate and ſulphate of lead are then to be left to ſubſide entirely, and the acid is to be decanted off into a tubulated glaſs retort, placed on a ſmall earthen plate filled with ſand, in the midſt of a reverberatory furnace, all the parts of which are retained except the dome. A receiver is to be adapted, which fits cloſely without luting; for, as the vapour of the acid eaſily deſtroys every kind of lute, the product would otherwiſe be liable to become impure; and the diſtillation is to be ſo conducted, as to admit a ſhort interval between the fall of each drop. Great care muſt be taken not to ſuffer the acid to boil, for thus it would be diſſipated in incompreſſible vapour. The firſt half that comes over marks 35°, the ſecond 40°. Both portions are colourleſs, and have all the properties of a very pure nitric acid, if $\frac{1}{12}$ of the liquor be left in the retort.

Beautiful cryſtals of muriate of lead may be obtained when half the acid is diſtilled off.

If a ſtop be put to the diſtillation after the firſt portion is ſeparated, and the retort left to grow cold, you will obtain a beautiful cryſtallization of muriate of lead in large and very brilliant ſtriated hexaedral laminæ. This ſalt is a true muriate, for ſulphuric acid expels from it vapours eaſily diſtinguiſhable to be thoſe of the muriatic acid. On continuing the diſtillation, theſe cryſtals gradually loſe their regular figure, and at length fall to the bottom in a powdery precipitate.

Eaſy

XXII.

Eaſy Method of making the very combuſtible Oxide of Phoſphorus.

By Amicus.

To Mr. NICHOLSON.

Dear Sir,

Oxide of phoſphorus.

YOU know very well that phoſphorus united to a much ſmaller proportion of oxigen than is requiſite to render it into the acid ſtate, brings it into the condition of an oxide which ſets on fire ſulphur, on juſt rubbing it againſt a common match. But the common method of oxidizing phoſphorus for phoſphoric matches by fixing it in the bottles with a hot iron, is troubleſome and waſteful. This oxidation, however, may be effected with great facility and economy by expoſing a large proportion, viz. a hundred grains of phoſphorus in a jar containing half a pint meaſure of oxy-muriatic acid, in which circumſtance the phoſphorus will be melted and fume, but ſcarcely take fire. After cooling, it muſt be kept excluded from the air, to prevent the inflammation from mere expoſure.

Dear Sir, Yours,

AMICUS.

May 28th, 1805.

XXIII.

Deſcription of an extremely ſenſible Micro-electrometer. By Mr. Marechaux.†

A piece of leaf ſilver is ſuſpended ſo as to be moveable in a glaſs cylinder.

IN a glaſs cylinder, about an inch and half in diameter and five or ſix inches high; a piece of leaf ſilver is ſuſpended from a ſmall pair of nippers, capable of being lowered or elevated as the length of the leaf may require. The piece that carries the nippers may likewiſe be moved horizontally, ſo that the leaf may be moved at pleaſure nearer to or further from a ſphere of copper, which is one of the poles of the inſtrument.

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* Tranſlated from Von Mons's *Journal de Chimie*, Vol. VI. p. 38. Abridged by Van Mons from Gilbert's *Annalen der Phyſik.*

The glaſs cylinder has, about 1″ (centimetre, near 4 lines Engliſh meaſure?) from the plate on which it is fixed, a ſmall hole, through which paſſes the extremity of a micrometer-ſcrew, about the ſize of a large gooſe-quill, and very carefully cut. This ſcrew has fifty threads in a Rhynland inch, cut very deep, though very fine. It is made of two pieces, and ſhould be at leaſt three quarters of an inch long, to avoid any ſhake. The extremity of this ſcrew carries a little ball, which is put on after the ſcrew is paſſed through the opening in the cylinder. To avoid all friction againſt the glaſs, care is taken that the ſcrew, when turned, does not touch the edges of the cylinder. The ſcrew carries a plate three inches and a half (3.8 Engliſh) in diameter, which has 360 diviſions, and conſequently divides each thread of the ſcrew into as many parts.

Through a hole in the cylinder a micrometer-ſcrew paſſes,

In this manner we are enabled to determine the ſphere of activity of the two electricities in 18000ths of a Rhynland inch. The mounting which contains the female ſcrew has a ſmall pillar, which advances on the plate, and carries an index, by which the degrees are marked with preciſion.

by which the diſtance of $\frac{1}{18000}$ of an inch may be meaſured.

To uſe this inſtrument, which is perfectly inſulated by the glaſs plate on which it reſts, the firſt thing is to place the plate in ſuch a poſition that the 0 ſhall be exactly under the index. The adjuſting ſcrew which carries the nippers is then to be moved till the leaf ſilver is ſo near the ball, that no light paſſes between them. Thus we have the point of contact, and of 0 for the ſphere of activity of the two electricities. To be certain that the leaf is brought as cloſe as poſſible to the ball without being forced out of the vertical direction, the micrometer ſcrew ſhould be moved a turn firſt backward then forward ſeveral times, and the poſition of the leaf obſerved every time the ball is brought into contact with it. The inſtrument being thus adjuſted, the micrometer-ſcrew muſt be moved backward one turn, and then we have between the leaf and the ball a diſtance of one-fiftieth of an inch, which may be ſubdivided at pleaſure by means of the plate; for with a plate near four inches in diameter, and by means of the fine needle on which it turns, we may diſtinguiſh half or even a quarter of a degree if neceſſary.

Mode of preparing the inſtrument for uſe.

An adjuſtment fixed to the plate on which the cylinder reſts ſerves to aſcertain whether the leaf of ſilver be in fact drawn out

Apparatus for aſcertaining the perpendicularity of the leaf.

out of the vertical line by any attractive power. This conſiſts of a fine ſilk thread, ſtretched in the ſame plane as the ſilver leaf. By means of the ſcrew this thread may be moved both horizontally and vertically, ſo as to follow the movements of the ſilver leaf.

Mode of uſing the inſtrument.

Every thing being thus arranged, make a communication by means of conducting wires between a ſingle couple of metallic diſks, or one conſtituent part of a pile, placed on a plate of glaſs, and the inſtrument, ſo that one of the metals ſhall communicate with the top of the inſtrument, and the other with the bottom. Then by means of a glaſs handle fitted to it move the plate ſlowly from one degree to another, and you will find the leaf touch the ball with ordinary electricity, when it is 60° or 80° of the micrometer ſcrew from the vertical plane, in which the leaf ſilver reſted before its communication with the metallic diſks. This diſtance increaſes for every pair of diſks added; and as the ball remains fixed at the point to which the ſcrew has carried it, the motion of the inſtrument may be obſerved with great accuracy.

Its extreme ſenſibility.

This inſtrument is ſo ſenſible, that, if a ſlender glaſs tube be rubbed but twice, and brought near the apparatus, though ſeveral inches from its ſummit, it paſſes through the whole extent of its ſcale. It is for this reaſon the inventor calls it a *micro-electrometer*, becauſe we can meaſure only very weak degrees of electricity with it.

XXIV.

Action of Phoſphorus on the Solutions of Metals. By Mr. SCHNAUBERT [*].

Phoſphorus obſerved to precipitate metals, by Sage, Illemann, and Mrs. Fulhame.

SAGE [†] had already obſerved, that phoſphorus precipitates the ſulphates of copper and of mangaueſe. After him Illemann [‡] obtained a cryſtallization of ſilver in the humid way by means of phophorus. Still more recently Mrs. Ful-

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[*] Van Mons's *Journal de Chimie*, Vol. VI. p. 95. Abridged from Goettling's *Chemiſches Taſchenbuch*.

[†] *Analyſe chimique et Concordance des trois Règnes.*

[‡] Crell's *Chemiſche Annalen*, 1789, Tom. II. p. 323.

2 hame

hame* publiſhed experiments on the reduction of ſome metals by phoſphorus diſſolved in ether. Laſtly, Mr. Schnaubert has undertaken a new inveſtigation of the ſubject, the principal reſults of which are as follows:

Schnaubert's experiments.

Gold.

Gold completely precipitated in a metallic form by phoſphorus.

Two little bits of phoſphorus were put into a nitro-muriatic ſolution of gold diluted with a ſmall portion of water. At the expiration of twenty-four hours the ſolution was completely colourleſs, and pellicles of the colour of metallic gold ſwam on the ſurface of the liquid. The phoſphorus itſelf was covered with a deep brown coating, and in this was obſervable in ſeveral places thin layers of reduced gold. At the place where the phoſphorus was a black circle was perceived. The ſolution thus treated by phoſphorus, had not a ſingle atom of gold left in it.

Silver.

Silver precipitated in dendritic cryſtals,

Some phoſphorus, which was left for twenty-four hours in a nitric ſolution of ſilver diluted by water, was completely covered with metallic ſilver in the form of dendrites, the ramifications of which were directed upwards. During ebullition this remarkable cryſtallization of ſilver, which made the phoſphorus appear as if garniſhed with points, aſſumed firſt a white colour, and afterward formed a light black maſs, which at length became of a light brown colour.

which boiling converted into a phoſphure.

Quickſilver.

Mercury precipitated in globules,

Mercury diſſolved in nitric acid is precipitated on the phoſphorus in the form of little metallic globules, which cover it entirely. By heating to ebullition the mercurial globules diſappear, and a black maſs without any metallic luſtre is formed.

which heat converts to a phoſphure.

Lead.

Lead not reduced without a boiling heat.

At the ordinary temperature phoſphorus did not act on the nitric ſolution of lead, though the digeſtion was continued for ſeveral days: at a boiling heat however a change was obſerved in the phoſphorus, which was covered with a grey colour ſlightly metallic.

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* Eſſay on Combuſtion, &c.

Copper.

Copper.

Copper powerfully acted on.

No metallic ſolution was more ſtrongly attacked by phoſphorus than that of copper in nitric acid. The phoſphorus was no ſooner introduced into the ſolution of copper, than it aſſumed a black colour. In twenty-four hours it was covered with a ſtratum of metallic copper in very thin layers [*]; and the ſolution had become much paler. The application of heat cauſed drops of phoſphorus to flow out upon the reduced copper, where they immediately aſſumed a black colour. Theſe drops after a time were in their turn covered with a metallic coat of copper. After this the ſolution was perfectly colourleſs, and ammonia did not detect in it the leaſt particle of metal.

Completely precipitated from the ſolution.

Tin.

Tin partly reduced, partly converted into phoſphure.

Several bits of phoſphorus were put into a ſolution of tin in nitro muriatic acid. The next day the phoſphorus was coloured of a deep brown, only in ſome parts a metallic colour was obſervable. Theſe metallic ſpots diſappeared on boiling, and the phoſphorus became ſtill deeper coloured.

Sulphate of copper forms a beautiful experiment.

[*] In making this experiment with a ſolution of ſulphate of copper, and ſlightly heating the mixture, at firſt a vapour ariſes, conſiſting of phoſphorus gas, that carries off with it ſome ſmall particles of phoſphorus, which take fire on the ſurface of the ſolution: but the extrication of this vapour ſoon ceaſes, and the phoſphorus becomes hermetically encloſed in a box of copper, in which it is defended againſt any farther action of the ſulphate, and even of the air, to whatever temperature ſhort of fuſing the copper it be afterwards expoſed. The plate of copper that forms this covering is two or three lines thick: it poſſeſſes more tenacity than common copper, for it may be flattened with a hammer in different directions without cracking, which at the ſame time proves the great compreſſibility of the phoſphorus; and it ſhines with a very pure metallic luſtre. On opening the box carefully with a cutting inſtrument, the phoſphorus is found in it retaining perfectly its form, filling its copper caſe completely, and not appearing even to have acted upon the ſulphate.

The phoſphorus enveloped in a caſe of particularly malleable copper impervious to air.

Other metals did not produce this effect.

I did not obtain the ſame effect with ſeveral other metals which I tried, no doubt on account of their containing more oxigen.

VAN MONS.

Manganeſe.

Manganeſe reduced by it.

Some bits of phoſphorus were put into a ſulphuric ſolution of manganeſe. The next day the phoſphorus was of a deep brown colour, and on its ſurface was perceivable a pleaſing mixture of colours, owing to the reduced metal. The mixture was then heated to ebullition, and after it had grown cold, the manganeſe was found reduced in the form of little radiating lines on the fuſed phoſphorus, intermixed with a few ſmall globules of the white colour of tin.

General concluſions.

In theſe experiments we may obſerve, that the phoſphorus, beſide deoxidating the metals, united with the metals when reduced to form phoſphures, as was evident in the ſolutions of ſilver, mercury, and tin.

XXV.

Account of a new Pyrometer, which is capable of indicating Degrees of Heat of a Furnace. By Mr. J. G. SCHMIDT, *of Yaſſy, in Moldavia. From the Author.*

Pyrometrical ſubſtances.

WITHOUT entering into any detail concerning the ſubſtances beſt calculated for pyrometrical enquiries, I flatter myſelf that it will be admitted that thoſe muſt receive the preference which are capable of regularly contracting or expanding, without altering their chemical properties, when ſubjected to elevated temperatures.

Gaſes are the beſt for ſtrong heat.

The permanently elaſtic aeriform fluids appear to me to be ſuperior in thoſe reſpects to any other bodies.

Atmoſpheric air in a veſſel of platina.

Let atmoſpheric air be freed from moiſture by cauſtic alkalis, or other bodies, and included in a veſſel of platina. This veſſel A (*Fig.* 3, *Pl. VI.*), which may be made of any convenient ſize, is connected with the tube B B, of as fine a bore as poſſible. This tube is alſo made of platina, and reaches into a veſſel C, which is filled with water up to *e e*, and into this the tube is fixed air-tight. Out of the veſſel C riſes a glaſs cylinder G hermetically ſealed, including a thermometer, and a graduated tube F F is ſecured into the veſſel C in a ſimilar manner.

Uſe and application.

This is the whole conſtruction of my pyrometer. To make uſe of it nothing more is neceſſary than to introduce the platina veſſel A into the furnace the temperature of which is to be learned. The moment the included air is acted upon by the heat it expands, and expels the water up into the graduated tube F F. This riſe will take place accordingly as heat increaſes. If care be taken that the air be cooled in the veſſel C as much as poſſible (which will be the caſe from the large ſurface of water to which it is expoſed), it is obvious that a volume of water equal to the volume of air in the veſſels of platina, can never paſs up into the tube. The refrigeration may be facilitated by the application of vaporizable fluids, ſuch as ether, alcohol, &c.

If the degree of temperature be obtained which the air had before it was ſubjected to the experiment, and a proper allowance be made for the preſſure of the water in F F, the true expanſion of the air may thus be found, and compared with the reſpective temperatures.

XXVI.

New, eaſy, and economical Method of ſeparating Copper from Silver. By *Mr.* GOETTLING [*].

Sulphuric acid uſed inſtead of nitric on account of cheapneſs,

THERE are four methods of ſeparating copper from ſilver, all of which require the alloy to be diſſolved in nitric acid. As this acid is very dear, Mr. Geottling thought of uſing the ſulphuric in its ſtead, which is comparatively very cheap. His ſucceſs perfectly equalled his expectation, and the following is his method:

and with perfect ſucceſs.

Deſcription of the proceſs.

Having aſcertained by the touchſtone, or in any other way, the proportion of ſilver contained in the alloy, take one part of ſulphuric acid for every part of ſilver, and for every part of copper three parts and three-fifths of a part of the ſame acid. Dilute the acid with half its weight of water, and pour it into a matraſs on the alloy reduced to very ſmall pieces. In order to promote the action of the acid, it is of uſe to put one part more to every ſixteen parts of the alloy. The matraſs is then to be placed in a ſand-heat, and the acid brought to a ſtate of ebullition. In two or three hours time the alloy is commonly diſunited and converted into ſulphate, particularly if care be taken to ſtir the maſs from time to time with a glaſs ſpatula. This maſs is thick, and frequently hard. While it is ſtill hot, ſix or eight times its weight of boiling water is to be added to it, and it is to be left ſome time longer on the fire. The ſulphate of copper will be diſſolved, and great part of the ſulphate of ſilver will be precipitated. The operator will now examine whether the whole be completely diſſolved; and if it be, a plate of copper, or ſome pieces of copper or halfpence tied up looſely in a piece of coarſe linen, muſt be ſuſpended in the mixture, and the whole kept boiling for ſome hours. The ſulphate of ſilver will thus be decompoſed, and the ſilver ſeparated in the metallic ſtate.

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[*] Tranſlated from Van Mons's *Journal de Chimie*, Vol. VI. p. 77. Originally publiſhed in the *Taſchen-Buch fuer Scheidekunſtler*, and abridged by Van Mons.

Mode of aſcertaining whether the ſeparation be complete.

To aſcertain whether the ſeparation be complete, a few drops of ſolution of muriate of ſoda are to be dropped into a little of the liquor. If a cheeſelike precipitate be formed, it is a proof, that all the ſilver is not ſeparated, and in this caſe the ebullition with the copper muſt be continued longer. After the whole of the ſilver is ſeparated, the liquor is to be poured off, the precipitated ſilver is to be well waſhed, and the entire ſeparation of the cuprous ſalt is to be aſcertained by the addition of a few drops of liquid ammonia to the water with which the precipitate has been waſhed, which, if it contain any copper, will be rendered blue by the ammonia. After the ſilver is thoroughly freed from the ſulphate of copper, it may be kept in the ſtate of powder as it is, or it may be fuſed with a fourth or at moſt half its weight of ſulphate of potaſh.

May be kept in powder, or fuſed with ſulphate of potaſh.

Blue vitriol obtained equal to the coſt of the acid.

The water poured off is then to be mixed with what was uſed for waſhing the precipitate, and evaporated in a copper pan, ſo as to obtain the ſulphate of copper by cryſtallization. The blue vitriol thus produced will be at leaſt equal in value to the ſulphuric acid employed.

Undiſſolved alloy to be ſet by.

If any parts of the alloy remained undiſſolved, it ſhould be ſeparated by decantation, and reſerved for a future operation.

SCIENTIFIC NEWS.

Extract of a Letter from BRUGNATELLI, *concerning the non-exiſtence of the charged Pile*[*].

VOLTA has made many experiments on piles compoſed of a ſingle metal, and a ſingle wet ſtratum, which, from being inactive by themſelves, become more or leſs active after affording a paſſage for a longer or ſhorter ſpace of time to an electric current ſet in motion by active pile, &c.

Ritter aſſerted that a pile compoſed of one fluid, and one metal, was capable of being charged by another pile, but the fact is, that the fluid is converted into two different fluids by the electric current.

Ritter, the moſt judicious of the galvanic philoſophers of Germany, has aſſerted, as Volta ſays, that the active pile, or common electrometer, tranſmits a real charge to the pile that is itſelf inactive, which it therefore calls the *charged pile.* Volta however has convinced himſelf, that no charge is tranſmitted, but, by virtue of the ordinary chemical action, the electric current being continued, changes the ſingle wet ſtratum interpoſed between two pieces of gold, for example, into two different fluids, one acid, by which the electric current iſſues out of the metal, and the other alcaline, by which it enters; which conſtitutes a pile of the ſecond order, namely, of one metal and two fluids of different natures, the action of which however does not continue long, becauſe the fluids ſoon mix.

Mechanical work by Mr. Gregory.

Mr. GREGORY of the Royal Military Academy, Woolwich, has now in the preſs a Treatiſe on Mechanics, which is intended to be publiſhed in two volumes octavo. The firſt will be devoted chiefly to the theory, and will be divided into five books under the ſeveral heads of ſtatics, dynamics, hydroſtatics, hydrodynamics, and pneumatics. The ſecond volume will be chiefly appropriated to practical and deſcriptive ſubjects, and will commence with general remarks, rules, and tables, relative to the nature, conſtruction, and ſimplification of machinery; the effects of friction, and the rigidity of cords; and eſtimates of the varied energy of different firſt movers, &c. Theſe will be followed by deſcriptions arranged alphabetically, of about 100 of the moſt curious, uſeful, and important machines. In this latter part, Mr. Gregory has been promiſed communications from ſome celebrated civil engineers, ſo that he hopes, on the whole, to render the work in ſome meaſure deſerving the attention of thoſe who are engaged in the cultivation and improvement either of the theory or the practice of mechanics.

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[*] *Van Mons Journal de Chimie*, Vol. VI. p. 132.

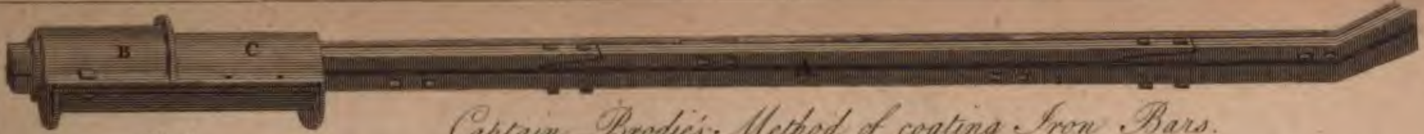

Fig. 1.

Captain Brodie's Method of coating Iron Bars.

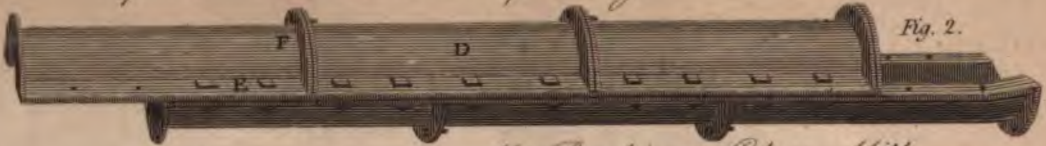

Fig. 2.

Mr. Rawlinson's Colour Mill.

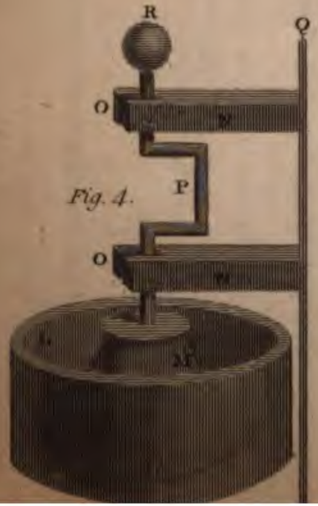

Fig. 4.

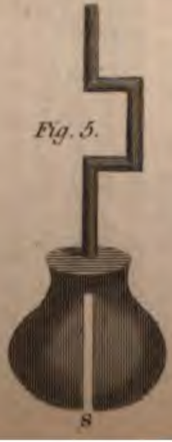

Fig. 5.

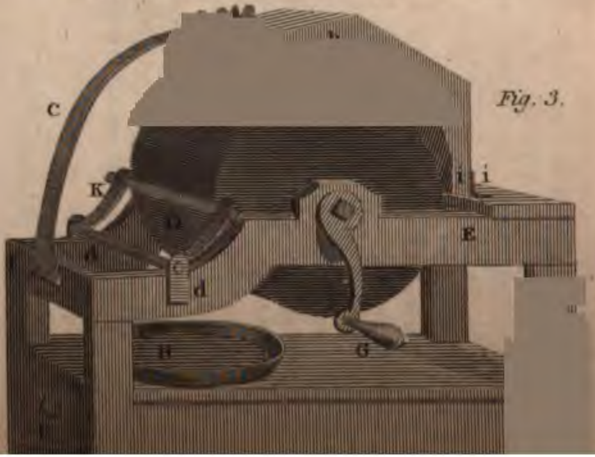

Fig. 3.

A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

JULY, 1805.

ARTICLE I.

Remarks on the Eſtimation of the Strength of Horſes. In a Letter from Mr. O. Gregory, *of the Royal Military Academy, Woolwich.*

To Mr. NICHOLSON.

SIR,

THE remarks of your ingenious correſpondent, Mr. Hornblower, on the various eſtimates of the *Power of a Horſe*, and the abſurdity of adopting a quantity ſo fluctuating and ſo difficult to aſcertain, as a common meaſure by which the powers and effects of ſteam engines and other machines are to be eſtimated and compared, have induced me to throw together a few obſervations on the ſame ſubjects; the theoretic part of which, though familiar to moſt men of ſcience, ſeems not to be always known, or at leaſt recollected, by ſome perſons who are employed in the practice; and which are altogether much at your ſervice for inſertion in the Journal, if you think them likely to be of any utility.

Reference to Mr. Hornblower's paper on horſe powers.

Dr. Deſaguliers has given another eſtimate of the labour of a horſe, beſide that mentioned by Mr. Hornblower, and which indeed does not ſeem very conſiſtent with it; for in vol. II. p. 251. of his Experimental Philoſophy, he affirms that a horſe in

Eſtimate of horſe power by Deſaguliers. 200lb eight hours per day, at 2½ miles per an hour; or 3⅔ feet per ſecond.

—by Sauveur 189lb. avoird. three feet per ſecond,

—by the author 130lb. three feet per ſecond.

an advantageous ſituation, is able to draw 200lbs. eight hours a day, walking at the rate of $2\frac{1}{2}$ miles in an hour, or $3\frac{2}{3}$ feet in a ſecond. This ſtatement of the power of a horſe, though it is not ſo great as that which is arbitrarily aſſumed by Meſſrs. Boulton and Watt, exceeds the determination of M. Sauveur, who eſtimates the mean effort of a horſe at 175 French, or 189 avoirdupois lbs. with a velocity of rather more than three feet per ſecond; and it probably exceeds Mr. Smeaton's ſtatement of 550lbs. moved 40 feet in a minute; though, as will ſoon be ſeen, we are not furniſhed with proper data to inſtitute a comparifon between theſe various reſults. It is probable, however, as obſerved by the ingenious contributor of the article at page 216, vol. IX. of your Journal, that "the loweſt of theſe "performances is more than equal to the average power of a "horſe employed in huſbandry for eight hours per day." So far as my own obſervations on this point extend, I am inclined to conclude that the average work of a ſtout London cart horſe, for eight hours in a day, is little if any more than 130lbs. moved at the rate of three feet in a ſecond, or $2\frac{1}{22}$ miles per hour. But this it would be ridiculous to aſſume poſitively as a univerſal unit of meaſure, in a caſe where the cauſes of variety are ſo numerous, and my opportunities of experiment comparatively few. The eſtimate juſt given, it ſhould be obſerved, is not intended to expreſs what a horſe can draw upon a wheel carriage, where friction alone is to be overcome, after the load is once put into motion, and where a horſe will often draw much more than 1000lbs. but the weight which a horſe would raiſe out of a well, &c. the animal acting by a horizontal line of traction turned into the vertical direction by a ſimple pully or roller, whoſe friction is reduced as much as poſſible.*

Miſapplication of the general principle that gain in power is loſs in time.

Before we can inſtitute any compariſon between the reſults of different experiments, it will always be neceſſary to enquire what machine was interpoſed between the weight moved and the animal, in each caſe, that we may thence deduce the real velocity with which the animal moved, from the velocity of the weight or load given by the obſervations. This is too frequently omitted in conſequence of an implicit reliance upon a maxim, which, though highly uſeful under proper reſtrictions, is far from univerſal in its application. In the caſe before us, if we admit the maxim now alluded to, namely, that *what is gained in power is loſt in time,* with regard to the machine through whoſe intervention the velocity of the weight is rendered different from that of the horſe, it would be unſafe to adopt it in the appreciation of the varied energy of the animal when moving with different velocities. The reaſon of this is obvious. The energy of the horſe is obliged to be employed not only in overcoming the weight or reſiſtance which oppoſes his progreſs, but in part in moving *himſelf;* for the particles which conſtitute his frame poſſeſs weight and inertia, and therefore cannot be put into motion without effort. Hence it follows that there is a certain velocity, which may be denoted by U, with which, when the animal moves, his whole power will be employed in producing his own motion ſolely, without being able to move any other body. If a body whoſe maſs is M be attached to the horſe, ſo that he cannot move without giving an equal velocity to the extraneous body, the ſame effort being employed both in moving the animal and the maſs attached; the velocity V, with which they move muſt neceſſarily be leſs. And if M be farther increaſed while the weight and energy of the horſe continue the ſame, the velocity V will be ſtill farther diminiſhed; and thus as M increaſes V will diminiſh, until when M arrives at a certain magnitude, W, the animal is unable to make any progreſſive motion, and exerts his force at what is called a dead pull. If M exceeds W, then will V become negative, and inſtead of the animal advancing with the load, the load will compel him to move backwards, and no uſeful work can be accompliſhed.

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* The late Mr. More, Sec. to the Society of Arts, found by the interpoſition of a graduated ſpring inſtrument between the horſe and his work, that the re-action was between 70 and 80lb. when the velocity was three miles in an hour. I think the work was ploughing. See Philoſ. Journal, quarto, Vol. III. 136. N.

An horſe, having himſelf to carry, does not exert his force againſt his work only.

Method of computing the effect of horſes.

Now theſe circumſtances may be expreſſed algebraically, by the general formula $M \propto (U-V)^n$, in which the exponent n can only be determined by means of judicious and numerous experiments, where the magnitude of M ſhould be aſcertained for many variable values of V between the terms $V=U$, and $V=0$. From this theorem, following the common rules for the maxima and minima of quantities, it may readily be found that in order to have the *uſeful* work done the greateſt poſſible, we muſt increaſe or decreaſe the weight till V becomes $= \frac{U}{n+1}$, when the performance will be denoted by $\frac{n^{n}}{(n+1)^{n+1}} \times$ W U

And if the value of V thus exhibited be once aſcertained experimentally, we need never be apprehenſive of a material loſs by a ſmall variation from it; for by a well known property of thoſe quantities which admit of a proper maximum and minimum, a value aſſumed at a moderate diſtance from either of theſe extremes will produce no ſenſible change in the effect.

Example.

In ſome of the actions of men, ſuch as dragging a boat along a canal, &c. the value of n in the preceding theorems has been found to be nearly $= 2$. And the draught of horſes is conformable to a law not widely different. The beſt experiments which have yet been made on this point with regard to horſes drawing in nearly rectilinear paths, lead us to conclude that n is then very nearly $= \frac{9}{4}$, in the expreſſion $M \propto (U-V)^{n}$. Aſſuming therefore for the utmoſt walking velocity of a horſe, the value $U = 9$ feet per ſecond, a value which is quite high enough, any propoſed eſtimates of the ſtrength of this animal may be compared with facility. Thus, for example, let us enquire which is greater, the eſtimate of Mr. More (mentioned by Mr. Hornblower) of 80lbs. three miles per hour, or 4⅖ feet per ſecond; or that of 130lbs. moved at the rate of three feet per ſecond? Here we ſhall have

$$(9-3)^{\frac{9}{4}} : (9-4\cdot4)^{\frac{9}{4}} :: 130 : 71\tfrac{1}{2}\text{lbs. nearly.}$$

The operation may eaſily be performed by means of a table of logarithms, and ſhews that the mean eſtimate I have laid down, when reduced to the ſame velocity as that by Mr. Moore, furniſhes a reſult leſs than his by 8½lbs. Which of theſe is the moſt accurate can only be determined by future experiments.

On the power of horſes walking in circular paths;

If, however, either of theſe eſtimates ſhould be adopted, it may be proper to remark that they would not hold with regard to the power of horſes working in circular paths; yet, if it be at all proper to uſe *horſe powers* in eſtimating the energy of machines, it ſeems moſt natural to take theſe powers as exerted by the animal in a round walk; ſo that it is ſtill neceſſary to have a ſeries of experiments to determine the values of n, and the relation of M and V when horſes draw in circular walks of different radii. I ſay, of *different* radii, becauſe it is certain that *cæteris paribus*, the greater the radius of the circle

—of different radii.

in whoſe circumference the animal moves, the leſs fatigue will be occaſioned by that kind of motion. Indeed it is obvious, that ſince a rectilinear motion is the moſt eaſy and natural for the horſe, the leſs the line in which he moves is curved, with the greater facility he will walk over it, and the leſs he need recline from the vertical poſition. Beſides this, with equal velocity at the circumference, the centrifugal force will be leſs in the greater circle, which will proportionally diminiſh the friction of the cylindrical part of the trunnions, and the labour of moving the machine. And farther, the greater the radius of the horſe-walk, the nearer the chord of the circle in which the horſe draws is to coincidence with the tangent, which is the moſt advantageous poſition of the line of traction. Hence it follows, that although a horſe *may* draw in a walk of 18 feet diameter, yet he will work with far greater eaſe in one whoſe diameter is 35 or 40 feet; and it is very deſireable that an experimental enquiry ſhould be made to aſcertain the proportion and abſolute quantity of work in different circles.

The larger walks are moſt advantageous.

Deſcription of a mechanical apparatus for meaſuring the re-action in mills.

I am of opinion that it would not be difficult to make ſome uſeful experiments, while work was actually carrying on at any horſe mill, or machine where horſes are conſtrained to move in a circular walk. The ſimple drawing which accompanies this letter will aſſiſt in conveying a clear idea of the method which I fancy might be advantageouſly adopted. Let AB *Fig.* 1. *Plate* X. be the vertical ſhaft to which the horizontal horſe poles AC, AD, are attached. Let one horſe work the machine by drawing at the ear E; but, inſtead of the tranſverſe bar to which the harneſs is fixed being ſimply hung upon the hook *h*, let a good ſpring ſteelyard be interpoſed between that croſs-bar and the hook, the graduations of which ſhall, when the machinery is put into motion, indicate the reſiſtance (in lbs.) overcome by the animal, including the weight of the maſs moved, the friction, &c. Near the extremity of the oppoſite horſe-pole AD, let there be fixed a ſtrong and correct common ſteelyard, whoſe diviſions ſhall ſhew the various weights from 40 or 50 to 200lbs. and whoſe centre of motion ſhall be at the point *f* on the fixed ſtand. Let the cord *c* which is faſtened to the ſhorter arm of this ſteelyard, paſs (with as little friction as poſſible) over the pulley *p*, and thus, being turned into the horizontal direction, or rather, inclining a little upwards, let it be fixed to the croſs

Deſcription of a mechanical apparatus for meaſuring the re-action in mills.

bar of the harneſs of a ſecond horſe, equal in point of ſtrength to the former. Then if the two horſes, thus attached to the ears E and F, be made to paſs over the walk in the ſame direction, following each other conſtantly at the diſtance of a ſemi-circumference; while that which draws at the ear E overcomes the whole preſſure and reſiſtance oppoſed by the work, the other which draws at F by the cord over the pulley *p*, will raiſe the weight *w* of the ſteelyard; which therefore, by being moved to and fro upon the arm *f i*, may be brought to exhibit an exact counterpoiſe, or meaſure of the exertion and power of the horſe. And in order to enſure the greateſt degree of accuracy in this reſpect, the motion of the two animals, and the poſition of the weight *w*, ſhould be ſo adjuſted, that the ſame weight ſhould be ſhewn by the graduations both of the ſpring and of the lever ſteelyard. The ſhaking of the machinery will in ſome meaſure diſturb the effect; but an ingenious manager of the experiments will find means of checking this: and as to the centrifugal force to which the weight *w* is expoſed, it will never be of any material conſequence in any of the ſlow motions which will be produced by this kind of work.

Each experiment ſhould occupy the ſpace of a fair day's work for the horſes: for the concluſions deduced from ſhorter and irregular efforts are always erroneous in exceſs, and ſhould be guarded againſt. The rate at which the animals move may readily be aſcertained from the known circumference of the walk, and the number of rounds they are obſerved to make in 10 or 15 minutes. Thus, by continuing the experiments day after day, varying the velocity of the motion in ſome caſes, and the radius of walk in others, ſuch a ſeries of reſults might at length be obtained, as would in a great meaſure remove the obſcurity and doubt in which this buſineſs is at preſent enveloped. It is ſcarcely neceſſary to ſuggeſt the propriety of making a few experiments with a view of determining how far a load upon the back of each draught horſe, would aſſiſt him in his labour. Nor can it be requiſite to point out in what way, by means of ſuch ſteelyards properly applied to waggons, &c. upon tolerably ſmooth roads, and two horſes marching abreaſt, (one drawing the load, the other raiſing the weight,) experiments might be inſtituted to aſcertain the magnitude of the efforts of horſes when drawing in rectilinear paths. Judicious experiments having theſe purpoſes in view, would certainly be beneficial, as they would enable us to tell what advantages might be expected from the labours of this uſeful quadruped in different circumſtances. But with reſpect to the adoption of "*horſe power*" as a unit of force in eſtimating the power of ſteam engines, &c. I confeſs that if it were as well known, and as unvariable as the length of the day of the equator, I ſhould feel an averſion to applying it to any ſuch purpoſe. It is a common meaſure arbitrarily adopted, which has no neceſſary connection with the ſubject that is referred to it, which does not in any reſpect facilitate the computation of the powers of an engine, and which may, without proper caution, lead to conſiderable errors in the concluſions deduced from it.

The adoption of an arbitrary unit from the power of an horſe.

Before I cloſe this letter, already perhaps too long, I beg permiſſion to ſay a few words reſpecting the meaſure which is generally employed to determine the mechanical effect produced. This is the meaſure of the deſervedly celebrated Mr. John Smeaton, who ſays that "the weight of a body multiplied by the height through which it deſcends, while driving a machine, is the only proper meaſure of the power expended; and that the weight multiplied by the height through which it is uniformly raiſed is the only proper meaſure of the effect produced." Mr. Smeaton was led to the uſe of this meaſure by his profeſſional habits; and many who in this reſpect pay too great a deference to his authority, have adopted this meaſure as univerſal and preferable to any other. Taking this as a popular meaſure eaſy to recollect, and ſimple in its application, it undoubtedly has its uſes; but in many inſtances it is inadequate to the purpoſe for which it is propoſed. The late Profeſſor Robiſon has ſome excellent obſervations on this ſubject, in the article *Machinery*, *Sup. Encyclopædia Britan.* where he lays down the juſt meaſure to which the ſcientific inveſtigator will generally have recourſe. "We take, ſays he, for the meaſure, (as it is the effect) of exerted mechanical power, the quantity of motion which it produces (or whoſe accumulation it prevents) by its uniform exertion during ſome given time. We ſay *uniform exertion*, not becauſe this uniformity is neceſſary, but only becauſe, if any variation of the exertion has taken place, it muſt be known in order to judge of the power."

Smeaton's meaſure of mechanical power and effect. He ſays it is as the product of the weight into the height paſſed through in either caſe;

rectified by Profeſſor Robiſon.

A ſingle

Smeaton's meaſure is not applicable to an horſe ſuſtaining (but not raiſing) a weight.

A ſingle inſtance may be adduced, to which the meaſure of Mr. Smeaton is inapplicable, and in which we muſt have recourſe to ſome ſuch meaſure as that mentioned by Profeſſor Robiſon. Suppoſe that a horſe while ſtanding ſtill ſuſtains by means of a rope and ſimple fixed pulley, a maſs of a hundred-weight, and thus keeps it ſuſpended at the top of a well, for the ſpace of a minute. Neither the animal nor the weight moves, but ſhall we therefore ſay, in conformity, as it would ſeem, with Mr. Smeaton's meaſure, that there is no power expended, and no effect produced? On the contrary we know there is a power expended, and that the effort if ſufficiently long continued would completely tire the horſe. The effect which is produced is the annihilation of the ſimultaneous action of gravity upon the ſuſpended maſs; conſequently, the effect produced is equal, and contrary to the momentum that would be generated by gravity in the ſpace of a minute. So that $60 \times 32\frac{1}{6} \times 112 = 216160$, is the proper repreſentative of the power expended, as well as of the work done. Were the rope to be cut and the weight ſuffered to fall for a minute, the ſame number would likewiſe denote the labour of the horſe in reſtoring it to its original place, provided that could be accompliſhed in an equal ſpace of time, without the horſe changing his ſituation.

General ſtatement.

It may not, perhaps, be entirely uſeleſs to ſtate this matter rather more univerſally. To this end, let M repreſent any maſs or body, $g = 32\frac{1}{6}$ feet, the velocity communicated to a body falling freely in the firſt ſecond of time, and t' an indefinitely ſmall portion of any time whatever t. Then will $g\,t'$ be the velocity generated in the inſtant t', and $M\,g\,t'$ the correſponding quantity of motion; this, therefore, meaſures the effort which muſt be exerted at each inſtant to ſuſtain the weight, whether that effort be applied immediately, or through the intervention of a ſingle fixed pulley. Hence it follows, that during the whole time t, the force will have conſumed a quantity of motion equal to $\int M\,g\,t' = M\,g\,t$: that is to ſay, if t denote the time at the end of which the agent is no longer able to ſuſtain the maſs M, we may regard $M\,g\,t$ as being an adequate meaſure of the force φ of that agent. If the agent not only prevent the maſs from falling, but actually raiſe it with a given uniform velocity V during the whole time t, then we muſt add the quantity of motion MV to the former, which gives $\phi = MV \times Mgt = M(V \times gt.)$ And laſtly, if the agent poſſeſs inertia, its maſs muſt alſo be conſidered. Thus, in the caſe of a horſe whoſe maſs is H, moving along with the velocity V during the time t, and raiſing the maſs M, we ſhall have $\phi = (M \times H) V \times Mgt$. And from ſimilar principles formulæ may be inveſtigated to repreſent the power of a firſt mover in more complicated caſes.

It will after all, be proper to diſtinguiſh carefully between the quantity of power expended, and that portion of it which is *uſefully* employed: but a due conſideration of this would too widely extend the limits of the preſent communication. Indeed I ought to apologize to yourſelf, and the ſcientific part of your readers, for dwelling ſo long as I have done upon topics which are well known to all who are converſant in the theory of mechanics: but if thoſe, for whoſe uſe this letter is chiefly intended, ſhall derive ſome preciſe information, or add to the ſtock of their practical knowledge, by any hints of mine, I ſhall not fear being heavily cenſured for having entered thus into minutiæ.

Power expended and power uſefully employed require to be diſtinguiſhed.

I am, Sir,

Your's very reſpectfully,

OLINTHUS GREGORY.

Royal Mil. Academy, Woolwich.
June 10th, 1805.

II.

An Account of ſome Analytical Experiments on a Mineral Production from Devonſhire conſiſting principally of Alumine and Water. By HUMPHRY DAVY, *Eſq. F. R. S. Profeſſor of Chemiſtry in the Royal Inſtitution. From the Philoſophical Tranſactions in* 1805.

I. *Preliminary Obſervations.*

THIS foſſil was found many years ago by Dr. Wavell, in a quarry near Barnſtaple: Mr. Hatchett, who viſited the place in 1796, deſcribed it as filling ſome of the cavities and veins in a rock of ſoft argillaceous ſchiſt. When firſt made known, it

History of a Foſſil.

it was conſidered as a zeolite; Mr. Hatchett, however, concluded, from its geological poſition, that it moſt probably did not belong to that claſs of ſtones; and Dr. Babington, from its phyſical characters, and from ſome experiments on its ſolutions in acids, made at his requeſt by Mr. Stocker, aſcertained that it was a mineral body, as yet not deſcribed, and that it contained a conſiderable proportion of aluminous earth.

It is to Dr. Babington that I am obliged for the opportunity of making a general inveſtigation of its chemical nature; and that gentleman liberally ſupplied me with ſpecimens for analyſis.

II. *Senſible Characters of the Foſſil.*

Its ſenſible characters. Radiated hemiſpherical groups; white, ſilky, hard, little tenacious.

The moſt common appearance of the foſſil is in ſmall hemiſpherical groups of cryſtals, compoſed of a number of filaments radiating from a common center, and inſerted on the ſurface of the ſhiſt; but in ſome inſtances it exiſts as a collection of irregularly diſpoſed priſms forming ſmall veins in the ſtone: as yet, I believe, no inſulated or diſtinct cryſtal has been found. Its colour is white, in a few caſes with a tinge of gray or of green, and in ſome pieces (apparently beginning to decompoſe) of yellow. Its luſtre is ſilky; ſome of the ſpecimens poſſeſs ſemi-tranſparency, but in general it is nearly opaque. Its texture is looſe, but its ſmall fragments poſſeſs great hardneſs, ſo as to ſcratch agate.

Other characters.

It produces no effect on the ſmell when breathed upon, has no taſte, does not become electrical or phoſphoreſcent by heat or friction, and does not adhere to the tongue till after it has been ſtrongly ignited. It does not decrepitate before the flame of the blow-pipe; but it loſes its hardneſs, and becomes opaque. In conſequence of the minuteneſs of the portions which it is found, few of them exceeding the ſize of a pea, it is very difficult to aſcertain its ſpecific gravity with any preciſion; but from ſeveral trials I am diſpoſed to believe, that it does not exceed 2,70, that of water being conſidered as 1,00.

III. *Chemical Characters of the Foſſil.*

Chemical habitudes. Soluble in acids and in f. alkalis.

The perfectly white and ſemi-tranſparent ſpecimens of the foſſil are ſoluble both in the mineral acids and in fixed alkaline lixivia by heat, without ſenſibly efferveſcing and without leaving any notable reſiduum; but a ſmall part remains undiſſolved, when coloured or opaque ſpecimens are expoſed to the alkaline lixivia.

A ſmall ſemi-tranſparent piece, acted on by the higheſt heat of an excellent forge, had its cryſtalline texture deſtroyed, and was rendered opaque; but it did not enter into fuſion. After the experiment it adhered ſtrongly to the tongue, and was found to have loſt more than a fourth of its weight. Water and alcohol, whether hot or cold, had no effect on the foſſil. When it was acted on by a heat of from 212° to 600° Fahrenheit in a glaſs tube, it gave out an elaſtic vapour, which when condenſed appeared as a clear fluid poſſeſſing a ſlight empyreumatic ſmell, but no taſte different from that of pure water.

Forge heat gave opacity, but did not fuſe it. Loſs one-fourth.

It emitted water.

The ſolution of the foſſil in ſulphuric acid, when evaporated ſufficiently, depoſited cryſtals which appeared in thin plates, and had all the properties of ſulphate of alumine; and the ſolid matter, when rediſſolved and mixed with a little carbonate of potaſh, ſlowly depoſited octahedral cryſtals of alum. The ſolid matter precipitated from the ſolution of the white and ſemi-tranſparent foſſil in muriatic acid, was in no manner acted upon by ſolution of carbonate of ammonia, and therefore it could not contain any glucine or ittria; and its perfect ſolubility without reſiduum in alkaline lixivia ſhewed that it was alumine.

Sulphuric ſolution afforded ſulphate of alumine.

Muriatic ſolution contained alumine *only*.

When the opaque *varieties* of the foſſil were fully expoſed to the agency of alkaline lixivia, the reſiduum never amounted to more than one-twentieth part of the weight of the *whole*. In the white opaque variety, it was merely calcareous earth, for when diſſolved in muriatic acid, not in exceſs, it gave a white precipitate when mixed with ſolution of oxalate of ammonia, and did not affect ſolution of pruſſiate of potaſh and iron.

The white varieties contained ſome lime:

In the green opaque variety, calcareous earth was indicated by ſolution of oxalate of ammonia: and it contained oxide of mangancſe; for it was not precipitated by ſolution of ammonia; but was rendered turbid, and of a gray colour, by ſolution of pruſſiate of potaſh and iron.

and the green, alſo manganeſe:

The reſiduum of the alkaline ſolution of the yellow variety, when diſſolved in muriatic acid, produced a ſmall quantity of white ſolid matter when mixed with the ſolution of the oxalate

the yellow, a trace of iron.

of ammonia, and gave a light yellow precipitate by expoſure to ammonia; but after this, when neutralized, it did not affect pruſſiate of potaſh and iron, ſo that its colouring matter, as there is every reaſon to believe, was *oxide of iron*.

IV. *Analyſis of the Foſſil.*

Analyſis of 80 grains.
1. Expoſure to heat.

Eighty grains of the foſſil conſiſting of the whiteſt and moſt tranſparent parts that could be obtained, were introduced into a ſmall glaſs tube having a bulb of ſufficient capacity to receive them with great eaſe. To the end of this tube, a ſmall glaſs globe attached to another tube, communicating with a pneumatic mercurial apparatus, was joined by fuſion by means of the blow-pipe.

The bulb of the tube was expoſed to the heat of an Argand's lamp; and the globe was preſerved cool by being placed in a veſſel of cold water. In conſequence of this arrangement, the fluid diſengaged by the heat, became condenſed, and no elaſtic matter could be loſt. The proceſs was continued for half an hour, when the glaſs tube was quite red.

Water came over. 19 grains very ſlightly acid.

A very minute portion only of permanently elaſtic fluid paſſed into the pneumatic apparatus, and when examined, it proved to be common air. The quantity of clear fluid collected, when poured into another veſſel, weighed 19 grains, but when the interior of the apparatus had been carefully wiped and dried the whole loſs indicated was 21 grains. The 19 grains of fluid had a faint ſmell, ſimilar to that of burning peat; it was tranſparent, and taſted like diſtilled water: but it ſlightly reddened litmus paper. It produced no cloudineſs in ſolutions of muriate of barytes, of acetite lead, of nitrate of ſilver, or of ſulphate of iron.

2. Solution of the reſidue in ſulph. acid: precipitation and re-ſolution by alkali. One grain and a quarter of lime remained undiſſolved.

The 59 grains of ſolid matter were diſſolved in diluted ſulphuric acid, which left no reſiduum; and the ſolution was mixed with potaſh, in ſufficient quantity to cauſe the alumine at firſt precipitated again to diſſolve. What remained undiſſolved by potaſh, after being collected and properly waſhed, was heated ſtrongly and weighed; its quantity was a grain and quarter. It was white, cauſtic to the taſte, and had all the properties of lime.

3. Nitric acid was added in exceſs, and then carbonate of am-

The ſolution was mixed with nitric acid till it *became ſour*. Solution of carbonate of ammonia was then poured into it till the effect of decompoſition ceaſed. The whole thrown into a

filtrating

trating apparatus left ſolid matter, which when carefully waſhed and dried at the heat of ignition, weighed 56 grains. They were pure alumine: hence the general reſults of the experiments, when calculated upon, indicated for 100 parts of this ſpecimen,

monia. The precip. of alumine, after ignition, was 56 grains.

Of alumine - -	70
Of lime - - -	1.4
Of fluid - -	26.2
Loſs - - -	2.4

Component parts.

The loſs I am inclined to attribute to ſome fluid remaining in the ſtone after the proceſs of diſtillation; for I have found, from ſeveral experiments, that a red heat is not ſufficient to expel all the matter capable of being volatilized, and that the full effect can only be produced by a ſtrong white heat.

The loſs appears to have been volatilizable matter.

Fifty grains of a very tranſparent part of the foſſil, by being expoſed in a red heat for fifteen minutes, loſt 13 grains; but when they were heated to whiteneſs, the deficiency amounted to 15 grains, and the caſe was ſimilar in other trials.

Different ſpecimens of the foſſil were examined with great care, for the purpoſe of aſcertaining whether any minute portion of fixed alkali exiſted in them; but no indications of this ſubſtance could be obſerved; the proceſſes were conducted by means of ſolution of the unaltered foſſil in nitric acid; the earths and oxides were precipitated from the ſolution by being boiled with carbonate of ammonia; and after their ſeparation, the fluid was evaporated to dryneſs, and the nitrate of ammonia decompoſed by heat, when no reſiduum occurred.

The mineral contains no alkali.

A comparative analyſis of 30 grains of a very pellucid ſpecimen was made by ſolution in lixivium of potaſh. This ſpecimen loſt 8 grains by long-continued ignition, after which it eaſily diſſolved in the lixivium by heat, leaving a reſiduum of a quarter of a grain only, which was red oxide of iron. The precipitate from the ſolution of potaſh, made by means of muriate of ammonia, weighed, when properly treated, 21 grains.

Other analyſes.

Several ſpecimens were diſtilled in the manner above deſcribed, and in all caſes the water collected had ſimilar properties. The only teſt by which the preſence of acid matter in it could be detected, was litmus paper; and in ſome caſes the effect upon this ſubſtance was barely perceptible.

V. General

V. *General Obſervations.*

The acid matter in the water was not nitric, mur. or ſulph.

I have made ſeveral experiments with the hope of aſcertaining the nature of the acid matter in the water; but from the impoſſibility of procuring any conſiderable quantity of th foſſil, they have been wholly unſucceſsful. It is, however evident, from the experiments already detailed, that it is no one of the known mineral acids.

It is foreign to the ſtone.

I am diſpoſed to believe, from the minuteneſs of its proportion, and from the difference of this proportion in differen caſes, that it is not eſſential to the compoſition of the ſtone and that, as well as the oxide of mangneſe, that of iron and the lime it is only an accidental ingredient, and on thi idea the pure matter of the foſſil muſt be conſidered as a chemical combination *of about thirty parts of water and ſeventy o alumine.*

Alumine has an affinity for water.

The experiments of M. Theodore de Sauſſure on the precipitation* of alumine from its ſolutions, have demonſtrated the affinity of this body for water; but as yet I believe no aluminous ſtone, except that which I have juſt deſcribed, ha been found, containing ſo large a proportion of water, a thirty parts in the hundred.

Diaſpore exam. by Vauquelin, contains 80 alumine and 16 water.

The diaſpore, which has been examined by M. Vauquelin, and which loſes fixteen or ſeventeen parts in the hundred b ignition, and which contains nearly eighty of alumine, and onl three of oxide of iron, is ſuppoſed by that excellent chemiſt t be a compound of alumine and water. Its phyſical and chemical characters differ however very much from thoſe of th new foſſil, and other reſearches are wanting to aſcertain whether the part of it volatilized by heat is of the ſame kind.

Corniſh mineral reſembling the ſubject of this paper.

I have examined a foſſil from near St. Auſtle, in Cornwall very ſimilar to the foſſil from Barnſtaple in all *its general chemical characters;* and I have been informed, that an analyſi of it, made by the Rev. William Gregor ſome months ſince proves that it conſiſts of ſimilar ingredients.

Propoſed names.

Dr. Babington has propoſed to call the foſſil from Devonſhire *Wavellite*, from Dr. Wavell, the gentleman who diſcovere it; but if a name founded upon its chemical compoſition b preferred, it may be denominated *Hydrargillite*, from ὕδω water, and ἄργιλλος clay.

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* Journal de Phyſique, Tom. LII. p. 280.

III.

On the Aberrations of Light paſſing through Lenſes. By Mr. EZEKIEL WALKER.

Newton's diſcovery of the aberrations of coloured rays.

THE diſcovery of the aberration of the rays of light, cauſed by their unequal refrangibility, formed a new area in the ſcience of optics. It is the foundation of all *Sir Iſaac Newton's* diſcoveries in light and colours; and alſo the foundation of moſt of the uſeful improvements in the conſtruction of optical inſtruments, that have done ſo much honour to our country. And this ſcience may ſtill derive further improvements from the ſame diſcovery, not only in the conſtruction of inſtruments, but alſo in explaining ſome curious phenomena in nature.

But before I attempt to ſhow how the uſe of this property of viſion may be extended, it ſeems neceſſary to give a ſhort account of that kind of aberration which ariſes from the unequal refrangibility of the differently coloured rays of light: the other aberration, or that which is cauſed by the ſpherical figure of the lens, is not here conſidered as being inconſiderable when compared with the former.

Aberration of light that paſſes through a lens.

Therefore let A C B, *Fig.* 2, *Plate* X. repreſent a plano-convex lens; P A and R B two pencils of white or compound rays of light, falling upon it at the points A and B in a direction parallel to its axis. Alſo let A xv and B yv be the red or leaſt refrangible rays, and A gy and B qx the violet, or moſt refrangible. The red ray from A will cut the violet ray from B at the point x, and the red ray from B will cut the violet ray from A at the point y; through theſe interſections draw the line xy, and this line will be the diameter of the leaſt circular ſpace into which all the rays that fall upon the lens, parallel to its axis, can be collected. And this circle, which for brevity's ſake is called the circle of aberration, is the true focus of the lens or place where the image of the object is formed.

Let the ſine of incidence going out of glaſs be n, the ſines of refraction (into air) of the leaſt and the moſt refrangible rays be p and q; then if a plano-convex lens be expoſed with the plane ſide to the ſun, the diameter of the circle of aberration xy (or image of the ſun formed of rays of different refrangibility) is to the diameter of the lens A B, as $q-p$ to $q \times p-2n$.*

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* *This theorem is well known* to mathematicians.

From this given ratio of A B to xy it follows, that the image of the ſun within a teleſcope varies with the aperture of the object glaſs.

Sir Iſaac Newton found by moſt accurate experiments that where the ſine of incidence was 50, the ſines of refraction of the red and the violet rays were 77 and 78. Hence $q-p$ is to $q+p-2n$ as 1 to 55. And therefore A B : xy : : 55 : 1; or $xy = \frac{AB}{55}$.

To elucidate this theorem by examples, let the diameter of the object glaſs of a teleſcope, which is 4 inches, be contracted to 3 inches, and afterwards to 2; then the diameters of the circles by aberration formed by parallel rays, will be $\frac{4}{55} = .072$, $\frac{3}{55} = .054$, and $\frac{2}{55} = .036$ reſpectively.

The ſame Property of Viſion demonſtrated otherwiſe.

Thus, let n repreſent the ſine of incidence, and p and q the ſines of refraction, as before.

The ſine of incidence of every ray, is to its ſine of refraction in a given ratio.*

And the ſine of incidence of the extreme ray P A, varies with the aperture of the lens. For n becomes leſs as P A approaches the axis of the lens E v.

Therefore the ſines of refraction p and q, the angle x A y, and its ſubtenſe xy increaſe or decreaſe with A B. Conſequently the image of the ſun or moon, upon the retina increaſes or decreaſes in magnitude with the pupil of the eye.

Deduction; Now as the rays of artificial light are differently refrangible, it is evident from the given ratio of A B to xy, in which they increaſe or decreaſe at the ſame time that the image of a candle formed in the focus of a convex lens decreaſes with the aperture of the glaſs. For the rays of the ſun and the light of a candle are both governed by the ſame law, in the formation of images in the focus of a lens; but this law does not obtain in the ſame degree in both objects, in conſequence of the rays of the latter being in a more diverging ſtate than thoſe of the former.

—applied in ſupport of the author's experiments. Hence the truth of the reſult of my experiments, which were publiſhed in Vol. IX. page 164 of this Journal, is proved from the diſcoveries of *Sir Iſaac Newton.*

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* Newton's Optics, page 64.

What I have further to advance on this ſubject muſt be reſerved until ſome future opportunity, as it would exceed the limits of this paper.

E. WALKER.

Lynn, May 16, 1805.

IV.

New Method of decompoſing the Sulphate of Barytes for preparing the Muriate of that Earth, and Preparation of the Muriate. By Mr. GOETTLING.*

Muriate of barytes much uſed.

THE muriate of barytes is now in ſuch general uſe, that every improvement in the mode of preparing it muſt meet a favourable reception. This will render the new method of Mr. Goettling acceptable to the public.

Decompoſition of ſulphate of barytes by charcoal troubleſome and incomplete.

The decompoſition of ſulphate of barytes by means of charcoal requires a ſtrong fire continued a long time, and never ſucceeds completely. This is owing on the one hand to the ſtrongly oxigenated quality of the acidifying principle in the ſulphuric acid, ſo that in its tranſlation to the charcoal it gives out but little caloric; and on the other hand to the difficulty of imparting a certain degree of heat to a mixture, into which a large quantity of a body that is ſo bad a conductor of heat as charcoal enters. To remedy the firſt of theſe defects, I had already propoſed to increaſe the proportion of charcoal a little, and to incorporate with the mixture of charcoal and ſulphate of barytes a twentieth of nitrate of potaſh. To remedy the ſecond, Mr. Goettling adviſes to add muriate of ſoda to the mixture, which ſerves at the ſame time as a conductor of heat and a flux. The following is his method.

Remedies: to uſe more charcoal and nitrate of potaſh; —and muriate of ſoda.

Mr. Goettling's proceſs. Sulphate of barytes with muriate of ſoda and ſome charcoal are heated together.

Four parts of native ſulphate of barytes in fine powder are to be mixed with one part of muriate of ſoda and half a part of charcoal powder. This mixture is to be preſſed hard into a Heſſian crucible, and expoſed for an hour and half to a red heat in a good wind furnace. After it has grown cold, the

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* Tranſlated from Van Mons's *Journal de Chimie*, Vol. VI. p. 80. Originally publiſhed in the *Taſchen-Buch fuer Scheidekuenſtler*.

maſs

The ſaline maſs is then to be diſſolved.

maſs is to be reduced to a coarſe powder, and boiled for a moment with ſixteen parts of water. The liquor is then to be filtered, and kept in well ſtopped bottles.

The time of expoſure to heat may be ſhortened to one half, if the quantity of muriate of ſoda be doubled, and the matter occaſionally ſtirred. In this caſe too, double the quantity of water ſhould be uſed to lixiviate the maſs.

An addition of muriatic acid expels the ſulphureous acid and leaves muriates of barytes and of ſoda.

To prepare muriate of barytes with this lixivium of ſulphuret of barytes, which at the ſame time holds in ſolution muriate of ſoda, muriatic acid is to be added in ſeparate portions, till ſulphurated hidrogen gas is no longer extricated. The liquor is then to be filtered, a little hot water is to be poured on the reſiduum, and the liquor is to be evaporated to a pellicle. The lixivium being then filtered afreſh, is to be ſet to

Theſe cryſtals of muriate of barytes are ſeparated by cooling after evaporation.

cryſtallize; the muriate of ſoda, which is much more ſoluble in water than the muriate of barytes, and not more ſoluble with heat than without, is not depoſited by cooling, and the muriate of barytes cryſtallizes alone.

The remaining lixivium is to be evaporated and ſet to cryſtallize again, and this is to be repeated till no more cryſtals of muriate of barytes are formed.

The barytic ſalt is perfectly white and pure.

The barytic ſalt thus obtained, if care be taken not to employ an exceſs of muriatic acid, is perfectly white, on account of the hydroſulphuret, by which the iron and other metallic ſubſtances are precipitated. To be more certain that it contains no muriate of ſoda, the different products of the cryſtallization ſhould be mixed together, diſſolved, and re-cryſtallized.

V.

On the Action of Platina and Mercury upon each other. By RICHARD CHENEVIX, *Esq. F. R. S. M. R. I. A, &c. From the Philoſophical Tranſactions for 1805, p. 104.*

Freyberg, June 3d, 1804.

Reference to the author's paper on palladium.

ON the 12th of May, 1803, I had the honour of preſenting a Paper to the Royal Society,* the object of which was to diſcover the nature of palladium, a ſubſtance juſt then announced to the public as a new ſimple metal. The experiments which I had made for this purpoſe led me to conclude that palladium was not what it had been ſtated to be, but that it was a compound of platina and mercury.

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* Inſerted in our Journal, Vol. VII. 85, 176.

It was natural to ſuppoſe that a ſubject ſo likely to ſpread its influence throughout the whole domain of chemiſtry, and which tended even to the ſubverſion of ſome of its elements, would awaken the attention of philoſophers. We find accordingly, that it has become a ſubject of enquiry in England, France, and Germany; but the experiments which I had recommended as the leaſt likely to fail, have been found inſufficient to inſure the principal reſult; and I have had the mortification to learn that they have been generally unſucceſsful. I have even reaſon to believe that the nature of palladium is ſtill conſidered by chemiſts, at leaſt with a very few exceptions, as unaſcertained; and that the fixation of mercury by platina is by many regarded as viſionary.

Attempts to repeat his experiments have been unſucceſsful.

The firſt doubts were manifeſted in England; and Dr. Wollaſton very early denied the accuracy of my inquiries. But as he has not publiſhed his experiments, I have had no opportunity of diſcuſſing them. His opinion, however, muſt have ſuch weight in the learned world, that I ſhould have neglected a material fact in the hiſtory of palladium, if I had not mentioned it in this place.

Dr. Wollaſton denies their accuracy, but has not yet publiſhed his own.

In France the compound nature of palladium has been more generally credited. When the National Inſtitute was informed of my experiments, a report was ordered to be made upon them, and M. Guyton was the perſon appointed for the purpoſe. He repeated ſome of the experiments, and produced ſome of his reſults. His general concluſion was the ſame as mine.

Guyton, in France, coincides with the author.

Meſſrs. Vauquelin and Fourcroy then undertook the ſubject, and they were led by it to the confirmation of the recent diſcovery of Monſ. Deſcotils. The exiſtence of a new metal, which that chemiſt had found in crude platina, received great ſanction from their experiments; and thus the diſcuſſion upon palladium has eſtabliſhed a fact which will be conſidered as intereſting, but which would be much more ſo, were we not already overburthened with ſubſtances which our preſent ignorance obliges us to acknowlege as ſimple.

Vauquelin and Fourcroy,

—infer that palladium contains no mercury; but is platina with the new metal of Deſcotils.

No ſooner were theſe celebrated chemiſts convinced of the exiſtence of a new metal in platina, than they concluded that it muſt play a principal part in the compoſition of palladium. Shortly after this, in a note to a letter from M. Prouſt to M. Vauquelin, in which M. Prouſt expreſſes his aſtoniſhment concerning all he has read upon palladium, Meſſ. Fourcroy and Vauquelin further declare, as their opinion, that this compound metal does not contain mercury, but is formed of platina and the new metal. Whether this new ſubſtance does or does not play a principal part in the formation of palladium, could not be aſcertained at the time my experiments were made, becauſe the new metal itſelf was not then known. But from all that Meſſ. Fourcroy and Vauquelin have ſtated, in ſuch of their different memoirs upon this ſubject as I have ſeen, the grounds of their ſuppoſition have not appeared. May we not refer their opinion, then, to that common propenſity of the mind, againſt which M. Fourcroy has himſelf warned us with equal juſtneſs and eloquence on another occaſion, namely, a proneneſs to be allured by novelty beyond the bounds of rational belief, and to convert principles which are new into principles of univerſal influence.

Meſſ. Roſe and Gehlen,

Meſſ. Roſe and Gehlen* were the firſt among the German chemiſts who inſtituted experiments upon palladium; and M. Richter has alſo publiſhed a paper on the ſame ſubject.

—attempted without ſucceſs to form palladium by precipitating a mixed ſolution of platina and mercury by gr. ſulphate of iron;

The firſt attempt of Meſſ. Roſe and Gehlen to form palladium was by the precipitation of a mixed ſolution of platina and mercury by green ſulphate of iron. Their reſult was preciſely that which I had obſerved when my operations failed altogether, and which of courſe was the moſt frequent. This method was repeated twice. The ſecond time the precipitate of platina and mercury was boiled with muriatic acid, in order to free it from iron; but the latter trial was not more ſucceſsful than the former.

—and alſo by paſſing ſulphuretted hidrogen through the mixed ſolution;

Their third experiment was, what they have called, a repetition of that in which I had obtained palladium by paſſing a current of ſulphuretted hydrogen gas through a mixed ſolution of platina and mercury. Their method was the following.

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* Neues Algemeines Journal der Chemie herauſgegeben von Hermſtadt, Klaproth, Richter, Scherer, Tromſdorff, und Gehlen. Erſten bandes funftes heft.

They diſſolved one hundred and fifty grains of platina with four hundred and fifty of mercury, and added a ſolution of hydroſulphuret of potaſh. They obtained a precipitate which, at firſt, was black, afterwards gray; but the whole became black by being ſtirred. To be certain that all the metal was precipitated, they added an exceſs of ſulphuret of potaſh, and perceived that a part of the precipitate was re-diſſolved. The liquor was then filtered, and to that part of it, which contained the re-diſſolved precipitate, an acid was added. From this proceſs they obtained a yellow precipitate weighing ninety-one grains; and fifty grains of this expoſed to a ſtrong heat, left three-eights of a grain of platina. They obtained no palladium from that part of the precipitate which had not been re-diſſolved; and the reſult of the experiment was complete failure.

by adding hydroſulph. of potaſh to the m. ſolution;

—but they obtained no palladium.

I ſhall not make any obſervation upon the iſſue of this proceſs, ſince, in this caſe, the beſt conducted is but too liable to be unſucceſsful, and that without any apparent fault in the operator. But as it has been given as a repetition of one of mine, it may not be fruitleſs to examine how far the repetition was exact.

I had paſſed a current of ſulphuretted hydrogen gas through a mixed ſolution of platina and mercury, by which means they were precipitated together. My object was ſo intimately to combine ſulphur with theſe metals, that when expoſed to heat, they might (if I may be allowed the expreſſion) be in chemical contact with it at the moment of their naſcent metallic ſtate; and as a low temperature ſuffices, as well to reduce thoſe metals, as to combine palladium with ſulphur, I hoped that thoſe effects might be produced before the total diſſipation of the mercury. How far my expectation was fulfilled has been ſtated in my former paper.

They did not exactly repeat the author's experiment. He uſed the pure gas; they added an alkali;

The ſulphuretted hydrogen gas which Meſſrs. Roſe and Gehlen preſented to thoſe metals was combined with potaſh. Now, in the courſe of docimaſtic lectures annually delivered by M. Vauquelin at the Ecole des Mines in Paris, when he was Profeſſor at that eſtabliſhment, it was his conſtant cuſtom to exhibit an experiment to prove that mercury, precipitated from its ſolution by many of the alkaline and earthy hydroſulphurets, was rediſſolved by adding an exceſs of them.

—and this laſt ſubſtance acting not only on mercury;

—but on the platina;

—would have an effect nearly oppoſite to that of the author's proceſs.

It is moreover well known, that there is a ſtrong affinity between potaſh and the oxide of platina, and alſo that when thoſe ſubſtances are brought together in ſolution, a triple ſalt, but little ſoluble, is the reſult. It was to avoid theſe difficulties that I had employed uncombined ſulphuretted hidrogen gas; for the method adopted by Meſſ. Roſe and Gehlen appearing to me to be the application of two divellent forces, I preſumed that it would produce a ſeparation. The reſult of their experiment, which, it appears from their paper, they had not anticipated, ſhews the neceſſity of the precaution I had uſed. The operation which they performed to unite platina and mercury was, in fact, nearly the reverſe of that which they ſuppoſed they had repeated from me, and might have been applied perhaps with a better proſpect of ſucceſs towards the decompoſition of palladium.

They could not fuſe platina.

The author uſed borax.

Meſſ. Roſe and Gehlen ſeem, in many parts of their paper, to queſtion my having fuſed platina; and inform us that although they had expoſed this metal in the furnace of the Royal Porcelain Manufactory of Berlin, in which Wedgewood's pyrometer ceaſed to mark the degree of heat, they could not accompliſh its fuſion. Many of my friends in England have however ſeen the buttons which I obtained, and which were not few in number. The flux which I had uſed was borax. But no mention is made in any one of the operations of Meſſ. Roſe and Gehlen of borax having been employed.

Particular account of his proceſs.

A Heſſian crucible was lined with lamp black;

In many of their attempts they obtained an irregular and porous maſs, which of courſe was of a ſpecific gravity much inferior to that of platina; and it might be inferred from their paper that the diminution of ſpecific gravity, which I had obſerved, was owing to the ſame cauſe. It is true, not only that I had very often obtained ſuch a maſs, but that I had frequently alſo obſerved no diminution whatſoever in the ſpecific gravity of the button which reſulted from my operations. But all thoſe upon which I had founded the concluſions alluded to by Meſſ. Roſe and Gehlen were performed in the following manner, and have been repeated ſince. A Heſſian crucible was filled with lamp-black, and the contents preſſed hard together. The lamp-black was then hollowed out to the ſhape of the crucible as far as one-third from the bottom, leaving that much filled with the compreſſed materials; this lining, which adhered ſtrongly to the ſides of the crucible, was made extremely thin, in order not to obſtruct the paſſage of caloric. A cylindrical piece of wood, as a pencil, was then forced into the centre of the thick maſs of lamp-black at the bottom, and the diameter of this rod was determined by the quantity of metal to be fuſed, or varied according to other circumſtances at pleaſure. In general the axis of the cylindrical hole was about three or four times the diameter of the baſis. After withdrawing the rod the crucible was about half filled with borax. Upon this was placed the metal to be fuſed; and if it had been before melted into a cylindrical form, the axis of the metallic cylinder was placed horizontally, and was of courſe perpendicular to the axis of the cylindrical excavation at the bottom of the crucible. More borax was then added to cover the piece of metal, and another quantity of lamp-black was preſſed hard over the whole in order to keep it tight together. An earthen cover was finally luted to the crucible, and in this ſtate it was expoſed to heat in a forge, in which upon another occaſion, I had, in the preſence of Meſſ. Hatchett, Howard, Davy, and others, completely melted a Heſſian crucible lined and prepared in the ſame manner. The fuel which I uſed was the patent coak of Meſſ. Davey and Sawyer. In the preſent experiments I moderated the heat ſo as not materially to injure the crucible, and upon taking it out of the fire, the lining was generally found ſo compact and ſo firm that it remained in a ſolid maſs after the crucible was broken. When the metallic cylinder occupied the ſpace at the bottom, it was natural to ſuppoſe that it had been fuſed; becauſe in no other ſtate but that of liquidity could it have run into the mould. In order however to prevent all objections I had the precaution to make the hole of a different diameter from the metallic cylinder, and to obſerve whether the neceſſary change in the ſhape of the latter enſued. If after ſuch a teſt, repeated as often as required, I perceived that the metal did not vary in its ſpecific gravity, I thought myſelf authoriſed to conclude that it was exempt from air.

—a ſmall cylindrical cavity was made in the lower part of the lining;

—the metal occupied the middle of a maſs of borax that filled up the cavity of the veſſel;

—ſtrong heat was applied;

—and when the metal loſt its firſt dimenſions and occupied the cylindrical cavity it muſt have been fuſed.

M. Richter ſays that he had hoped to have put himſelf in poſſeſſion of a conſiderable piece of palladium by repeating with minute accuracy the proceſs which I had recommended as the beſt. He precipitated a mixed ſolution of platina and mercury by a ſolution of green ſulphate of iron; and after varying the ſubſequent operations, to which he ſubmitted the product

M. Richter's attempt and concluſions;

product he had obtained by this method, he was led to the following important concluſions amongſt others of leſs conſequence. 1ſt, That two metals, the ſeparate ſolutions of which are not acted upon by a third body, may be acted upon, and even reduced to the metallic ſtate, by that ſame body when preſented to them in one and the ſame ſolution.

2dly. That mercury is capable of entering into combination with platina ſo, that it cannot afterwards be ſeparated by fire. From the firſt of theſe concluſions it is evident, that metals in their metallic ſtate are not incapable of chemical action upon each other; and from the ſecond, that mercury can be fixed (it is purpoſely that I uſe the alchemical expreſſion) by platina.

Attempts of Tromſdorff and Klaproth: not ſucceſsful.

In addition to the chemiſts abovementioned, I muſt name two more who in Germany have been occupied by palladium. M. Tromſdorff, in a letter to the authors of the journal already quoted, mentions his having made ſome fruitleſs attemps to form this combination; and Klaproth, in a letter to M. Vauquelin publiſhed in the *Annales de Chemie*, for Ventoſe, an 12, likewiſe ſays that he could not ſucceed in producing palladium.

Remarks. The chemiſts who expected to ſucceed in a few trials, could not have attended to the author's paper;

Meſs. Roſe and Gehlen, as well as M. Richter, had conceived from my Paper a reliance on the ſucceſs of their experiments, which no words of mine had authoriſed, and have accuſed me of enforcing the truth of my reſults with a degree of certainty which their obſervations do not countenance. M. Richter ſuppoſed that the formation of palladium was attended with no difficulty; and in general they have laid ſo much ſtreſs upon this charge, that I ſhould be inclined to think my Paper had not been read by theſe chemiſts. In referring to it again, I find there is hardly a page in which I do not mention ſome failure, and no experiment, of the very few which occaſionally ſucceeded, is related without my ſtating at the ſame time that it was repeatedly unſucceſsful. As far as regards palladium, it is rather a narration of fruitleſs attempts than a deſcription of an infallible proceſs, and more likely to create averſion to the purſuit than to inſpire a confidence of ſucceſs. The courſe of experiments which I had made, as well before as after reading my Paper to the Society, took me up more than two months, and employed me from twelve to ſixteen hours almoſt every day. I had frequently ſeven

He made many hundreds of experiments, and ſucceeded completely but four times, ſeven or eight operations in the forge to perform daily, and I do not exaggerate the number of attempts I made during this time, as well in the dry as in the humid way, in ſtating them to have been one thouſand. Amongſt theſe I had four ſucceſsful operations. I perſevered, becauſe even in my failures I ſaw ſufficient to convince me that I ſhould quit the road to truth if I deſiſted. After all my labour and fatigue I cannot ſay that I had come nearer to my object, of obtaining more certainty in my proceſſes. Their ſucceſs was ſtill a hazard on the dice, againſt which there were many chances; but till others had thrown as often as I had done, they had no ſolid right to deny the exiſtence of ſuch a combination. On this foundation none, I believe, have eſtabliſhed ſuch a right. Meſs. Roſe and Gehlen do not ſay how often their experiments were repeated; but it is probable that if they had been performed very often, theſe authors would not have neglected to mention it. M. Richter ſtates his merely as preparatory to more extenſive reſearches; and M. Tromſdorff, as well as M. Klaproth, mention little more than the fact. If the German chemiſts have concluded againſt my reſults, they have done ſo without juſt grounds, and without having beſtowed upon them that labour and aſſiduity for which they are uſually ſo remarkable.

M. Ritter the great galvaniſt, has eſtabliſhed the order of galvanic relation in bodies.

In this ſtate of uncertainty the compound nature of palladium received an indirect, but a very able, ſupport from ſome experiments of M. Ritter, the celebrated Galvaniſt of Jena. M. Ritter had aſcertained the rank which a greater number of ſubſtances hold in a galvanic ſeries, arranged according to the property they poſſeſs of becoming poſitive or negative when in contact with each other. He had eſtabliſhed the following order, the preceding ſubſtance being in a *minus* relation to that which comes next. Zinc, lead, tin, iron, biſmuth, cobalt, antimony, platina, gold, mercury, ſilver, coal, galena, cryſtallized tin ore, kupfer nickel, ſulphur, pyrites, copper pyrites, arſenical pyrites, graphite, cryſtallized oxide of manganeſe. He had the goodneſs to try palladium in my preſence, and found it to be removed, not only from what I believed to be its conſtituent parts, but altogether from among the metals, and to ſtand between arſenical pyrites and graphite. This reſult led Mr. Ritter into a new and general train of reaſoning, and induced him to undertake the examination of a great

He found palladium to be removed in that order from among the metals to the compound bodies.

Other intereſting facts.

great number of alloys, and of a variety of amalgams. He conſidered the ſubject as a philoſopher; and his operations were thoſe of a conſummate experimentaliſt. It would be doing him an injuſtice to attempt an extract of his ingenious paper, which contains a ſeries of the moſt intereſting experiments. I ſhall merely obſerve for the preſent purpoſe, that it very rarely happened that the mixture of two metals bore any determinate relation to the ſame metals when ſeparate; that in every caſe the ſmalleſt variation in the proportions produced the moſt marked effects; and that M. Ritter has furniſhed us with an inſtrument calculated to detect the preſence of ſuch ſmall quantities as have hitherto been conſidered as out of the reach of chemiſtry. As palladium preſents a very ſtriking inſtance of the anomaly, to which all compounds ſeem to be more or leſs ſubject, by being removed altogether from the ſeries of ſimple metals, this may ſerve to ſupport the other proofs of its compound nature.

The objectors to the author's concluſions have not paid attention to the repeated failures of all his methods.

One of the principal objections of thoſe who diſpute the truth of my concluſions with reſpect to palladium, is grounded upon the repeated failure of all the methods I had made uſe of in forming it; but this cannot be of very great weight, when we conſider the uncertainty of many other operations of chemiſtry. The moſt ſimple are ſometimes liable to fail: and the eaſieſt analyſes have often given different products in the hands of different chemiſts, who yet enjoy indiſputable and equal rights to the title of accuracy. The progreſs which we have made in ſome parts of the ſcience has not removed the obſtacles which impede our advancement in others. We have no method of proving the truth of an experiment except by repeating it: yet this often tends to ſhow nothing more than contradictory reſults, and conſequently the fallibility of the art.

A recent caſe in chemiſtry which is perfectly analogous to this.

But a recent caſe has occurred which is perfectly analogous to that of palladium. A few years ago Profeſſor Lampadius, in diſtilling ſome ſubſtances which contained ſulphur and charcoal, obtained a liquid product of a peculiar nature. He repeated his experiments, but in vain: and after many fruitleſs attempts abandoned his reſearches, and confined himſelf to ſtating the fact to the chemical world. Little notice was taken of it, and not much intereſt excited by an experiment ſo likely to fail. Some time after this Meſſ. Clement and

Deſormes

Diſcovery of the ſulphur-alcohol of Lampadius, which he could not repeat: It was afterwards once only verified by Clement and Deſormes.

The inventor re-diſcovered the ſubſtance, and it can be produced at pleaſure.

Deſormes obtained the ſame reſult, and attempted to produce the ſubſtance a ſecond time. They performed a vaſt number of experiments; but their ſucceſs bore no proportion to their diligence and zeal. They publiſhed an account of their proceſs and its conſequences, but gained little credit, as no perſon was fortunate enough to produce the ſame ſubſtance. Many diſbelieved the experiments altogether, and denied the exiſtence of ſuch a combination; whilſt others, leſs inclined to doubt, attributed its formation to fortuitous circumſtances which might never again occur together. In February, 1804, Profeſſor Lampadius, in diſtilling ſome pyritized wood, though with a different intent, obtained the ſame ſubſtance. As he had it now in his power to obſerve the phenomena that attended its formation, he diſcovered, and has communicated to the world, a method of producing it, which never fails. Since his late paper upon the ſubject, as the neceſſary precautions can be followed by every chemiſt, Meſs. Clement and Deſormes have obtained that credit to which their experiments had, in truth, always been entitled; and the formation, of what Profeſſor Lampadius terms his ſulphur-alcohol is no longer a reſult of chance, or accounted for by being ſuppoſed one of thoſe ſubterfuges to which human pride reſorts, in order to ſpare itſelf the confeſſion of human weakneſs.

New facts are of value the more extenſive their relations, &c.

The obſervation of any new fact becomes a matter of general concern, and truly worthy of philoſophic contemplation, then only, when its influence is likely to be extended beyond the ſingle inſtance to which it owes its diſcovery. Whether water were a ſimple body or a compound could have been of little importance as an inſulated fact; but, connected with the vaſt chain of reaſoning it gave riſe to, it opened a new field for genius to explore. If in the preſent caſe our reſearches were to be confined merely to aſcertaining whether palladium were a ſimple metal or a compound, all the advantages likely to ariſe from the facts obſerved during the inquiry would be loſt; and an object of the moſt comprehenſive intereſt would thus ſink into a controverſy concerning the exiſtence of one more of thoſe ſubſtances, which we have dignified with the name of elements. It was in this point of view that Meſs. Richter and Ritter conſidered the ſubject as far as they went, and a few facts are ſtated in my firſt Paper in ſupport of the opinion, that palladium is but a particular inſtance of a general truth.

Concluſion: that compound metals may be taken for ſimple, and the detection be extremely difficult.

By taking the reaſoning on this ſubject then, in its wideſt extent, we ſhall be led, I think, to the following concluſion: That metals may exerciſe an action upon each other, even in their metallic ſtate, capable of ſo altering ſome of their principal properties as to render the preſence of one or more of them not to be detected by the uſual methods. In this is contained the poſſibility of a compound metal appearing to be ſimple; but to prove this muſt be a work of great time and perſeverance; and can only be done by conſidering ſingly and ſucceſſively the different caſes which it contains, and by inſtituting experiments upon each. When an affinity which unites two bodies, and ſo blends their different properties as to make them apparently one, has taken its full effect, it will not be eaſy to ſeparate them; and this will be more particularly the caſe when neither of thoſe ſubſtances is remarkable for exerciſing a powerful action upon others. The method of analyſis therefore does not promiſe much ſucceſs; and the labour of ſyntheſis is ſufficient to deter any individual from the undertaking.

Platina and mercury do act on each other, and diſguiſe the properties of each.

It is my intention now to exhibit one example of my poſition, and to prove that platina and mercury act upon each other, in ſuch a manner as to diſguiſe the properties of both. I ſhall therefore wave for the preſent all conſideration of palladium, which is in fact but a ſubordinate inſtance of the caſe before us.

Example: Platina is precipitated in the metallic ſtate by green ſulph. of iron, if mercury or ſilver be preſent; but not elſe.

When a ſolution of green ſulphate of iron is poured into a ſolution of platina, no precipitate, nor any other ſenſible change enſues. This I had already obſerved, and it has ſince been confirmed by all who have written upon the ſubject. But, if a ſolution of ſilver or of mercury be added, a copious precipitate takes place. This precipitate contains metallic platina and metallic ſilver or mercury; ſome muriate of one or other of the latter metals is alſo preſent, as it is not eaſy to free the ſolution of platina from all ſuperfluous muriatic acid. But theſe ſalts are of no importance in the experiment; and can be ſeparated by ſuch methods as a knowledge of their chemical properties will eaſily ſuggeſt. The proper object of conſideration is the reduction of the platina to the metallic ſtate, which does not happen when it is alone. I have tried to produce the ſame effect with other metals and platina, but I have not obſerved any thing ſimilar. It is therefore fair to conclude, that when a ſolution of platina is precipitated in a metallic ſtate by a ſolution of green ſulphate of iron, either ſilver or mercury is preſent.

Nitrate of ſilver added to mur. platina throws down a triple muriate of pl. and ſilver.

The precipitation of a mixed ſolution of platina and ſilver requires no further caution than to free the ſalt of platina as much as poſſible from muriatic acid; for as I obſerved in my former Paper, the effect of nitrate of ſilver poured into muriate of platina, is to produce a precipitate, not of muriate of ſilver, but of a triple muriate of platina and ſilver. It was by this experiment that I then proved the affinity of theſe two metals; for when ſilver is not preſent, muriate of platina is among the moſt ſoluble ſalts. The beſt method of preſenting the three ſolutions of platina, ſilver, and green ſulphate of iron to each other, is firſt to pour the filtered ſolution of the laſt into the ſolution of platina, and then, after mixing them thoroughly together, to add the ſolution of ſilver by degrees, and to ſtir them conſtantly. In this, as in all ſimilar operations, the preſence of all acids, ſalts, &c. excepting thoſe neceſſary for the operation, ſhould be avoided; and if proper proportions have been uſed, and all circumſtances attended to, the precipitation of theſe two metals will be very complete.

Preciſe method of precip. pl. and ſilv. in the metallic ſtate by ſulph. iron.

The ſame, but with ſolution of mercury.

But the precipitation by a ſolution of mercury requires to be further conſidered, as the ſtate of oxidizement of this metal, as well as the acid in which it is diſſolved, produces a conſiderable modification in the reſult. In the firſt place the oxide, at the minimum of oxidizement, diſſolved in muriatic acid, is unfit for the experiment; and even the red oxide diſſolved in the ſame acid, or corroſive ſublimate, is not the moſt advantageous. When a warm ſolution of the latter is poured into a mixed ſolution of platina and green ſulphate of iron alſo warm, as in the caſe of ſilver, theſe ſubſtances are brought into contact under the moſt favourable circumſtances. Yet even thus the precipitation is ſlowly and imperfectly formed, often not till ſeveral hours have elapſed; and ſometimes a very great deficiency of weight is obſerved, between the quantities uſed and thoſe recovered directly by this method. If a ſolution of nitrate of mercury be uſed, the effect is produced more rapidly, and the precipitate is more abundant. The precipitation of muriate of platina by nitrate of ſilver, and the combination which enſues from it, ſuggeſted to me an experiment which I muſt ſtate at length, as from the reſult of it conſequences are deduced which modify ſome of the experiments of my former Paper.

Mercury diſſolved at the minimum of oxidation will fall in a triple ſalt with platina without any addition of ſulph. iron.

It occurred to me that a method of uniting platina and mercury without the intervention of any other metal, or of any ſubſtance but the ſolvents of theſe metals might be accompliſhed as in the caſe of ſilver and platina. I therefore poured a ſolution of nitrate of mercury, which ſolution being at the minimum of oxidizement, conſequently formed an inſoluble muriate with muriatic acid, into a ſolution of muriate of platina. The reſult was a triple ſalt of platina and mercury, which when the mercury was completely and totally at the minimum of oxidizement was nearly inſoluble. To procure it in this ſtate it is ſufficient to put more metallic mercury into dilute nitric acid than the nitric acid can diſſolve, and to boil them together. This triple ſalt of platina and mercury ſhall be preſently examined. From this it is evident than to produce the union of platina and mercury, the latter being at its minimum of oxidizement in nitric acid the addition of green ſulphate of iron is ſuperfluous.

Not ſo if the mercury be at the maximum of oxidizement.

But if mercury be raiſed to its maximum of oxidizement in nitric acid the caſe is different, for no precipitation occurs till the green ſulphate of iron is added. The moſt advantageous method for precipitating platina and mercury by green ſulphate of iron is, I believe, the following. Mix a ſolution of platina with a ſolution of green ſulphate of iron, both warm, and add to them a ſolution of nitrate of mercury at the maximum of oxidizement alſo warm. It is neceſſary to avoid exceſs of acid, ſalt, &c. in this as in all ſuch caſes. With due care the precipitation of both metals will then be complete.

Whether mercury be precipitable, ſingly, by green ſulphate of iron? &c.

By comparing the experiments made with mercury and platina with thoſe made with ſilver and platina, a ſtriking reſemblance will be found. This induced me to purſue the analogy, and to examine whether, independently of the action of platina, mercury had not the ſame property of being precipitated by green ſulphate of iron as ſilver. Nitrate of ſilver is precipitated by green ſulphate of iron, but muriate of ſilver is not ſenſibly acted upon by the ſame reagent. The inſolubility of muriate of ſilver might be alledged as the cauſe of this, if I had not tried the experiment by pouring nitrate of ſilver into green muriate of iron, in which caſe all the ſubſtances were preſented to each other in ſolution. The reſult was not reduction, but muriate of ſilver and nitrate of iron. This fact reſts upon a much more extenſive baſis than mere mechanical circumſtances; and, if purſued with the intention it deſerves, it would lead us into the wide expanſe of complicated affinities and their relations. From reaſoning alone we ſhould be diſpoſed to think that an acid, ſo eaſily decompoſed as the nitric, would be ſufficient to prevent the reduction of a metal which it can diſſolve. But on the one hand it can ſpend its oxygen upon a part of the oxide of the green ſulphate of iron, while on the other its affinity for oxide of ſilver is not powerful enough to retain it, when there is another part of the oxide of iron preſent to deprive it of oxygen. But the affinity of muriatic acid for oxide of ſilver, one of the ſtrongeſt at preſent known, is ſufficient to counterbalance all the other forces. There are many other inſtances of the ſame kind.

Mercury in nitric acid is precipitated (metallic) by gr. ſulph. of iron.

If then a ſolution of green ſulphate of iron be brought into contact with either ſoluble or inſoluble muriate of mercury, no reduction takes place; but if mercury, whether at the maximum or the minimum of oxidizement, be diſſolved in nitric acid, and green ſulphate of iron be added, the mercury is precipitated in the metallic ſtate.

Theſe remarkable facts modify the reſults formerly ſtated.

Theſe experiments are much ſtronger examples than the former of the effects produced by complicated affinities. They are of importance not only as objects of general conſideration but in their application to the preſent ſubject. They moſt materially modify and are indiſpenſable to the accuracy of the reſults I formerly ſtated; but I was not aware of them at the time I firſt engaged in the inveſtigation of this ſubject. I can alſo now explain a very material difference between ſome proportions obſerved by M. Richter and myſelf in an experiment which that chemiſt had made as a repetition of one of mine.

Precipitation of gold and mercury by gr. ſulphate of iron: repeated by Richter.

I had poured a ſolution of green ſulphate of iron into a ſolution of 100 parts of gold and 1200 of mercury, and had obtained a precipitate conſiſting of 100 of gold and 774 of mercury. M. Richter repeated, as he terms it, this experiment;

ment; that is, he uſed 100 of gold and 300 of mercury, and obtained a precipitate weighing 102. He is ſurpriſed at the difference of weight between our reſults, which might be owing to his *method* of *repeating* the experiment; but the real cauſe of this difference lies, as I ſuppoſe, in my having accidentally uſed nitrate inſtead of muriate of mercury. I had never obſerved that with mercury and ſilver this operation had failed, and it muſt have been, becauſe, on account of the known effect of muriatic ſalts upon thoſe of ſilver, I had naturally avoided uſing a muriate of mercury,

The ſtate of oxidation of the nitrate of mercury uſed with the ſolution of gold is of conſequence: If the minimum of oxidation prevails, calomel and metallic gold fall down;

But the ſtate of the nitrate of mercury which is uſed with a ſolution of gold is not indifferent. As green ſulphate of iron reduces mercury when diſſolved in nitric acid, as well as gold, it is neceſſary to mix the ſolutions of thoſe metals before the green ſulphate of iron is added, in order that both may be acted upon together. If the nitrate be at the minimum of oxidizement, a precipitate is immediately formed upon mixing the ſolutions of gold and mercury. Calomel is produced by the muriatic acid of the ſolution of gold and the oxide of mercury; whilſt the gold is reduced to the metallic ſtate by a portion of the oxide of mercury becoming more oxidized, and forming the ſoluble muriate. The precipitate conſiſts of calomel, of metallic gold, and of a very ſmall portion of mercury which I believe to be in the ſame ſtate; my reaſon for thinking ſo, is, that I have often obſerved, that a glaſs veſſel in which I had ſublimed ſome of it, was lined with a thin gray metallic coat. If, on the contrary, a nitrate of mercury be highly oxidized, no precipitate nor reduction of gold takes place until the green ſulphate of iron is added. But at any rate the precipitation of gold and mercury, or of ſilver and mercury by green ſulphate of iron, cannot be adduced as an argument to ſupport the affinity of theſe metals, ſince the effect is the ſame, whether they are ſeparate or united.

but if the maximum, then nothing falls till the green ſulphate of iron is added.

Theſe preliminary conſiderations were neceſſary as well for the rectification of my former experiments as for the purſuit of my preſent object; and now to return to platina.

Experiments with platina.
1. Much of the highly oxid. ſol. of mercury poured into a mixed ſolution of plat.

Exper. 1. If a ſolution of highly oxidized nitrate of mercury be poured into a mixed ſolution of platina and green ſulphate of iron, the firſt action which takes place paſſes between the muriatic acid of the ſolution of platina and the oxide of mercury, by which a muriate of mercury is formed, but retained in ſolution. This effect makes it advantageous to uſe a greater quantity of the ſolution of mercury than is merely capable of drawing down the given quantity of platina along with itſelf in the form of a metallic precipitate. When this precipitate is waſhed and dried, it will be found to weigh much more than the original quantity of platina; and the augmentation of weight has no limit but thoſe of the mercury and the green ſulphate of iron employed. But even after nitric acid has been boiled for a long time and in great quantities upon this precipitate, until it no longer diſſolves any part of it, there ſtill remains more undiſſolved matter than the original weight of the platina uſed in the experiment. By expoſure to heat little more is left in general than the original platina; and ſometimes even a diminution may be obſerved; for as the experiment is not attended with uniform ſucceſs, it does not always happen that the whole of the platina is precipitated, but a portion of it will ſometimes reſiſt the action of the green ſulphate of iron, even when ſufficient mercury has been uſed. Before the precipitate has been expoſed to heat it is diſſolved more eaſily than platina by nitro-muriatic acid; and the ſolution when nearly in a neutral ſtate gives a copious metallic precipitate, (yet not equal to the quantity employed,) when boiled with a ſolution of green ſulphate of iron.

and gr. ſulph. of iron, throws down platina and mercury united.

Part of the latter is defended from nitric acid.

Heat drives off the mercury.

The compound precipitate is more ſoluble than plat. in n. m. acid, and gives much metal. precip. by add. of ſ. of gr. ſ. of iron.

Exper. 2. When a mixed ſolution of platina and mercury is precipitated by metallic iron, a quantity equal to the ſum of the former metals is generally obtained. After nitric acid has been boiled for a long time upon the precipitate ſo formed, the original weight of platina, together with a conſiderable increaſe, remains behind, nor can nitric acid ſenſibly diminiſh it. It yields more eaſily than platina to the action of nitro-muriatic acid, and its ſolution in that acid, when neutralized, gives a precipitate, as in the former experiment, by green ſulphate of iron. If this precipitate be expoſed to a ſtrong heat after it has been boiled with nitric acid, it loſes a great part of its weight, and the platina alone will generally be found to remain.

Exp. 2. Metallic iron throws down the pl. and merc. from a mixed ſolution. Nitric acid does not deprive this metal of all its mercury. It is more ſoluble in n. m. acid; and precip. by gr. ſ. of iron. Heat uſually expels the mercury from this.

Exper. 3. When a quantity of ammoniacal muriate of platina is treated according to the method of Count Muſſin Puſhkin to form an amalgam, and, after being rubbed for a conſiderable time with mercury, is expoſed in a crucible to a heat gradually increaſed till it becomes violent, a metallic powder remains in the

Exp. 3. Amalgam (from ammon. mur. of plat.) ſtrongly heated, leaves a powder, acted on by n. m. acid, and copiouſly precip. by gr. ſ. of iron.

the crucible. This powder is acted upon by nitro-muriatic acid, and when the ſolution is neutralized, a copious precipitate is formed upon the addition of green ſulphate of iron. This effect takes place even after the metal has been fuſed in the manner deſcribed in the former part of this paper.

Exp. 4. Sulphur added in the laſt Exp. cauſes a greater precip.

Exper. 4. If ſulphur be added to the ingredients recommended by Count Muſſin Puſhkin, and the whole treated as in the laſt experiment, the quantity of precipitate cauſed by green ſulphate of iron in the nitro-muriatic ſolution of the button which reſults from the operation, is generally more conſiderable.

Exp. 5. Sulphur rubbed with am. m. of plat. eaſily melts. Mercury being added, ſtrong fuſion gives a button, ſol. in n. m. acid and precipitable by gr. ſ. of iron.

Exper. 5. If ſulphur be rubbed for ſome time with ammoniacal muriate of platina, and the mixture be introduced into a ſmall Florence flaſk, it can be melted on a ſand-bath. If mercury be then thrown into it, and the whole be well ſtirred together and heated, it may afterwards be expoſed to a very ſtrong fire and melted into a button. If this be diſſolved in nitro-muriatic acid, it will give a precipitate, as in the former caſes, by green ſulphate of iron.

Exp. 6. The precip. from ſol. of plat. and merc. by ſulph. hydrogen being fuſed affords a ſol. precip. by gr. ſ. of iron.

Exper. 6. If a current of ſulphuretted hydrogen gas be ſent through a mixed ſolution of platina and mercury, and the precipitate which enſues be collected, the metal may be reduced by heat; and with the addition of borax, it may be melted into a button which will not contain any ſulphur. Green ſulphate of iron cauſes a precipitate in the ſolution of this metal alſo.

Exp. 7. So likewiſe the precip. by phoſphate of ammon.

Exper. 7. If to a mixed ſolution of platina and mercury, phoſphate of ammonia be added, a precipitate takes place. If this be collected and reduced, it will be acted upon by green ſulphate of iron poured into its ſolution, in the ſame manner as the metallic buttons in the preceding examples.

Exp. 8. Nitr. of merc. at min. of oxidiz. precipitates mur. of pl. The metallic compound diſſolved in n. m. acid is precipitable by gr. ſ. of iron.

Exper. 8. I have already mentioned that when a ſolution of nitrate of mercury, at the minimum of oxidizement, is poured into a ſolution of muriate of platina, a mercurial muriate of platina is precipitated. The ſupernatant liquor may be decanted and the reſiduum waſhed; if this be reduced and afterwards diſſolved in nitro-muriatic acid, it will yield a precipitate with green ſulphate of iron. This method appears to me to be the neateſt for combining platina and mercury, as the action which takes place is independent of every ſubſtance except the metals themſelves.

Exper. 9. One of the moſt delicate teſts that I have obſerved in chemiſtry is recent muriate of tin, which detects the preſence of the ſmalleſt portion of mercury. When a ſingle drop of a ſaturate ſolution of neutralized nitrate or muriate of mercury is put into 500 grains of water, and a few drops of a ſaturate ſolution of recent muriate of tin are added, the liquor becomes a little turbid, and of a ſmoke-gray colour. If theſe 500 grains of liquid be diluted with ten times their weight of water, the effect is of courſe diminiſhed, but ſtill it is perceptible. I had on a former occaſion obſerved the action of recent muriate of tin upon a ſolution of platina. If a ſolution of recent muriate of tin be poured into a mixed ſolution of platina and mercury, not too concentrated, it can hardly be diſtinguiſhed from a ſimple ſolution of platina. But if too much mercury be preſent, the exceſs is acted upon as mercury; and the liquor aſſumes a darker colour than with platina alone.

Exp. 9. Recent muriate of tin is the moſt delicate teſt of mercury. It does not indicate the mercury in a mixed ſolution of that metal and platina.

From all theſe experiments it is evident that mercury can act upon platina, and confer upon it the property of being precipitated in a metallic ſtate by green ſulphate of iron. By *Experiments* 1 and 2, it is proved, 1ſt, That platina can protect a conſiderable quantity of mercury from the action of nitric acid; and 2dly, That mercury can increaſe the action of nitro-muriatic acid upon platina. From *Experiments* 3, 4, 5, 6, 7, 8, it appears that mercury can combine with platina in ſuch a manner as not to be ſeparated by the degree of heat neceſſary to fuſe the compound, ſince after the fuſion it retains that property, which is eſſentially characteriſtic of the preſence of mercury in a ſolution of platina. The 8th *Experiment* proves that the action of mercury upon platina is not confined to the metallic ſtate; but that theſe metals can combine and form an inſoluble triple ſalt with an acid which produces a very ſoluble compound with platina alone. The 9th *Experiment* ſhows that platina can retain in ſolution a certain quantity of mercury, and prevent its reduction by a ſubſtance which acts moſt powerfully to that effect, when platina is not preſent. That part of the general poſition therefore which is the object of this paper is proved, if theſe experiments, upon being repeated by other chemiſts, ſhall be found to be accurate.

Hence, 1, 2, Platina protects mercury from nitric acid; and mercury renders platina more ſubject to nitro-mur. acid: 3—8. Platina retains mercury in the ſtrong heat of fuſion. 8. Mercury and platina act on each other in ſaline compoſition. 9. Platina defends mercury in ſolution from being reduced by m. of tin.

One or two of the above experiments ſeem to be in contradiction to ſome that I have ſtated in my paper upon palladium; for

Remark on palladium.

for in the preſent examples platina protects mercury againſt the action of nitric acid; whereas in palladium the mercury is not only acted upon itſelf, but it conduces to the ſolution of platina in the ſame acid. I am well aware of this objection; but confining myſelf to my preſent object, I ſhall wave all further diſcuſſion of it till another opportunity. In the mean time, however, it may be laid down as an axiom in chemiſtry, that the ſtrongeſt affinities are thoſe, which produce in any ſubſtance the greateſt deviation from its uſual properties.

The compounds of platina and mercury are ſubject to great variations, &c.

When a button of the alloy of platina and mercury as prepared by any of the above methods, is diſſolved in nitromuriatic acid, and afterwards precipitated by green ſulphate of iron, the entire quantity of the alloy uſed is ſeldom obtained. A conſiderable portion of platina reſiſts the action of green ſulphate of iron, and remains in ſolution. This may be looked upon as the exceſs of platina, and can be recovered by a plate of iron. Hence it appears that leſs mercury is fixed, than can determine the precipitation of the entire quantity of platina; yet in this ſtate it can draw down a greater quantity of the latter, than when it is merely poured into a mixed ſolution of platina, not before ſo treated. Indeed the whole of theſe experiments tend, not only to ſhow that theſe two metals exerciſe a very powerful action upon each other, but that they are capable of great variation in the ſtate of their combination; and alſo that ſubſtances poſſeſſing different properties have reſulted from my attempts to combine platina with mercury.

Inveſtigation of the quantity of mercury thus fixed. It is ſuppoſed or inferred to be about 17 mercury to 83 platina, with the ſpecific gravity 16.

This obſervation furniſhed me with a method of aſcertaining, or at leaſt of approaching to the knowledge of, the quantity of mercury thus fixed by platina, and in combination with it. The experiment, however, having been ſeldom attended with full ſucceſs, I mention the reſult with the entire conſciouſneſs of the uncertainty to which it is ſubject. I obſerved the increaſe of weight, which the original quantity of platina had acquired in ſome caſes after it had been treated with mercury, and fuſed into a button. I counted that augmentation as the quantity of mercury fixed. I then determined how much was precipitated by green ſulphate of iron from a ſolution of this alloy, and ſuppoſed it to contain the whole quantity of mercury found as above. But, even if attended with complete ſucceſs, there is a chemical reaſon which muſt make us refuſe our aſſent to this eſtimate. It is poſſible, and not unlikely, that a portion of mercury may be retained in ſolution by the platina, as well as that a portion of the platina may be precipitated by means of the mercury. The mean reſult, however, was that the precipitate by green ſulphate of iron conſiſted of about 17 of mercury, and 83 of platina, when the ſpecific gravity was about 16.

The author continues to think that palladium is a compound,

With regard to palladium, leſt it ſhould be ſuppoſed that either my own obſervations, or thoſe of others have given me cauſe to alter my opinion; I will add, that I have as yet ſeen no arguments of ſufficient weight to convince me, in oppoſition to experiment, that palladium is a ſimple ſubſtance. Repeated failure in the attempt to form it I am too well accuſtomed to, not to believe that it may happen in well conducted operations; but four ſucceſsful trials, which were not performed in ſecret, are in my mind a ſufficient anſwer to that objection. By determining the preſent queſtion we may overcome the prepoſſeſſion conceived by many againſt the poſſibility of rendering mercury as fixed, at an elevated temperature, as other metals: we may be led to ſee no greater miracle in this compound than in a metallic oxide, or in water, and be compelled to take a middle path between the viſions of alchemy on the one hand, and the equally unphiloſophical prejudices on the other, which they are likely to create. In the courſe of experiments juſt now related, I have ſeen nothing but what tends to confirm my former reſults, yet the only means which I can, after all, preſcribe for ſucceeding, is perſeverance.

ſince four ſucceſsful experiments were made of forming it.

Platina, purified in Fourcroy and Vauquelin's method, is equally combinable with mercury.

To aſcertain whether the opinion of Meſſ. Fourcroy and Vauquelin, that the new metal was the principal ingredient in palladium had any juſt foundation, I obſerved the methods they have recommended for obtaining pure platina; but I did not perceive any difference in the facility with which either kind of platina combined with mercury.

Theſe chemiſts and M. Richter have promiſed to purſue the ſubject.

I might have added ſome more experiments to corroborate the evidence I have adduced to prove my aſſertion of the fixation of mercury by platina; but Meſſ. Vauquelin and Fourcroy have promiſed the Inſtitute of France a continuation of their reſearches, and M. Richter concludes his paper with ſaying that he will return to the ſubject. From the labours of ſuch perſons ſome great and important fact muſt iſſue, and I hope that the preſent ſubject will not be excluded from their conſideration. The facts contained in this paper cannot be

ſubmitted

ſubmitted to too ſevere a ſcrutiny; and no judge can be more rigid or more competent than the very perſon who was the firſt to doubt my former experiments. But it is neceſſary to be obſerved by whoever ſhall think them worth the trouble of verifying, that even theſe experiments are liable to fail unleſs proper precautions are uſed: that I have never operated upon leſs than one hundred grains; and that the reſults, which I have ſtated, however ſimple they may appear, have been the conſtant labour of ſome weeks.

POSTSCRIPT.

Dr. Wollaſton's palladium in crude platina proves nothing; and it may have been the product of the amalgamating proceſs.

Since this paper was written Dr. Wollaſton has publiſhed ſome experiments upon platina. He has found that palladium is contained in very ſmall quantities in crude platina. This fact was mentioned to me more than a year ago by Dr. Wollaſton. I have not yet ſeen a copy of his paper; but I ſhall merely obſerve here that, whatever be the quantity of palladium found in a natural ſtate, no concluſion can be drawn as to its being ſimple or compound. Nothing is more probable than that nature may have formed this alloy, and formed it much better than we can do. At all events, the amalgamation to which platina is ſubmitted before it reaches Europe, is ſufficient to account for a ſmall portion of palladium.

VI.

Method of obviating the Neceſſity of Lifting Ships. By Mr. ROBERT SEPPINGS, *of Chatham Yard*[*].

Great ſaving and advantage of ſuſpending inſtead of lifting ſhips.

THE method here to be deſcribed of ſuſpending, inſtead of lifting, ſhips, for the purpoſe of clearing them from their blocks; affords a very great ſaving to the public; and abridges two-thirds of the time formerly uſed in this operation. From the ſaving of time another very important advantage is derived, namely, that of enabling large ſhips to be docked, ſuſpended, and undocked, the ſame ſpring tides. Without enumerating the inconveniencies ariſing, and, perhaps, injuries, which ſhips are liable to ſuſtain, from the former practice of lifting them, and which are removed by the preſent plan; that which relates to manual labour deſerves particular attention; twenty men being ſufficient to ſuſpend a firſt rate, whereas it would require upwards of 500 to lift her. The ſituation which Mr. Seppings held in Plymouth-yard, attached to him, in a great degree, the ſhoring and lifting of ſhips, as well as the other practical part of the profeſſion of a ſhipwright. Here he had an opportunity of obſerving, and indeed it was a ſubject of general regret, how much time, expenſe, and labour, were required in lifting a ſhip, particularly ſhips of the line. This induced him to conſider whether ſome contrivance could not be adopted to obviate theſe evils. And it occurred to him, that if he could ſo conſtruct the blocks on which the ſhip reſts, that the weight of the ſhip might be applied to aſſiſt in the operation, he ſhould accompliſh this very deſireable end. In September 1800, the ſhoring and lifting the San Joſef, a large Spaniſh firſt-rate, then in dock at Plymouth, was committed to his directions; to perform which, the aſſiſtance of the principal part of the artificers of the yard was requiſite. In conducting this buſineſs, the plan, which will be hereafter deſcribed, occured to his mind; and from that time, he, by various experiments, proved his theory to be correct: the blocks, conſtructed by him, upon which the ſhip reſts, being ſo contrived, that the facility in removing them, is proportionate to the quantity of preſſure; and this circumſtance is always abſolutely under command, by increaſing or diminiſhing the angle of three wedges, which conſtitute one of the blocks; two of which are horizontal, and one vertical. By enlarging the angle of the horizontal wedges, the vertical wedge becomes of conſequence more acute; and its power may be ſo increaſed, that it ſhall have a great tendency to diſplace the horizontal wedges, as was proved by a model, which accompanied the ſtatement to the Society; where the power of the ſcrew is uſed as a ſubſtitute for the preſſure of the ſhip.

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[*] From the Tranſactions of the Society of Arts, who voted him the gold medal, 1804.

Twenty men ſupply the place of 500.

Hiſtory.

The contrivance is a block, compounded of two horizontal wedges, acting under an obtuſe vertical wedge, which by the ſhip's weight, can, if needful, diſplace the others.

Mr. Seppings cauſed three blocks to be made of hard wood agreeable to his invention, and the wedges of various angles. The horizontal wedges of the firſt block were nine degrees; of the ſecond, ſeven; and of the third, five; of courſe, the angle of the vertical wedge of the firſt block was 162 degrees; of the ſecond 166; and of the third, 170 Theſe blocks, *or*

Experiments.

Angles of the wedges.

wedges, were well executed, and rubbed over with ſoft ſoap for the purpoſe of experiment. They were then placed in a dock, in his Majeſty's yard at Plymouth, in which a ſloop of war was to be docked; on examining them after the veſſel was in, and the water gone, they were all found to have kept their ſituations, as placed before the ſhip reſted upon them. Shores in their wake were then erected to ſuſtain the ſhip, prior to the ſaid blocks being taken from under the keel. The proceſs of clearing them was, by applying the power of battering-rams to the ſides of the outer ends of the horizontal wedges; alternate blows being given fore and aft; by which means they immediately receded, and the vertical wedges were diſengaged. It was obſerved, even in this ſmall ſhip, that the block which was formed of horizontal wedges of nine degrees, came away much eaſier than thoſe of ſeven, and the one of ſeven, than that of five. In removing the aforeſaid blocks by the power of the battering-rams, which were ſuſpended in the hands of the men employed, by their holding ropes paſſed through holes for that purpoſe, it was remarked by Mr. Seppings, that the operation was very laborious to the people; they having to ſupport the weight of the battering-rams, as well as to ſet them in motion. He then conceived an idea of affixing wheels near the extremity of that part of the rams, which ſtrikes the wedges. This was done before the blocks were again placed; and it has ſince been found fully to anſwer the purpoſe intended, particularly in returning the horizontal wedges to their original ſituations, when the work is performed for which they were diſplaced; the wheels alſo giving a great increaſe power to the rams, and decreaſe of labour to the artificers; beſides which, the blows are given with much more exactneſs. The ſame blocks were again laid in another dock, in which a two-decked ſhip of the line was docked. On examination they were found to be very ſeverely preſſed, but were removed with great eaſe. They were again placed in another dock, in which a three-decked ſhip of the line was docked. This ſhip having in her foremaſt and bowſprit, the blocks were put quite forward, that being the part which preſſes them with the greateſt force. As ſoon as the water was out of the dock, it was obſerved, that the horizontal wedges of nine and ſeven degrees had receded ſome feet from their original ſituations. This afforded Mr. Seppings a ſatisfactory proof, which experience has ſince demonſtrated, (though

A ſloop reſted upon them, and ſhored up.

The horizontal wedges were then driven out,

by battering-rams,

ſupported on wheels.

Other experiments.

It was found that the weight of the veſſel was capable of preſſing out the wedges;—

(though many perſons before would not admit of, and others could not underſtand, the principle) that the facility of removing the blocks or wedges, was proportionate to the quantity of preſſure upon them. The block of five degrees kept its place, but was immediately cleared, by applying the power of the battering-rams to the ſides of the outer ends of the horizontal wedges. The above experiments being communicated to the Navy Board, Mr. Seppings was directed to attend them, and explain the principle of his invention; which explanation, farther corroborated by the teſtimonials of his then ſuperior officers, was ſo ſatisfactory, that a dock was ordered to be fitted at Plymouth under his immediate directions. The horizontal wedges in this, and in the other docks, that were afterwards fitted by him, are of caſt iron, with an angle of about five degrees and a half, which, from repeated trials, are found equal to any preſſure, having in no inſtance receded, and, when required, were eaſily removed. The vertical wedge is of wood, lined with a plate of wrought iron, half an inch thick. On the bottom of the dock, in the wake of each block, is a plate of iron three quarters of an inch thick, ſo that iron at all times acts in contact with iron.

and therefore that if made of the proper angle they would be ſecure, and always capable of being driven out.

The beſt angle is about 5½ degrees for caſt iron wedges.

The placing the ſuſtaining ſhores, the form and ſizes of the wedges, and battering-rams, &c. alſo the proceſs of taking away, and again re-placing, the wedges of which the block is compoſed, are alſo exemplified by a model.

The dock being prepared at Plymouth, in Auguſt, 1801, the Canopus, a large French 80-gun ſhip, was taken in, and reſted upon the blocks; and the complete ſucceſs of the experiment was ſuch, that other docks were ordered to be fitted at Sheerneſs and Portſmouth dock-yards, under Mr. Seppings's directions. At the former place a frigate, and at the latter a three-decked ſhip, were ſuſpended in like manner. This happened in December, 1802, and January, 1803; and the reports were ſo favourable, as to cauſe directions to be given for the general adoption of theſe blocks in his Majeſty's yards. This invention being thought of national conſequence, with reſpect to ſhips, but particularly thoſe of the navy, government has been pleaſed to notice and reward Mr. Seppings for it.

A large 80-gun ſhip ſuſpended by this method.

The time required to diſengage each block, is from one to three minutes after the ſhores are placed: and a firſt-rate ſits on

Each block may be diſengaged in three minutes.

It is not required to ſuſpend the ſhip in all caſes. For the repairs may be done by ſucceſſive removal of blocks.

on about fifty blocks. Various are the cauſes for which a ſhip may be required to be cleared from her blocks, viz. to ſhift the main keel; to add additional falſe keel; to repair defects; to caulk the garboard ſeams, ſcarples of the keel, &c. Imperfections in the falſe keel, which are ſo very injurious to the cables, can in the largeſt ſhip be remedied in a few hours by this invention, without adding an additional ſhore, by taking away blocks forward, amid-ſhips, and abaft, at the ſame time; and when the keel is repaired in the wake of thoſe blocks, by returning them into their places, and then by taking out the next, and ſo on in ſucceſſion. The blocks can be replaced in their original ſituations, by the application of the wheel battering-rams to the wedges, the power of which is ſo very great, that the weight of the ſhip can be taken from the ſhores that were placed to ſuſtain her. There were one hundred and ſix ſhips of different claſſes, lifted at Plymouth dock-yard, from the 1ſt of January, 1798, to the 31ſt of December, 1800; and, had the operation of lifting taken leſs time, the number would have been very conſiderably increaſed; for the ſaving of a day is very frequently the cauſe of ſaving the ſpring tide, which makes the difference of a fortnight. The importance of this expedition, in time of war, cannot be ſufficiently eſtimated.

The lifting of ſhips was a frequent operation in the navy.

This invention is of value in other undertakings.

This invention may be applied with great advantage, whenever it is neceſſary to erect ſhores, to ſupport any great weights, as, for inſtance, to prop up a building during the repair of its foundation, &c. Captain Wells, of his Majeſty's ſhip Glory, of 98 guns, uſed wedges of Mr. Seppings's invention for a fid of a top-gallant maſt of that ſhip. In 1803, the top-gallant maſts of the Defence, of 74 guns, were fitted on this principle by Mr. Seppings: and, from repeated trials, ſince ſhe has been cruizing in the North Sea, the wedge fids have been found in every reſpect to anſwer.

Fid of a top gallant maſt,

applied by Capt. Wells.

But it is Mr. Seppings's wiſh that it ſhould be underſtood, that the idea of applying this invention to the fid of a top-gallant maſt originated with Capt. Wells, who well underſtood the principle, and had received from him a model of the invention.

Manœuvre of ſtriking the maſt, &c.

When it is required to ſtrike a top-gallant maſt, the top ropes are hove tight, and the pin which keeps the horizontal wedges in their place, is taken out, by one man going aloft for that purpoſe; the other horizontal wedge is worked in the fid, as ſhown in the drawing and model that accompany this ſtatement. The upper part of the fid hole is cut to form the vertical wedge. The advantage derived from fidding top-gallant maſts in this way is, that they can be ſtruck at the ſhorteſt notice, and without ſlacking the rigging, which is frequently the cauſe of ſpringing and carrying them away, particularly thoſe with long pole heads. The angle of the horizontal wedges for the fids of maſts ſhould be about twenty degrees.

It can now be done without ſlacking the rigging.

The above Account was accompanied with Certificates from Sir John Henſlow, Surveyor of the Navy; Mr. M. Didram, maſter-ſhipwright of Portſmouth-Yard; and Mr. John Carpenter, foreman of Sheerneſs Dock-yard, confirming Mr. Seppings's ſtatement.

Certificates.

Reference to the Engraving of Mr. Seppings's method of obviating the neceſſity of lifting Ships. Plate XI.

This plan and ſection of a ſeventy-four gun ſhip deſcribes the method of obviating the neceſſity of lifting ſhips, when there may be occaſion to put additional falſe keels to them, or to make good the imperfections of thoſe already on; alſo when it may be neceſſary to caulk the garboard ſeams, ſcarples the keel, &c. by which means a very conſiderable part of the expenſe will be ſaved, and much time gained. The blocks are cleared, and again returned by the following proceſs. A ſufficient number of ſhores are placed under the ſhip to ſuſtain her weight, and ſet taught, ſtationed as near the keel as the working of the battering-rams fore and aft will admit. Avoid placing any oppoſite the blocks, as they would in that caſe hinder the return of the wedges with the battering-rams. A blow muſt then be given forward on the outer end of the iron wedges with the battering-rams in a fore and aft direction, which will cauſe them to ſlide aft, as ſhown in the plan. The battering rams abaft then return the blow, and the wedges again come forward; by the repetition of this operation, the wedges will be with great eaſe cleared, and the angular block on the top will drop down. When the work is performed, the block muſt be replaced under the keel, and the wedges driven back by working the rams athwartſhips, as deſcribed in the ſection.

Deſcription and reference to the plate, &c.

Inſtructions.

N. B. In

N. B. In returning the iron wedges, to avoid ſtraining the angular blocks, it is propoſed to leave a few of them out forward and aft, and ſtop the ſhip up, by laying one iron wedge on the other, as ſhown at Fig. 1, Plate XI.

To facilitate the buſineſs, blocks may be cleared forward and aft at the ſame time, ſufficient to get in place one length of falſe keel. If the falſe keel ſhould want repairing, it may be done without any additional ſhores, by clearing one block at a time, and when the keel is repaired in the wake of that block, return the wedges, as above directed, and clear the next, &c.

Section and Plan, Plate XI. *Fig.* 2.

Parts of the ſection and plan.

A. Keelſon.
B. Ceiling.
C. Floor timber.
D. Dead or riſing wood.
E. Plank of the bottom.
F. Keel and falſe keel.
G. Angular blocks with a half-inch iron-plate bolted to them.
H. Caſt-iron wedges.
I. Iron plate of three-fourths of an inch thick on the bottom of the dock.
K. Battering-rams, with wheels, and ropes for the hands.
L. Caſt-iron wedges, having received a blow from forward.
M. Shores under the ſhip to ſuſtain her weight.

Fig. 3, repreſents part of a top-gallant maſt fitted with a wedge fid.
a. Top-gallant maſt.
b. Fid, with one horizontal wedge worked on it.
c. Moveable wedge, with the iron ſtrap and pin over it, to keep it in its ſituation.
d. Truſſel trees.

VII. *On*

VII.

On muſcular Motion. By ANTHONY CARLISLE, *Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, November* 8, 1801.

Introduction.

ANIMAL phyſiology has derived ſeveral illuſtrations and additions, from the inſtitution of this lecture on muſcular motion; and the details of anatomical knowledge have been conſiderably augmented by deſcriptions of muſcular parts before unknown.

Still, however, many of the phenomena of muſcles remain unexplained, nor is it to be expected that any ſudden inſulated diſcovery ſhall ſolve ſuch a variety of complicated appearances.

Muſcular motion.

Muſcular motion is the firſt ſenſible operation of animal life: the various combinations of it ſuſtain and carry on the multiplied functions of the largeſt animals: the temporary ceſſation of this motive faculty is the ſuſpenſion of the living powers, its total quieſcence is death.

By the continuance of patient, well directed reſearches, it is reaſonable to expect much important evidence on this ſubject and, from the improved ſtate of collateral branches of knowledge, together with the addition of new ſources, and methods of inveſtigation, it may not be unreaſonable to hope for an ultimate ſolution of theſe phenomena, no leſs complete, and conſiſtent, than that of any other deſideratum in phyſical ſcience.

The preſent attempt to forward ſuch deſigns is limited to circumſtances which are connected with muſcular motion, conſidered as cauſes, or rather as a ſeries of events, all of which contribute, more or leſs, as conveniencies, or eſſential requiſites, to the phenomena; the details of muſcular applications being diſtinct from the objects of this lecture.

Neither the changes in muſcles during action, nor their connections with the other parts of the ſyſtem have been explained.

No ſatisfactory explanation has yet been given of the ſtate or changes which obtain in muſcles during their contractions or relaxations, neither are their correſponding connections with the vaſcular, reſpiratory, and nervous ſyſtems, ſufficiently traced. Theſe ſubjects are therefore open for the preſent enquiry, and, although I may totally fail in this attempt to elucidate any one of the ſubjects propoſed, nevertheleſs I ſhall not eſteem my labour uſeleſs, or the time of the Royal Society altogether unprofitably conſumed, if I ſucceed in pointing out the way to the future attainment of knowledge ſo deeply intereſting to mankind.

The muſcle itſelf is fibrous.

The muſcular parts of animals are moſt frequently compoſed of many ſubſtances, in addition to thoſe which are purely muſcular. In this groſs ſtate, they conſtitute a flexible, compreſſible ſolid, whoſe texture is generally fibrous, the fibres being compacted into faſciculi, or bundles of various thickneſs. Theſe fibres are elaſtic during the contracted ſtate of muſcles after death, being capable of extenſion to more than one-fifth of their length, and of returning again to their former ſtate of contraction.

The enveloping membrane is elaſtic.

This elaſticity, however, appears to belong to the enveloping reticular or cellular membrane, and it may be ſafely aſſumed that the intrinſic matter of muſcle is not elaſtic.

The attraction of coheſion, in the parts of muſcle, is ſtrongeſt in the direction of the fibres, it being double that of the contrary, or tranſverſe direction.

Muſcles are irritable during life.

When muſcles are capable of reiterated contractions and relaxations, they are ſaid to be alive, or to poſſeſs irritability. This quality fits the organ for its functions. Irritability will be conſidered, throughout the preſent lecture, as a quality only.

They have leſs coheſion lengthways when dead.

When muſcles have ceaſed to be irritable, their coheſive attraction in the direction of their fibres is diminiſhed, but it remains unaltered in the tranſverſe direction.

Experiment in proof.

The hinder limbs of a frog attached to the pelvis being ſtripped of the ſkin, one of them was immerſed in water at 115° of Fahrenheit, during two minutes, when it ceaſed to be irritable. The thigh bones were broken in the middle, without injuring the muſcles, and a ſcale affixed to the ancle of each limb: a tape paſſed between the thighs was employed to ſuſpend the apparatus. Weights were gradually introduced into each ſcale, until, with five pounds avoirdupois, the dead thigh was ruptured acroſs the fleſhy bellies of its muſcles.

repeated.

The irritable thigh ſuſtained ſix pounds weight avoirdupois, and was ruptured in the ſame manner. This experiment was repeated on other frogs, where one limb had been killed by a watery ſolution of opium, and on another where eſſential oil of cherry laurel * was employed: in each experiment, the irritable limb ſuſtained a weight one-ſixth heavier than the dead limb.

The ſame doctrine confirmed by effects in the living ſubject.

It may be remarked, in confirmation of theſe experiments, that when muſcles act more powerfully, or more rapidly, than is equal to the ſtrength of the ſuſtaining parts, they do not uſually rupture their fleſhy fibres, but break their tendons, or even an intervening bone, as in the inſtances of ruptured tendo achillis, and fractured patella. Inſtances have however occurred, wherein the fleſhy bellies of muſcles have been lacerated by ſpaſmodic actions; as in tetanus the recti abdominis have been torn aſunder, and the gaſtrocnemii in cramps; but in thoſe examples it ſeems that either the antagoniſts produce the effect, or the over-excited parts tear the leſs excited in the ſame muſcle. From whence it may be inferred, that the attraction of coheſion in the matter of muſcle is conſiderably greater during the act of contracting, than during the paſſive ſtate of tone, or irritable quieſcence, a fact which has been always aſſumed by anatomiſts from the determinate forces which muſcles exert.

Differences obſervable in the colour, texture, &c. of muſcular parts.

The muſcular parts of different claſſes of animals vary in colour and texture, and not unfrequently thoſe variations occur in the ſame individual.

The muſcles of fiſhes and vermes are often colourleſs, thoſe of the mammalia and birds being always red; the amphibia, the accipenſer, and ſqualus genera, have frequently both red and colourleſs muſcles in the ſame animal.

Some birds, as the black game †, have the external pectoral muſcles of a deep red colour, whilſt the internal are pale.

In texture, the faſciculi vary in thickneſs, and the reticular membrane is in ſome parts coarſe, and in others delicate: the heart is always compacted together by a delicate reticular membrane, and the external glutæi by a coarſer ſpecies.

Origin of muſcle in the egg. Punctum ſaliens

An example of the origin of muſcle is preſented in the hiſtory of the incubated egg, but whether the rudiments of the punctum ſaliens be part of the cicatricula organiſed by the parent, or a ſtructure reſulting from the firſt proceſs of incubation, may be doubtful: the little evidence to be obtained on this point ſeems in favour of the former opinion; a regular

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Diſtilled oil from the leaves of the *Prunus Lauroceraſſus.*

† *Tetrao tetrix.* Lin.

confirmation of which would improve the knowledge of animal generation by ſhewing that it is gemmiferous. There are ſufficient analogies of this kind in nature, if reaſoning from analogies were proper for the preſent occaſion.

The punctum ſaliens, during its firſt actions, is not encompaſſed by any fibres diſcoverable with microſcopes, and the vaſcular ſyſtem is not then evolved, the blood flowing forwards and backwards, in the ſame veſſels. The commencement of life in animals of complex ſtructure is, from the preceding fact, like the ultimate organization of the ſimpler claſſes.

Muſcles of birds are formed out of the albumen, with a ſmall portion of vitellus and atmoſ. fluid,

It is obvious that the muſcles of birds are formed out of the albumen ovi, the vitellus, and the atmoſpheric air, acted upon by a certain temperature. The albumen of a bird's egg is wholly conſumed during incubation, and the vitellus little diminiſhed, proving that the albumen contains the principal elementary materials of the animal thus generated; and it follows that the muſcular parts, which conſtitute the greater proportion of ſuch animals when hatched, are made out of the albumen, a ſmall portion of the vitellus, and certain elements, or ſmall quantities of the whole compound of the atmoſphere.

and they do not differ from thoſe of the mammalia.

The muſcles of birds are not different, in any reſpect, from thoſe of quadrupeds of the claſs of mammalia.

The anatomical ſtructure of muſcular fibres is generally complex, as thoſe fibres are connected with membrane, blood-veſſels, nerves, and lymphæducts; which ſeem to be only appendages of convenience to the eſſential matter of muſcle.

Muſcular fibre is cylindric; membranous without, and pulpy within.

A muſcular fibre, duly prepared by waſhing away the adhering extraneous ſubſtances, and expoſed to view in a powerful microſcope, is undoubtedly a ſolid cylinder, the covering of which is reticular membrane, and the contained part a pulpy ſubſtance irregularly granulated, and of little coheſive power when dead.

The ultimate fibres.

A difficulty has often ſubſiſted among anatomiſts concerning the ultimate fibres of muſcles; and, becauſe of their tenuity, ſome perſons have conſidered them infinitely diviſible, a poſition which may be contradicted at any time, by an hour's labour at the microſcope.

Arteries.

The arteries arboreſce copiouſly upon the reticular coat of the muſcular fibre, and in warm-blooded animals theſe veſſels are of ſufficient capacity to admit the red particles of blood, but the intrinſic matter of muſcle, contained within the ultimate cylinder, has no red particles.

The arteries of muſcles anaſtomoſe with correſponding veins; but this courſe of a continuous canal cannot be ſuppoſed to act in a direct manner upon the matter of muſcle.

The capillary arteries terminating in the muſcular fibre muſt alone effect all the changes of increaſe in the bulk, or number, of fibres, in the repleniſhment of exhauſted materials, and in the repair of injuries; ſome of theſe neceſſities may be ſuppoſed to be continually operating, It is well known, that the circulation of the blood is not eſſential to muſcular action; ſo that the mode of diſtribution of the blood veſſels, and the differences in their ſize, or number, as applied to muſcles, can only be adaptations to ſome ſpecial convenience.

Vaſcularity is not infinite, but perceptibly limited.

Another prevalent opinion among anatomiſts, is the infinite extenſion of vaſcularity, which is contradicted in a direct manner by comparative reſearches. The ſeveral parts of a quadruped are ſenſibly more or leſs vaſcular, and of different contextures; and, admitting that the varied diameter of the blood veſſels diſpoſed in each ſpecies of ſubſtance, were to be conſtituted by the groſs ſenſible differences of their larger veſſels only, yet, if the ultimate veſſels were in all caſes equally numerous, then the ſole remaining cauſe of diſſimilarity would be in the compacting of the veſſels. The vaſa vaſorum of the larger trunks furniſh no reaſon, excepting that of a looſe analogy, for the ſuppoſition of vaſa vaſorum extended without limits. Moreover, the circulating fluids of all animals are compoſed of water, which gives them fluidity, and of animalized particles of defined configuration and bulk; it follows that the veſſels through which ſuch fluids are to paſs, muſt be of ſufficient capacity for the ſize of the particles, and that ſmaller veſſels could only filtrate water devoid of ſuch animal particles: a poſition repugnant to all the known facts of the circulation of blood, and the animal economy.

Capillary arteries,

The capillary arteries which terminate in the muſcular fibre, muſt be ſecretory veſſels for depoſiting the muſcular matter, the lymphæducts ſerving to remove the ſuperfluous extravaſated watery fluids, and the decayed ſubſtances which are unfit for uſe.

and lymphæducts.

The lymphæducts are not ſo numerous as the blood veſſels, and certainly do not extend to every muſcular fibre: they appear to receive their contained fluids from the interſticial ſpaces formed by the reticular or cellular membrane, and not from the projecting open ends of tubes, as is generally repreſented. This mode of receiving fluids out of a cellular ſtructure, and conveying them into cylindrical veſſels, is exemplified in the corpora cavernoſa, and corpus ſpongioſum penis, where arterial blood is poured into cellular or reticular cavities, and from thence it paſſes into common veins by the gradual coarctation of the cellular canals.

In the common green turtle, the lacteal veſſels univerſally ariſe from the looſe cellular membrane, ſituated between the internal ſpongy coat of the inteſtines and the muſcular coat. The cellular ſtructure may be filled from the lacteals, or the lacteals from the cellular cavities. When injecting the ſmaller branches of the lymphæducts retrograde in an œdematous human leg, I ſaw, very diſtinctly, three orifices of theſe veſſels terminating in the angles of the cells, into which the quickſilver trickled. The preparation is preſerved, and a drawing of the appearance made at the time. It was alſo proved, by many experiments, that neither the lymphæducts, nor the veins, have any valves in their minute branches.

Nerves of voluntary muſcles.

The nerves of voluntary muſcles ſeparate from the ſame bundles of fibrils with the nerves which are diſtributed in the ſkin, and other parts, for ſenſation; but a greater proportion of nerve is appropriated to the voluntary muſcles, than to any other ſubſtances, the organs of the ſenſes excepted.

Origin of the nerves of volition.

The nerves of volition all ariſe from the parts formed by the junction of the two great maſſes of the brain, called the Cerebrum and Cerebellum, and from the extenſion of that ſubſtance throughout the canal of the vertebræ. Another claſs of muſcles, which are not ſubject to the will, are ſupplied by peculiar nerves; they are much ſmaller, in proportion to the bulk of the parts on which they are diſtributed, than thoſe of the voluntary muſcles; they contain leſs of the white opaque medullary ſubſtance than the other nerves, and unite their fibrils, forming numerous anaſtomoſes with all the other nerves of the body, excepting thoſe appropriated to the organs of the ſenſes. There are enlargements at ſeveral of theſe junctions,

Muſcles for involuntary motion.

Ganglions.

Junctions, called Ganglions, and which are compoſed of a leſs proportion of the medullary ſubſtance, and their texture is firmer than that of ordinary nerves.

The extremities of nerves are definite in their termination.

The terminal extremities of nerves have been uſually conſidered of unlimited extenſion; by accurate diſſection however, and the aid of magnifying glaſſes, the extreme fibrils of nerves are eaſily traced as far as their ſenſible properties, and their continuity extends. The fibrils ceaſe to be ſubdivided whilſt perfectly viſible to the naked eye, in the voluntary muſcles of large animals, and the ſpaces they occupy upon ſuperficies where they ſeem to end, leave a remarkable exceſs of parts unoccupied by thoſe fibrils. The extreme fibrils of nerves loſe their opacity, the medullary ſubſtance appears ſoft and tranſparent, the enveloping membrane becomes pellucid, and the whole fibril is deſtitute of the tenacity neceſſary to preſerve its own diſtinctneſs; it ſeems to be diffuſed and mingled with the ſubſtances in which it ends. Thus the ultimate terminations of nerves for volition, and ordinary ſenſation, appear to be in the reticular membrane, the common covering of all the different ſubſtances in an animal body, and the connecting medium of all diſſimilar parts.

Diffuſion of nervous matter.

By this ſimple diſpoſition, the medullary ſubſtance of nerve is ſpread through all organized, ſenſible, or motive parts, forming a continuity which is probably the occaſion of ſympathy. Peculiar nerves, ſuch as the firſt and ſecond pairs, and the portio mollis of the ſeventh, terminate in an expanſe of medullary ſubſtance which combines with other parts and membranes, ſtill keeping the ſenſible exceſs of the peculiar medullary matter.

Reſtoration or repair of nerves.

The peculiar ſubſtance of nerves muſt in time become ineſficient; and, as it is liable to injuries, the powers of reſtoration, and repair, are extended to that material. The re-union of nerves after their diviſion, and the reproduction after part of a nerve has been cut away, have been eſtabliſhed by deciſive experiments. Whether there is any new medullary ſubſtance employed to fill up the break, and, if ſo, whether the new ſubſtance be generated at the part, or protruded along the nervous theca from the brain, are points undetermined: the hiſtory of the formation of a fœtus, the ſtructure of certain monſters, and the organization of ſimple animals, all ſeem to favour the probability, that the medullary matter of nerves is formed at the parts where it is required, and not in the principal ſeat of the cerebral medulla.

Whether the matter of nerve be not extenſively mixed in all irritable parts.

This doctrine, clearly eſtabliſhed, would lead to the belief of a very extended commixture of this peculiar matter in all the ſenſible and irritable parts of animals, leaving the nerves in their limited diſtribution, the ſimple office of conveying impreſſions from the two ſentient maſſes with which their extremities are connected. The moſt ſimple animals in whom no viſible appearances of brain or nerves are to be found, and no fibrous arrangement of muſcles, may be conſidered of this deſcription: Mr. John Hunter appeared to have had ſome incomplete notions upon this ſubject, which may be gathered from his repreſentation of a materia vitæ in his Treatiſe on the Blood, &c. Perhaps it would be more proper to diſtinguiſh the peculiar matter of muſcle by ſome ſpecific term, ſuch, for example, as materia contractilis.

Peculiar adaptation for the nerves of electric animals.

A particular adaptation for the nerves which ſupply the electrical batteries of the torpedo, and gymnotus, is obſervable on the exit of each from the ſkull; over which there is a firm cartilage acting as a yoke, with a muſcle affixed to it, for the obvious purpoſe of compreſſion: ſo that a voluntary muſcle probably governs the operations of the battery.

The matter of the nerves, and brain, is very ſimilar in all the different claſſes of animals.

The external configuration of animals is not more varied than their internal ſtructure.

Configuration and ſtructure of the various claſſes of animals.

The bulk of an animal, the limitation of its exiſtence, the medium in which it lives, and the habits it is deſtined to purſue, are each, and all of them, ſo many indications of the complexity or ſimplicity of their internal ſtructure. It is notorious that the number of organs, and of members, is varied in all the different claſſes of animals; the vaſcular and nervous ſyſtems, the reſpiratory, and digeſtive organs, the parts for procreation, and the inſtruments of motion, are ſeverally varied, and adapted to the condition of the ſpecies. This modification of anatomical ſtructure is extended in the loweſt tribes of animals, until the body appears to be one homogeneous ſubſtance. The cavity for receiving the food is indifferently the internal, or external ſurface, for they may be inverted, and ſtill continue

Very ſimple animals.

tinue to digeſt food; the limbs or tantacula may be cut off, and they will be regenerated without apparent inconvenience to the individual: the whole animal is equally ſenſible, equally irritable, equally alive: its procreation is gemmiferous. Every part is pervaded by the nutritious juices, every part is acted upon by the reſpiratory influence, every part is equally capable of motion, and of altering its figure in all directions, whilſt neither blood-veſſels, nerves, nor muſcular fibres, are diſcoverable by any of the modes of inveſtigation hitherto inſtituted.

In all animal ſtructures deſign is evident.

From this abſtract animal (if ſuch a term may be admitted) up to the human frame, the variety of acceſſory parts, and of organs by which a complicated machinery is operated, exhibit infinite marks of deſign, and of accommodations to the purpoſes which fix the order of nature.

In the more complicated animals much of their materials are not alive;

In the more complicated animals, there are parts adapted for trivial conveniences, much of their materials not being alive, and the entire offices of ſome liable to be diſpenſed with. The water tranſfuſed throughout the interſticial ſpaces of the animal fabric, the combinations with lime in bones, ſhells, and teeth; the horns, hoofs, ſpines, hairs, feathers, and cuticular coverings, are all of them, or the principal parts of their ſubſtance, extra-vaſcular, inſenſible, and unalterable by the animal functions after they are completed. I have formed an opinion, grounded on extenſive obſervation, that many more parts of animal bodies may be conſidered as inanimate ſubſtances; even the reticular membrane itſelf ſeems to be of this claſs, and tendons, which may be the condenſed ſtate of it; but theſe particulars are foreign to the preſent occaſion.

ſo that animated matter may be connected with inanimate.

The deduction now to be made, and applied to the hiſtory of muſcular motion, is, that animated matter may be connected with inanimate; this is exemplified in the adheſions of the muſcles of multi-valve, and bi-valve ſhell fiſh, to the inorganic ſhell, the cancer Bernhardus to the dead ſhells of other animals, and in the tranſplantation of teeth. All of which, although ſomewhat contrary to received opinion, have certainly no degree of vaſcularity, or vital connection with the inhabitant; theſe ſhells being liable to tranſudations of cupreous ſalts and other poiſonous ſubſtances, whilſt the animal remains uninjured. A variety of proofs to the ſame effect might be adduced, but it would be diſreſpectful to to this learned body to urge any farther illuſtrations on a ſubject ſo obvious.

Diviſion of the parts of an animal deſtroys the conformation;

The effects of ſubdiviſion, or comminution of parts among the complicated organized bodies, is unlike that of mineral bodies: in the latter inſtance, the entire properties of the ſubſtance are retained, however extenſive the ſubdiviſion; in the former ſubſtances, the comminution of parts deſtroys the eſſential texture and compoſition, by ſeparating the groſs arrangements of ſtructure upon which their ſpecific properties depend. From ſimilar cauſes it ſeems to ariſe, that animals of minute bulk are neceſſarily of ſimple ſtructure: ſize alone is not, however, the ſole cauſe of their ſimple organization, becauſe examples are ſufficiently numerous wherein the animal attains conſiderable bulk, and is of ſimple ſtructure, and *vice verſâ*: but, in the former, the medium in which they live, and the habits they aſſume, are ſuch as do not require extenſive appendages, whilſt the ſmaller complex animals are deſtined to more difficult, and more active exertions. It may be aſſumed however, as an invariable poſition, that the minuteſt animals are all of ſimple organization.

but leſs the more ſimple the ſtructure,

Life may on a ſmall ſcale be ſupported with ſimple materials; bulky animals require variety of organs.

Upon a ſmall ſcale, life may be carried on with ſimple materials; but the management, and proviſions for bulky animals, with numerous limbs, and variety of organs, and appendages of convenience, are not effected by ſimple apparatus; thus, the ſkeleton which gives a determinate figure to the ſpecies, ſupports its ſoft parts, and admits of a geometrical motion, is placed interiorly, where the bulk of the animal admits of the bones being ſufficiently ſtrong, and yet light enough for the moving powers; but the ſkeleton is placed externally, where the body is reduced below a certain magnitude, or where the movements of the animal are not to be of the floating kind: in which laſt caſe the bulk is not an abſolute cauſe. The examples of teſtaceous vermes, and coleopterous, as well as moſt other inſects, are univerſally known.

Thus large animals have their bones within them, ſmaller have them without.

Cryſtalline of the eye, muſcular.

The opinion of the muſcularity of the cryſtalline lens of the eye, ſo ingeniouſly urged by a learned member of this Society, is probably well founded; as the arrangement of radiating lines of the matter of muſcle, from the centre to the circumference of the lens, and theſe compacted into angular maſſes, would produce ſpecific alterations in its figure, This rapid ſketch of the hiſtory of muſcular ſtructure has been obtruded before the Royal Society to introduce the principal experiments, and reaſonings which are to follow: they are not ordered with ſo much exactneſs as becomes a more deliberate eſſay, but the intention already ſtated, and the limits of a lecture are offered as the apology.

Temperature of muſcles.

Temperature has an eſſential influence over the actions of muſcles, but it is not neceſſary that the ſame temperature ſhould ſubſiſt in all muſcles during their actions; neither is it eſſential that all the muſcular parts of the ſame animal ſhould be of uniform temperatures for the due performance of the motive functions.

All animals do produce heat; but are alſo affected by external communication.

It appears that all the claſſes of animals are endowed with ſome power of producing thermometrical heat, ſince it has been ſo eſtabliſhed in the amphibia, pices, vermes, and inſecta, by Mr. John Hunter; a fact which has been verified to my own experience; the term "cold-blooded" is therefore only relative. The ratio of this power is not, however, in theſe examples, ſufficient to preſerve their equable temperature in cold climates, ſo that they yield to the changes of the atmoſphere, or the medium in which they reſide, and moſt of them become torpid, approaching to the degree of freezing water. Even the mammalia, and aves, poſſeſs only a power of reſiſting certain limited degrees of cold; and their ſurfaces, as well as their limbs, being diſtant from the heart, and principal blood-veſſels, the muſcular parts ſo ſituated are ſubject to conſiderable variations in their temperature, the influence of which is known.

The colder animals.

In thoſe claſſes of animals which have little power of generating heat, there are remarkable differences in the ſtructure of their lungs, and in the compoſition of their blood, from the mammalia and aves.

Reſpiration: its organs in animals of higher temperature;

Reſpiration is one of the known cauſes which influences the temperatures of animals: where theſe organs are extenſive, the reſpirations are performed at regular intervals, and are not governed by the will, the whole maſs of blood being expoſed to the atmoſphere in each circulation. In all ſuch animals living without the tropics, their temperature ranges above the ordinary heat of the atmoſphere, their blood contains more of the red particles than in the other claſſes, and their muſcular irritability ceaſes more rapidly after violent death.

The

and in the cold-blooded:

The reſpirations of the animals denominated "cold-blooded," are effected differently from thoſe of high temperature; in ſome of them, as the amphibia of Linnæus, the lungs receive atmoſpheric air, which is arbitrarily retained in large cells, and not alternately, and frequently changed. The fiſhes, and the teſtaceous vermes, have lungs which expoſe their blood to water, but whether the water alone, or the atmoſpheric air mingled with it furniſh the changes in the pulmonary blood, is not known.

In inſects.

In moſt of the genera of inſects, the lungs are oborefcent tubes containing air, which, by theſe channels, is carried to every vaſcular part of the body. Some of the vermes of the ſimpler conſtruction have no appearance of diſtinct organs, but the reſpiratory influence is nevertheleſs eſſential to their exiſtence, and it ſeems to be effected on the ſurface of the whole body.

In all the colder animals, the blood contains a ſmaller proportion of the red colouring particles than in the mammalia, and aves; the red blood is limited to certain portions of the body, and many animals have none of the red particles.

Experiment: Cold blooded animals were included in water over mercury. After ſome days they died; but no gas was emitted, nor was the water changed.

The following animals were put into ſeparate glaſs veſſels, each filled with a pound weight of diſtilled water, previouſly boiled to expel the air, and the veſſels inverted into quickſilver; *viz.* one gold fiſh, one frog, two leeches, and one freſh-water muſcle.* Theſe animals were confined for ſeveral days, and expoſed in the ſun in the day-time, during the month of January, the temperature being from 43° to 48°, but no air bubbles were produced in the veſſels, nor any ſenſible diminution of the water. The frog died on the third day, the fiſh on the fifth, the leeches on the eighth, and the freſh-water muſcle on the thirteenth, This unſucceſsful experiment was made with the hope of aſcertaining the changes produced in water by the reſpiration of aquatic animals, but the water had not undergone any chemical alteration.

Hybernating animals can live under confined reſpiration.

Animals of the claſs mammalia which hybernate, and become torpid in the winter, have at all times a power of ſubſiſting under a confined reſpiration, which would deſtroy other animals not having this peculiar habit. In all the hybernating

Peculiarity of ſtructure in the heart and its veins.

mammalia there is a peculiar ſtructure of the heart, and its principal veins; the ſuperior cava divides into two trunks; the left, paſſing over the left auricle of the heart, opens into the inferior part of the right auricle, near to the entrance of the vena cava inferior. The veins uſually called azygos, accumulate into two trunks, which open each into the branch of the vena cava ſuperior, on its own ſide of the thorax. The intercoſtal arteries and veins in theſe animals are unuſually large.

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* *Mytilus Anatinus.*

Their habit and circumſtances that accompany the torpid ſtate, &c.

This tribe of quadrupeds have the habit of rolling up their bodies into the form of a ball during ordinary ſleep, and they invariably aſſume the ſame attitude when in the torpid ſtate: the limbs are all folded into the hollow made by the bending of the body; the clavicles, or firſt ribs, and the ſternum, are preſſed againſt the fore part of the neck, ſo as to interrupt the flow of blood which ſupplies the head, and to compreſs the trachea: the abdominal viſcera, and the hinder limbs are puſhed againſt the diaphragm, ſo as to interrupt its motions, and to impede the flow of blood through the large veſſels which penetrate it, and the longitudinal extenſion of the cavity of the thorax is entirely obſtructed. Thus a confined circulation of the blood is carried on through the heart, probably adapted to the laſt weak actions of life, and to its gradual recommencement.

The manner in which that ſtate commences and is afterwards terminated.

This diminiſhed reſpiration is the firſt ſtep into the ſtate of torpidity; a deep ſleep accompanies it; reſpiration then ceaſes altogether; the animal temperature is totally deſtroyed, coldneſs and inſenſibility take place, and finally the heart concludes its motions, and the muſcles ceaſe to be irritable. It is worthy of remark that a confined air, and a confined reſpiration, ever precede theſe phenomena: the animal retires from the open atmoſphere, his mouth and noſtrils are brought into contact with his cheſt, and enveloped in fur; the limbs become rigid, but the blood never coagulates during the dormant ſtate. On being rouſed, the animal yawns, the reſpirations are fluttering, the heart acts ſlowly and irregularly, he begins to ſtretch out his limbs, and proceeds in queſt of food. During this dormancy, the animal may be frozen, without the deſtruction of the muſcular irritability, and this always happens to the garden ſnail*, and to the chryſalides of many inſects during the winter of this climate.

(The concluſion in our next.)

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* *Helix nemoralis.*

VIII.

Deſcription of a Boring Tube, in general Uſe in America; but leſs known in this Country.

Deſcription of the American borer.

FIG. 2. *Plate* X. exhibits a very ſimple and ingenious borer, conſiſting of the common center bit of the carpenters followed by a wide flat thread ſcrew, hammered up from a plate of iron or ſteel. It is ſaid that they are uſed to bore holes ſeveral feet in length, and the peculiar property poſſeſſed by this inſtrument is, that it clears the cutting without requiring to be drawn out, as is the caſe with the augur, the gimblet, and other ſimilar tools. I do not, however, think that it would have this effect in boring perpendicularly down to conſiderable depths; but for horizontal or ſlightly inclined holes, its effect muſt fully anſwer.

Popular explanation of its action.

It may not at firſt occur to the reader why the introduction of this tool into a hole which muſt contain the wood that formerly blocked it up, ſhould not be attended with ſome degree of impediment or jamming; but this difficulty will vaniſh, when it is conſidered that the cuttings are, partly by their weight, and partly by friction againſt the internal cylindrical ſurface, prevented from revolving along with the ſcrew. The conſequence is that they are preſſed againſt its thread, and ſlide along it towards the handle. And as this motion or ſhifting of the thread is quicker than the motion of boring, by which the whole tool is carried inwards, the cuttings muſt come out with a velocity nearly equal to the difference of theſe two motions.

IX.

Geographical and Topographical Improvements. By JOHN CHURCHMAN, *Eſq. M. Imp. Acad. of Sciences, Peterſburgh.*[*]

Great value of topographical knowledge.

IT appears to be a matter of much importance to the people of any country, at all times, whether in war or peace; to poſſeſs a complete knowledge of its ſurface. In war, ſuch knowledge is abſolutely neceſſary for defence; in peace, for improving the country to the beſt advantage.

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[*] From the Tranſ. of the Soc. of Arts for 1804, who voted the ſilver medal to him for the ſame.

Now, ſince geography may be improved, an eaſy and accurate method to lay down maps of mountainous countries and hilly eſtates, will perhaps prove uſeful, as it will ſhow at a ſingle view the true ſhape and comparative height of the ground without the art of painting.

Utility of a correct delineation of hills, &c.

As mountains are apt to eclipſe each other, a perſpective view is ſeldom very extenſive, the rules of which fall ſhort of giving an accurate idea of any hilly country; becauſe ſuch a view, though ſtrictly true in one particular place, is not ſo in any other. The altitudes of mountains appear in proportion to the diſtance from the eye, and no rule in geometry has been found ſufficient to determine diſtances from any ſingle ſtation. Neither can a bird's-eye view of an eſtate aſcertain the depth of valleys or the height of mountains. But the method here propoſed will be found equally capable of giving the true ſhape of any ground above or below water. It may be ſucceſsfully applied to ſea charts, and will prevent much confuſion, ariſing from the tedious method of diſtinguiſhing ſoundings by a multitude of figures.

Mountains cannot be uſefully ſhewn in perſpective.

Explanation.

Suppoſe a full deſcription is required of any iſland in the ocean. Firſt, let an accurate map be laid down in the common way; and let the perpendicular height between the higheſt point of land and the ocean be divided into any number of equal parts. Suppoſe theſe equal diviſions are 100, 200, 300, 400 feet above the low-water mark. From the different points of theſe ſeveral diviſions, let horizontal lines be run with a good theodolite, and ſpirit level annexed, all round the iſland. If the work is well done, each line will end where it began; and if the bearings and diſtances of theſe ſeveral lines are truly laid down on the map, the crooked courſes of them will clearly ſhow the ſhape of the ground over which they paſs. For example: if any horizontal line paſſes by the ſide of a ſteep hill, it will incline towards the ocean, or approach the next horizontal line below it. When the ſame line croſſes a ſtream of running water or a valley, it will naturally bend up the ſide of the ſaid ſtream, until it can croſs it without loſing

New method. Make a good map or plan. Mark the point of higheſt elevation and other points differing in elevation by equal meaſures. Run lines with the theodolite and level from theſe points; in which if the ſurvey be good, they will again terminate. Theſe lines laid on the map will ſhew the figure of the country.

the level; or, in other words, it will bend towards the centre of the iſland. Hence, by a little practice, the ſhape of the ſeveral horizontal lines on the map will give as clear an idea to the mind, of the ſhape of any country over which they paſs, as a ſight of the country itſelf can convey to the eye. But to obtain a mathematical and true knowledge of the altitude and declivity of any part of the country, we have the following propoſition:

Trigonometrical rule for the declivities.

As the perpendicular height of any one horizontal line above another is to the radius: ſo is the horizontal diſtance between the horizontal lines meaſured on the map at any particular place: to the co-tangent of declivity at that place.

Note.—If the horizontal diſtance between any two horizontal lines on the map is equal to the perpendicular height of any horizontal line above another, the angle of altitude, or declivity, of any hill will be 45 degrees.

Advantages;

The preſent improvement, which I believe to be entirely new, will be found to poſſeſs the following advantages:

—to military men,

1ſt. Military men are well acquainted with the many advantages always to be gained from the exact repreſentation of high grounds. By this method, we are able to give the angle of altitude, the angle of declivity, and perpendicular height of every hill; likewiſe the comparative height of different hills, the beſt route by which the high grounds may be gradually aſcended, and where heavy burthens can be drawn up with moſt eaſe.

—and for domeſtic and economical purpoſes.

2dly. Experience has ſufficiently ſhown, that the inhabitants of low grounds are ſubject to different kinds of ſickneſs, from which thoſe living at places elevated to a certain degree are exempt. A map on this improved plan will point out the moſt proper ſituation for building dwelling-houſes. It will be uſeful in botany, in diſcovering or cultivating ſome kinds of plants which flouriſh beſt at particular diſtances above the level of the ocean. It will trace the line of vegetation on the ſides of lofty mountains, whoſe tops are covered with eternal ſnow.

—to direct agricultural improvements,

3dly. Some high lands are known to produce good grain, while low lands afford graſs more abundantly; but moſt grounds produce good graſs, over which a moderate quantity of running water is conveyed. A plan of any country in this way will ſhow all the ground that can be irrigated; where water-works may be erected; where navigable canals may be cut; and where high-ways and rail-roads may be laid out on the beſt and moſt level ground.

4thly. The ſubterraneous treaſures of the mineral and foſſil kingdoms are generally found in ſtrata; and if they are not truly horizontal, they make a certain angle with the horizon. A map on this projection may enable the mineralogiſt to follow any one ſtratum, at places even far diſtant from each other.

—and mine reſearches.

Problem.

To find the true declivity of any piece of ground, in any map laid down on the principles of the preſent plan.

Examples computed.

Example 1ſt. for D. *ſee Plate* IV.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 4 feet -	60206
	10.60206
To the co-tangent of the declivity, 45° -	10.00000

Example 2d. for B.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 8 feet -	90309
	10.90309
To the co-tangent of the declivity, 26° 34′	10.30103

Example 3d. for C.

As the perpendicular height, 4 feet - -	60206
Is to radius, 90° - - -	10.00000
So is the horizontal diſtance, 18 feet	1.25527
	11.25527
To the co-tangent of the declivity, 12° 32′	10.65321

The annexed ſurvey, *Plate* IX. of a ſmall lake and artificial mountain in the garden of his Excellency Count de Strogonoff, near St. Peterſburgh, has been cloſed by the tables of the difference of latitude and departure, as follows:

A ſurvey according to this method.

N 30 E

		N	S	E	W
N 30 E	2½	2.2	—	1.2	—
N 35 E	2	1.6	—	1.1	—
N 75 E	2	.5	—	1.9	—
N 55 E	2	1.1	—	1.6	—
N 45 E	3	2.1	—	2.1	—
N 52 W	2	1.2	—	—	1.6
N 59 W	3	1.5	—	—	2.5
S 56 W	12	—	6.7	—	9.9
S 60 E	7	—	3.5	6.1	—
		10.2	10.2	14.0	14.0

X.

A Memoir on the refining of Lead in the large Way. Containing ſome Reflections on the Inconveniencies reſulting from Cupels made of Aſhes; with a new and œconomical Method of conſtructing thoſe Cupels. By CITIZEN DUHAMEL.*

Proceſs for refining lead. Cupels made of aſhes.

EVERY one knows that in order to effect the ſeparation of ſilver from lead, a proceſs called refining or cupellation is made uſe of, which is effected in a baſon called a cupel; and it is likewiſe known that this baſon is formed of the aſhes or incinerated remains of animal or vegetable ſubſtances, after depriving them by waſhing of what ſaline matter they may contain.

The great quantity of wood aſhes required for making theſe cupels, and the difficulty of procuring it, have long ago induced me to ſeek for a more ſimple and leſs expenſive means of forming theſe veſſels.

Early obſervation that the oxide of lead penetrates chemical veſſels, while ſilver, if preſent, remains metallic.

The early chemiſts having obſerved that lead becomes oxided and converted into what is called litharge, when it is expoſed to fire with the contact of atmoſpheric air, while the ſilver it may contain, preſerves its metallic form; it only remained for them to contrive a method of ſeparating theſe two metals. They were led to this method by obſerving that the oxide of lead, in its ſtate of liquefaction eaſily penetrates the ſubſtances with which it may be in contact, particular bone aſhes, without deſtroying the figure of the veſſel made of that material. In fact there is no ſubſtance whatever which is better adapted to form the ſmall cupels for aſſaying.

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* This memoir is tranſlated from the Memoirs of the French National Inſtitute, Vol. III. at the requeſt of a correſpondent.

For want of bone aſhes the aſhes of wood are uſed for cupels; they have many inconveniences.

The difficulty, and often the impoſſibility of procuring three or four buſhels of bone aſhes in Germany, for each time of refining, has led to the adoption of the aſhes of wood. But not to mention that theſe are of conſiderable price, and not always to be had, they preſent another inconvenience, by often riſing and floating on the ſurface of the lead. When this happens, the proceſs muſt fail; and it does happen as often as the aſhes are ill prepared, or the cupel not ſufficiently or irregularly beaten, or that the openings left for the evaporation of the humidity are not properly diſpoſed, or enough in number, or cloſed by a portion of the ſcoriæ upon which the floor is made to receive the aſhes. This floor ought to be conſtructed of the moſt porous bricks, in order that the water with which the aſhes muſt be wetted may penetrate them and evaporate into the bed of ſcoria, and eſcape through the opening at the bottom of the furnace.

The elaſticity of this aqueous vapour frequently cauſes exploſions, which not only diſorder the cupel, but even the maſonry of the furnace, if it be not properly conſtructed.

Cupelling of lead.

In order to aſcertain the proportion of ſilver in any quantity of lead, it is only neceſſary to paſs a few pennyweights into a ſmall cupel of bone aſhes placed under the muffle of an aſſayer's furnace; as the lead becomes oxided it is imbibed in the cupel, and at length the phenomenon of brightening takes place upon the metallic button. This appearance ſhews that all the lead is diſſipated, and the remaining ſilver in a pure ſtate.

Refining in the large way cannot be done by the aſſaying proceſs wherein the oxide is abſorbed.

In the large way of refining, the ſame object of ſeparating the ſilver from the lead is aimed at, but the lead is not intended to penetrate into the cupel, which in fact is impracticable. For the total abſorption of this metal would require a much greater quantity of aſhes, with the conſumption of a ten fold portion of time and fuel; beſides which, the loſs in recovering the lead by fuſion of the cupel is very expenſive and the product leſs conſiderable than in the common way. The oxide of lead obtained in this laſt method may be eaſily fuſed, and reduced if needful; but it is an article of value in the arts, and therefore very acceptable in the market in its ſtate of oxide.

Veſſels of clay uſed in ſmelting lead.

The lead ores and litharge may be fuſed as is done in England and Britanny, in a reverbatory furnace, of which the floor or baſon is formed of moiſtened and rammed clay. Theſe floors reſiſt the action of fire as well as that of the oxide of lead during ſix or eight months conſtant work.

Theſe are applicable to refining.

The durability of theſe baſons of earth firſt gave me a notion of the method I ſhall propoſe for refining furnaces, in which the intention is to oxide the lead in order to obtain litharge, and not to cauſe it to be totally abſorbed in the cupels, as is done when the metal is aſſayed, to ſhew how much ſilver it contains.

It would be preferable that no abſorption took place.

In the operation upon a large ſcale the cupel, though of aſhes abſorbs only part of the lead, as I have already remarked, obſerving at the ſame time that it would be much more advantageous to obtain the whole converted into litharge, of which the reduction into lead is infinitely more eaſy than that of the oxide contained in the aſhes, which reſiſt fuſion, and afford a ſcoria that always contains ſome metal.

Engliſh proceſs. 12 tons of lead are oxided with ſcarcely any abſorption.

Upon a cupel of aſhes rammed into an oval ring of iron, about five feet in length and three and a half in width, the Engliſh refine in ſucceſſion about twelve ton of lead, which becomes converted into fine merchantable litharge, with the exception of the ſmall portion that penetrates the cupel, of which the thickneſs is leſs than three inches. This cupel is ſupported under the roof of the furnace by two bars of iron. The litharge is driven by the blaſt of bellows towards the anterior part of the furnace, whence it falls without interruption upon the area of the foundery; and at the ſame time to ſupply the ſpace which this ſubtraction of oxide would leave, a pig of lead is gradually advanced into the interior of the furnace, placed on one ſide of the nozel of the bellows. This lead, by its gradual fuſion, keeps the cupel full till towards the end of the operation.

I have given this ſketch of the Engliſh proceſs only to ſhew that it is poſſible to refine lead with very little expence of aſhes for forming the cupels. Thoſe here mentioned do not abſorb eighty pounds of oxide out of the large quantity of lead thus refined.

The veſſel is very cheap.

Hence we ſee that metallurgiſts have always endeavoured to obtain the greateſt poſſible quantity of litharge and the leaſt of aſhes containing oxide; but as they did not imagine they could depart from the docimaſtic proceſs, they have conſtantly made their cupels of aſhes.

We have ſeen that in the ſmall proceſs of cupellation the lead penetrates the aſhes as it becomes oxided, and that when no more lead remains the ſmall button of ſilver remains pure at the bottom in the ſpherical form. This operation is effected with more ſpeed becauſe the ſurface of the bath is always convex in theſe ſmall veſſels, and conſequently the litharge runs off on all ſides towards the edge of the cupel, where it is immediately imbibed.

The litharge runs off in the ſmall proceſs;

This is not the caſe in large cupels of ſeveral yards diameter. Bellows muſt be uſed, not only to accelerate the oxidation by their blaſt, but to drive the litharge towards the paſſage or gutter which is left for its iſſue.

—but cannot in the large.

The inconveniencies and even the impoſſibility of cauſing all the lead to penetrate the aſhes of theſe large cupels have been already remarked. This muſt be evident on reflecting that the oxidation can take place only at thoſe parts of the bath which are expoſed to the contact of the air and the blaſt of the bellows. The litharge near the middle of the baſon not being diſpoſed to flow towards the edge, would cover and defend the metal from any farther oxidation. Hence it is that the operators have found themſelves obliged to drive out the litharge by the mechanical action of a ſtream of air from bellows.

It muſt be driven off by bellows.

The oxidation therefore takes place only at the ſurface of the lead, and not lower; if it were otherwiſe, the aſhes of the cupel would be penetrated with oxide to a depth which would be more unequal the longer the operation laſted. Now I have always remarked that the portion of aſhes thus imbibed in the large refinery is not thicker towards the center of the baſon than towards its edge, though the lead remains thirty or forty times longer at the bottom than near the edges, becauſe the

The oxidation is effected only at the ſurface.

bath conſtantly diminiſhes till all the lead is reduced into litharge, and nothing at laſt remains but the button of ſilver at the bottom of the cupel.

Other reaſons why the large proceſs cannot be made by abſorption.

That the whole of the lead is abſorbed in the cupel of aſſay, ariſes from the unequal action of heat upon every part of the ſmall veſſel. As the cupel in the large way preſents only its ſuperior ſurface to the action of the heat, the oxide thus imbibed ceaſes to penetrate at the place where the temperature is no longer in a ſtate to hold the oxiding fuſion. For this reaſon it is that the whole of the cupel is throughout impregnated to an equal depth, and it is impoſſible to cauſe all the lead to penetrate the aſhes.

Litharge is ſought as a product in the large way.

From the preceding obſervations it will be eaſy to conclude that though the aſſay of lead muſt be made in ſmall cupels of bone aſhes, in order that the whole of the oxided metal may either be abſorbed or partly evaporated; yet the caſe is very different in the large operation, where the object is to proceed with celerity, and to obtain as much litharge as poſſible.

Addition of ſand to the wood aſhes, &c.

I have before ſtated that the wood aſhes uſed in forming large cupels are expenſive, and frequently not to be procured in ſufficient quantity; to which I have added their being ſubject to blow up or riſe entirely, which occaſions a conſiderable loſs. It muſt further be mentioned, that in order to give more weight and conſiſtence to theſe cupels it is often neceſſary to mix a conſiderable quantity of ſand with them, particularly if the lead ſhould contain foreign ſubſtances, ſuch as arſenic, cobalt, antimony, zinc, tin and other matters. If the lead be merely arſenical, after having taken off the firſt ſcum, it is uſual to throw from time to time, on the whole ſurface of the bath, about 20lbs. of iron ſcales or granulated crude iron. This iron being lighter than the lead, floats on the top and abſorbs the arſenic, after which it is cleared away, and then the litharge is formed without any obſtacle. This method is uſed in Saxony.

The neceſſity of adding ſand to the aſhes of the cupels ought to have led to a diſcovery which I propoſe: it is as follows.

New Conſtruction of the Cupels or Baſons for refining Lead.

New conſtruction of the baſons or veſſels for refining lead.

Without making any change in the maſonry of the furnaces for refining by what we call the German method, it is only neceſſary to be careful to make a ſufficient number of vents in their baſe for the evaporation of the moiſture, and to diſpoſe of them ſo as moſt effectually to anſwer this purpoſe. Theſe channels or vents are to be covered with a bed of ſcoria, upon which a pavement is to be made of the moſt porous bricks, and of the thickneſs of a ſingle brick.

On this area or pavement, which ought to be concave like the baſe upon which the aſhes of the ordinary cupels are placed, muſt be laid a quantity of founder's ſand, a little moiſtened. If it be not adheſive enough, a little clay may be added, in order to give the requiſite ſolidity, and the whole carefully mixed. The ſand muſt be rammed down in the ſame manner as is done to conſolidate the aſhes in the uſual way, and a baſon for refining muſt be formed, equally rammed in all its parts. The thickneſs of this cupel ſhould be about ſix inches; and it may be made in two layers, as we ſhall hereafter obſerve.

The cupel is made of ſand with a little clay inſtead of bone aſhes.

After the baſon has been in all parts uniformly beaten or rammed down, it will be proper to ſift over the whole ſurface two or three quarts of wood aſhes, which may be made to adhere by ramming.

When the cupel is thus prepared, the head of the furnace muſt be lowered, and a moderate fire kept up for ſeveral hours, in order to evaporate part of the water from the ſand. The reſt will be driven out, without inconvenience, through the vents during the refining.

It may be dried before uſe;

After a ſufficient drying, which may even be diſpenſed with; the head is to be raiſed, the cupel, ſuffered to cool a little, and ſtraw or hay then laid upon it, and upon this the pigs or pieces of lead, which are to be gently put down, in order that their weight may not make impreſſions in the ſand. The ſtraw is uſed for this purpoſe, in our method as well as the common method; and it would be convenient that the lead ſhould be caſt in iron hemiſpherical moulds or pots inſtead of the priſmatic form, as theſe pieces would be leſs ſubject to damage the cupel.

—but this is not abſolutely neceſſary.

Method of charging with lead.

When the quantity of lead neceſſary to fill the cupel is arranged in the furnace, the head is to be lowered and luted all round with clay, after which the fire is to be applied as in the uſual proceſſes.

When the lead is in perfect fuſion, and the bath covered with droſs and coally matter from the ſtraw, this ſkum muſt be

Fuſion.

Clearing of the ſurface.

raked off through the paſſage left for the litharge, by means of a wooden rake about a foot long, with an iron handle of ſufficient length to reach every part of the bath.

Method of blowing, &c.

When the lead has been ſeveral times ſkimmed, and begins to become red, the bellows muſt be ſet into action, gently at firſt, and afterwards more ſtrongly. Their nozles muſt be ſo diſpoſed that the blaſt may be directed towards the center of the bath, and in order that the wind may be urged upon the ſurface of the metal, each nozle muſt have a ſmall round plate of iron adapted to it. Theſe ſmall flaps or valves, called papillons (flies or butterflies) are uſed in the German fineries. They have an hinge at top, and at every ſtroke they riſe about half way from their perpendicular poſition towards the level, ſo that by reflecting the air downwards upon the lead, they haſten its oxidation.

Gutter for the litharge.

When all the droſs has been removed, and the lead is of a good red heat and covered with litharge, a little gutter muſt be made, with an hook appropriated for this purpoſe, in the ſand of the cupel. This muſt be carefully done until the bottom of the gutter anſwers to the level of the bath. The litharge driven by the blaſt of the bellows will flow out of this paſſage, and fall upon the hearth of the foundery.

Inſtructions.

When the operator perceives that only a ſmall quantity of litharge remains near the gutter, he will ſtop its eſcape with a ſmall quantity of moiſtened aſhes; but as ſoon as the lead ſhall be again covered with oxide, the paſſage muſt be opened and made deeper as the quantity of matter becomes depreſſed; taking care that no lead eſcapes, particularly towards the end of the operation, as it would carry along with it a large portion of ſilver which would be loſt.

In this manner the proceſs is to be carried on until the ſurface of the ſilver exhibits thoſe flaſhes which are called the brightening, taking care to raiſe the fire in proportion to the diminution of the bath, particularly towards the end, when the ſilver is collected; and as this metal is much more difficult to be kept in fuſion than its ſmall alloy of lead, the refining will be imperfect, unleſs the temperature be raiſed, and inſtead of about one twentieth of lead, which the ſilver uſually retains in the German method, it will remain much more highly charged. This would render it more difficult to be treated in the ſecond operation, called the ſilver refining, or by the Germans *ſilver brenen*, by which it is rendered pure.

Thoſe who are accuſtomed to refine lead in the German method, will find no difficulty in following mine. For though the cupel is made of ſand inſtead of aſhes, there is no difference in the manipulations.

This method admits of keeping up the charge during work,

We have ſeen that the Engliſh refine a great quantity of lead on a ſmall cupel. The ſame may be done in the method here deſcribed, by adding in proportion as the loſs by oxidation takes place. Suppoſing the capacity of the cupel to be ſuch as to contain about five ton of lead, we might continue the operation to three times that quantity in a ſingle proceſs, which would not have the inconveniences of the Engliſh method.

and the cupel may be repeatedly uſed.

I flatter myſelf that a well-made cupel of ſand may be uſed for ſeveral refinings, without requiring to be made up again every time like thoſe of aſhes; but in theſe circumſtances, and before the lead is put in, it is neceſſary to fill with well beaten ſand the gutter or opening which was before made for carrying off the litharge. Care muſt be taken in doing this to remove with a chiſſel that kind of glaze which the oxide of the lead leaves behind it. With this precaution, after wetting the part where the new moiſtened ſand is to be applied, they will firmly unite together.

From the long duration of the earthen floors of thoſe reverberatory furnaces in which lead ores and even litharge are fuſed, as was before mentioned, we have no reaſon to apprehend any bad conſequences from the oxide of lead, which acts only on the ſurface of the cupel, and penetrates to an inconſiderable depth.

Recovery of the abſorbed lead.

After one or two refinings, this cruſt of oxide may be taken off and fuſed in a blaſt furnace, in order to recover the lead; a proceſs no leſs eaſy than that of reducing the metal which exiſts in much greater quantity in the ordinary cupels. We therefore obtain a larger quantity of litharge, which is one advantage, and in addition to this, the loſs in ſilver which accompanies the abſorbed lead will be leſs. For in the ſmall quantity of precious metal which accompanies the oxide, it is found by experiment, that the proportion is greater in the abſorbed lead than in that which is driven over in litharge.

To make the cupel of clay inſtead of ſand would be more expenſive and leſs convenient.

Inſtead of ſand we might make uſe of clay in conſtructing our cupels, as is done in the hearths of the reverberatory furnaces of Brittany; but it would then be neceſſary to pound the earth repeatedly for ſeveral days, otherwiſe it would crack, and theſe cracks, which would become wider by the ſhrinking from heat, would afford a lodgment for ſome of the lead; an inconvenience which ſand, even if rather loamy, does not preſent. It muſt alſo be remarked, that a cupel of clay would become too hard to admit of the excavation for carrying off the litharge; ſo that this part at leaſt would require to be made of ſand or aſhes.

Two kinds of ſand.

It will be advantageous to uſe two kinds of ſand in forming a baſon of the cupel, the one fine ſuch as the founders' ſand, and the other coarſe. The latter may form the firſt ſtratum, which, after being well rammed with the implements uſed for this purpoſe, muſt be left about three inches in thickneſs. Upon this the fine and ſomewhat loamy ſand is to be ſpread and rammed like the firſt. A ſlight degree of moiſture muſt be uſed with both theſe, in order that they may more ſolidly adhere together. The lower ſtratum being more coarſe, will facilitate the eſcape of the humidity,

Repair or renewal of the cupel.

It will not be neceſſary to diſturb the lower ſtratum of ſand when a new cupel is to be made; and even of this laſt that portion which has not imbibed any oxide may be uſed along with the new ſand intended to be applied. The lower ſtratum muſt not be touched during this renewal, for fear of mixing coarſe ſand with the fine. This inconvenience may be guarded againſt by ramming upon the ſurface of the coarſe ſand a bed of a thin facing of aſhes, at which the operator muſt ſtop when he takes away the upper ſtratum.

We have remarked that the founders' ſand muſt be rather loamy, and that if it be not ſo, it will be neceſſary to add a ſmall quantity of clay to render it adheſive; but as it is neceſſary that this clay ſhould be equally diffuſed through the maſs, it may be diffuſed in the water with which the ſand is to be ſprinkled, and the whole muſt be carefully mixed.

Abſorption is of no advantage in the old proceſs.

It might be objected, that ſince the cupels of ſand do not abſorb ſo much litharge as thoſe of aſhes, more time will be required to complete the refining, becauſe the oxide inſtead of being in part abſorbed, muſt by this new proceſs be driven out of the furnace. This however is a ſubject which ought not to be conſidered as of any importance; for the blaſt of the bellows well directed will cauſe the oxide of litharge to flow out more abundantly through the gutter than if the abſorption took place.

I have ſeen operators in Germany, who, when they conſtructed their cupels, had the precaution to form a ſmall circular cavity in the middle, the depth of which was proportioned to the quantity of ſilver, which from the aſſay they knew to be contained in the lead of one operation. By this contrivance there were no inſulated grains of the metal left on the ſurface of their cupel, but the whole of the ſilver formed a perfectly round cake in the middle of the center excavation. I would adviſe the ſame ingenious expedient to be uſed in the cupels of ſand. Improvement. Cavity to receive the ſilver.

I am well aſſured that the cupels I propoſe, if made with care and attention, will ſucceed perfectly, and that, independent of their convenience beyond the others, they will be found very economical. I am deſirous that, for the advantage of metallurgy, this method ſhould be generally uſed, and its benefits will prove that we ought not always to follow with ſervility the eſtabliſhed uſages nor the common working proceſſes. Concluſion.

XI.

Letter from Mr. JAMES STODART, *explaining the Method of gilding upon Steel by Immerſion in a Liquid, which has lately engaged the public Attention in various Articles of Manufacture.*

To Mr. NICHOLSON.

DEAR SIR,

A Conſiderable degree of public intereſt and curioſity has lately been excited by the exhibition of inſtruments of ſteel coated or gilt with gold. The diſcovery, although not altogether new, does not appear to be very generally known; and as its application to various manufactures promiſes to be both elegant and uſeful, I have, with a view of ſaving ſome time and expence to others who may be inclined to make further experiments, added a ſhort account of a method which with me has ſucceeded perfectly well. I wiſh here in juſtice to obſerve, that this diſcovery of the method belongs more to my friend Mr. Hume, chemiſt, Long-Acre, than to myſelf. With that gentleman's kind aſſiſtance I had but few difficulties Inſtruments of ſteel gilt with gold.

Account of the proceſs. Three parts ſulphuric ether are added to one of ſolution of gold. The gold is taken up by the ether.

to overcome. The following is our method: To a ſaturated ſolution of gold in nitro-muriatic acid, add about three times the quantity of pure ſulphuric ether: Agitate them together for a ſhort time. The gold will ſoon be taken up by the ether in the form of a muriate, or nitro-muriate of gold, leaving the remaining acid colourleſs at the bottom of the veſſel, which muſt now be drawn off by means of a ſtop-cock, or other

The inſtrument to be dipped into the ethereal ſolution.

ſimilar contrivance. The acid being diſcharged, the inſtrument to be gilt having been previouſly well poliſhed and wiped very clean, is to be dipped for an inſtant into the ethereal ſolution, and on withdrawing it, as inſtantly waſhed by agitation in clean water. This is eſſential to get clear of a ſmall portion of acid neceſſarily taken up with the metal; and if this be neatly done, the ſurface of the ſteel will be completely and very beautifully covered with gold. Some little degree of dexterity is required to perform the whole operation well.

Eſſential oils do not ſucceed well.

I have tried ſome of the eſſential oils, knowing that they will take the gold from nitro-muriatic acid; but as far as I went they did not apply for the purpoſe of gilding: and as I had found all I wanted in ether, I certainly did not proſecute the other experiments with much induſtry.

I remain, with much reſpect,
Dear Sir,
Your obliged ſervant,
J. STODART.

Strand, June 24, 1805.

XII.

On the peculiar Noiſe emitted by Water before it acquires the Temperature of boiling; which is commonly denoted by the Word Simmering. W. N.

Introduction.

SOME time ago a philoſophical friend who favoured me with a viſit, mentioned in converſation that the ſimmering of water before it boils had formed the ſubject of enquiry between himſelf and other curious examiners of natural appearances, as being a fact not yet well explained. It ſeemed to me very remarkable, as I dare ſay it will to my readers, that any of the facts relating to the boiling of water, concerning which ſo much has been ſaid and written, ſhould ſtill remain in obſcurity. Having, myſelf, been in the habit of conſidering it as the conſequence of a rapid eſcape of interſperſed air from the heated water, I requeſted he would ſay why he thought the phenomenon repugnant to that ſuppoſition. My friend proceeded in reply to ſtate, that when water is firſt put on the fire, in a metallic veſſel, the veſſel itſelf gradually becomes lined with bubbles; that theſe bubbles become detached and riſe, ſo that the whole ſurface, or at leaſt the bottom, becomes clear again; that, ſoon afterwards a rattling noiſe is heard, ſharp and metallic, which encreaſes in loudneſs until it almoſt perfectly reſembles the pouring of ſmall ſhot into the veſſel; that the fluid continues in a ſtate of tranquillity and tranſparency during this ſtate; and laſtly, that when the noiſe is loudeſt the ſtate of ebullition ſuddenly comes on, the peculiar noiſe of ſimmering ceaſes all at once, and nothing is heard but the ſoft and moderate noiſe of aquatic agitation as long as the boiling laſts.

Particular deſcription of the act of ſimmering.

This account, ſo remarkable for its preciſion and accuracy, ſhewed clearly that my notion, which I believe is the common opinion, was ill founded. After a little meditation, it appeared evident to me that the noiſe of ſimmering muſt ariſe from the collapſion of ſteam bubbles, formed at the bottom of the veſſel, and condenſed almoſt inſtantly upon their aſcent in the fluid not yet heated to the boiling point. In ſupport of this opinion I ſhewed him a common experiment with the water-hammer. This inſtrument, which is made and ſold by the glaſs-blowers and barometer-makers, conſiſts of a tube, nearly a foot in length and about three quarters of an inch in diameter, terminating in a globe of about two inches in diameter; the other end of the tube being cloſed. The outer extremity of the globe ends in a capillary tube, through which as much water has been introduced as rather more than fills the globe itſelf. This water has been boiled in the veſſel or inſtrument, and at the time of boiling, when all the internal cavity not containing water was filled with ſteam to the almoſt total excluſion of air, the capillary aperture was hermetically cloſed. The inſtrument thus completed is found when cold to contain water and a ſpace nearly vacuous, and the experiment from which it derives its name of

It ſeems to ariſe from the ſudden production and condenſation of ſteam.

Deſcription of the water-hammer.

Singular noiſe of water agitated in vacuo.

Another experiment of ſteam produced and condenſed twelve times in a ſecond,

applied to explain the effect of ſimmering.

of the water-hammer, is that of agitating its contents. The remarkable effect it exhibits is, that the parts of the water ſtrike againſt the glaſs and againſt each other, with the ſharp noiſe uſually produced by the colliſion of hard bodies. Another experiment, which I alluded to, is that, if the ball be held downwards ſo as to become filled with the water, and it be then ſlowly raiſed up, ſo as to bring the tube nearer to the horizontal poſition, the heat of the hand which holds the tube will produce vapour or ſteam ſtrong enough to prevent the fluid from running down to its level from the upper ſurface of the globe; but a little farther inclination cauſes it to deſcend, and a bubble of the ſteam enters the globe itſelf and aſcends through the fluid. But it ſcarcely arrives at the top before it is condenſed, and the water collapſes with a ſmart ſtroke or noiſe, ſo as to fill the globe again. The ſucceſſion of theſe bubbles and their condenſation take place ſo rapidly, in a well made inſtrument, that ten or twelve collapſions occur in every ſecond of time. The fact, and the obvious remarks I made upon it, convinced my friend that I had ſuggeſted the proper explanation.

Objections.

It was my intention however to have heated ſome water in a veſſel in order to obſerve and aſcertain the progreſſive appearances, but I had not done it when I again had the pleaſure to meet this intelligent obſerver. He complimented me upon the ingenuity of my ſolution, but having himſelf ſince repeated the experiment of boiling water, it ſeemed from his report that ingenuity was all the value it could claim. "Take a bright tin veſſel" ſaid he "and heat water in it; you will hear the noiſe, but no bubbles are to be ſeen."

I took the earlieſt opportunity of making ſome experiments, the particulars and reſults of which are as follow:

Exp. 1. Water was heated in a glaſs retort. The ſimmering was attended with ſteam bubbles.

Exp. 1. A ſmall glaſs retort, the body of which is about two inches in its ſhorteſt or horizontal diameter, was ſuſpended ſo that its neck was elevated about twenty degrees above the level. Water was then poured in to fill the body and the greateſt part of the neck. My intention in filling the neck was, that I might be able to obſerve whether any greater or more ſudden riſe took place before the period of boiling than the well known expanſion of the fluid. A ſmall ſpirit-lamp was placed beneath the bulb. The coldneſs of the water in the veſſel immediately condenſed a portion of the water which iſſued from the flame itſelf, and formed drops on the outſide. As the included water became heated this condenſed water evaporated, and left the ſurface again clear; and at this period the diſſeminated air began to ſeparate and gave a duſty appearance to the inner ſurface, which laſted about three minutes. At the end of this time the inner ſurface began to clear; the peculiar noiſe of ſimmering was heard; and bubbles were ſeen ſuddenly appearing and collapſing; the retort itſelf being agitated and the ſurface of the water riſing and falling by ſtarts. The bubbles were pointed at top, ſomewhat reſembling ſmall flames ſuddenly appearing and vaniſhing at different parts of the ſurface. In the courſe of one minute they grew larger and larger and collapſed at greater heights, until at length they eſcaped through the fluid without being condenſed. This was the inſtant of ebullition or boiling and at this period the noiſe of ſimmering ceaſed and that of boiling was heard.

Exp. 2. Water was heated in a larger glaſs veſſel. The effects were rather more evident.

Exp. 2. As this effect appears to ariſe decidedly from the upper water being colder than that near the bottom of the veſſel, it was natural to infer that the appearances would be different according to its figure and magnitude. I therefore took a bolt-head, or ſpherical glaſs body, with a ſtraight neck: Its diameter was four inches; and when it was filled, a column of water eight inches long ſtood in its upright neck. The thickneſs of the glaſs at its bottom was conſiderable. At 35 minutes after three the lamp was lighted. At 40 minutes bubbles of gas roſe ſingly, and very little of the duſty appearance was ſeen. At 58 minutes the noiſe of ſimmering began, and the collapſing bubbles were plentiful and diſtinct. Little ſtreams or fountains of ſteam roſe from particular points and were condenſed; and ſome globes of half an inch diameter aſcended clear of the bottom and collapſed in the fluid above. At one minute after four the bubbles reached the top of the fluid, without collapſing, and at this time the noiſe of ſimmering ceaſed. The lamp was then blown out.

Exp. 4. The experiment was repeated in a copper veſſel.

Exp. 4. A bright copper hemiſphere, four inches in diameter, was filled with water at the temperature of 60 degrees. At four hours three minutes the lamp was lighted and water became condenſed on the outſide. At five minutes the inſide ſurface had a duſty appearance, from bubbles of air immediately over the flame. The temperature was then 110°, and the outſide dry. At ſix minutes bubbles of gas or air were detached and roſe, the temperature being 126°, and the vapour of ſteam being viſible from the ſurface of the water. At eight minutes the inſide ſurface was coated with large bubbles or beads of air; temperature 150°. At nine minutes, temp. 165°, much vapour. At 9½ min. temp. 175°, the bottom was clear of bubbles, and the noiſe of ſimmering began. At 10 min. temp. 184°, the ſteam collapſing bubbles were very evident, though, from their pointed ſhape, they were not immediately obvious to an obſerver looking ſtraight down into the ſhining veſſel. At 11 min. temp. 180°, noiſe very loud and bubbles more and larger. At 12 min. temp. 185°, ſome of the bubbles broke at the ſurface, and the noiſe was leſs. At 12¾ min. temp. 204°, the water boiled, and the ſimmering noiſe was ſucceeded by that of boiling. At 14 min. the lamp was extinguiſhed.

Exp. 5. Water already heated does not ſimmer ſo much or ſo loudly as water quickly raiſed from a low temperature.

Exp. 5. The water was ſuffered to cool to 170°, and the lamp was then lighted again, namely, at four hours 18 minutes. At 20 min. temperature 180°, the ſimmering began; but not till after the ſteam bubbles were ſeen very large: and ſoon afterwards, at 204°, they roſe through the fluid, and the boiling took place by fountains or ſtreams of bubbles riſing from particular points. The ſimmering noiſe in this experiment was much leſs than before.

The thermometer was placed horizontally, with the greateſt part of its tube, and part of its bulb, out of the water. When the bulb was plunged in the water, it ſhewed 208°.

Concluſion.

From theſe facts it appears to be clearly eſtabliſhed, that the cauſe firſt mentioned, namely, the condenſation of ſteam bubbles in their aſcent through the cold fluid above, is the occaſion of the noiſe of ſimmering. In the fifth experiment the ſuperincumbent water was hotter than that beneath, and conſequently the ſimmering could not be produced but by ſteam at a higher temperature, and even then the collapſion of the water was leſs ſudden and the noiſe leſs loud.

XIII.

Experiments on Wootz. By Mr. DAVID MUSHET. *From the Philoſophical Tranſactions,* 1805.

THE following experiments were made at the requeſt of Sir Joſeph Banks, on five cakes of wootz, with which he ſupplied me for that purpoſe. As the cakes, which were numbered 1, 2, 3, 4, 5, were not all of the ſame quality, it will be proper firſt to deſcribe the differences obſervable in their external form and appearance.

Experiments on five cakes of wootz.

No. 1 was a denſe ſolid cake, without any flaw or fungous appearance upon the flat, or, what I ſuppoſe to be, the upper ſide. The round or under ſurface was covered with ſmall pits or hollows, two of which were of conſiderable depth; one through which the ſlit or cut had run, and another nearly as large towards the edge of the cake. Theſe depreſſions, the effects, as I ſuppoſe, of a ſpecies of cryſtallization in cooling, were continued round the edges, and even approached a little way upon the upper ſurface of the wootz.

Deſcription of the cake, No. 1.

The cake was a quarter of an inch thicker at one extremity of the diameter than at the other, from which I infer, that the pot or crucible, in which this cake had been made, had not occupied the furnace in a vertical poſition. Its convexity, compared to that of the other five, was ſecond. Upon breaking the thin fin of ſteel, which connects the half cakes together, I found it to poſſeſs a very ſmall denſe white grain. This appearance never takes place but with ſteel of the beſt quality, and is leſs frequent in very high ſteel, though the quality be otherwiſe good.

Fractured very ſteely.

Upon examining the break with attention, I perceived ſeveral laminæ and minute cells filled with ruſt, which in working are never expected to unite or ſhut together. The grain otherwiſe was uniformly regular in point of colour and ſize, and poſſeſſed a favourable appearance of ſteel.

No. 2. This cake had two very different aſpects; one ſide was denſe and regular, the other hollow, ſpongy, and protuberant. The under ſurface was more uniformly honey-combed than No. 1; the convexity in the middle was greater, but towards the edges, particularly on one ſide, it became flatter. The grain expoſed by breaking was larger, bluer in colour, and

Deſcription of the cake of wootz, No. 2.

and more ſparkling than No. 1. In breaking, the fracture tore but ſlightly out, and diſplayed the ſame unconnected laminæ with ruſty ſurfaces, as was obſerved in No. 1. Beſide theſe, two thin fins of malleable iron projected from the unſound ſide, and ſeemed incorporated with the maſs of ſteel throughout. Towards the centre of the break, and near to the excreſcence common to all the cakes, groups of malleable grains were diſtinctly viſible. The ſame appearance, though in a ſlighter degree, manifeſted itſelf in various places throughout the break.

Cake of wootz, No. 3.

No. 3. The upper ſurface of this cake contained ſeveral deep pits, which ſeemed to reſult from the want of proper fluidity in fuſion. They differed materially from thoſe deſcribed upon the convex ſides of No. 1 and 2, and were of that kind that would materially effect the ſteel in forging.

The under or convex ſide of this cake preſented a few cryſtalline depreſſions, and thoſe very ſmall; the convexity was greater than that of No. 1 and 2, the fracture of the fin almoſt ſmooth, and only in one place exhibited a ſmall degree of tenacity in the act of parting. In the middle of the break, about half an inch of ſoft ſteel was evident; and in different ſpots throughout numerous groups of malleable grains, and thin laminæ of ſoft blue tough iron made their appearance.

Cake of wootz, No. 4.

No. 4. Was a thick denſe cake poſſeſſed of the greateſt convexity; the depreſſions upon the under ſide were neither ſo large, nor ſo numerous as thoſe in No. 1 and 2, nor did they approach the upper ſurface of the cake further than the acute edge. This ſurface had the moſt evident marks of hammering to depreſs the feeder, or fungous part of the metal, which in the manufacturing ſeems the gate or orifice by which the metal deſcends in the act of gravitation.

The break of this cake, however favourable as to external appearance, was far from being ſolid. Towards the feeder it ſeemed looſe and crumbly, and much oxidated. The grain divided itſelf into two diſtinct ſtrata, one of a denſe whitiſh colour, the other large and bluiſh, containing a number of ſmall ſpecks of great brilliancy. Several irregular lines of malleable iron pervaded the maſs in various places, which indicated a compound too heterogeneous for good ſteel.

Cake of wootz, No. 5.

5th cake. This was materially different in appearance from any of the former. It had received but little hammering, yet was ſmooth and free from depreſſions, or honey-comb on both ſurfaces. The feeder, inſtead of being an excreſcence, preſented a deep concave beautifully cryſtallized.

In breaking, the fracture tore out conſiderably, but preſented a very irregular quality of grain. That towards the under ſurface was ſmall and uniform, but towards the flat or upper ſurface it increaſed in ſize, and in the blueneſs of its colour, till it paſſed into the ſtate of malleable iron.

The break of this ſteel, though apparently ſoft, was the leaſt homogeneous of the whole, and throughout it preſented a very brilliant arrangement of cryſtal, which in other ſteel is always viewed with ſuſpicion.

General Remark.

Uniformly the grain and denſity of the wootz are homogeneous, and free from malleable iron towards the under or round ſurface; but always the reverſe towards the feeder or upper ſide.

Remarks in Forging.

The appearances on forging the cake of wootz, No. 1.

No. 1. One-half of the cake was heated ſlowly by an annealing heat to a deep red, and put under a ſharp broad-mouthed chiſſel with a ſmall degree of taper. It cut with difficulty, was reheated, and cracked a little towards one end of the ſlit or cut originally in the cake.

The heat in this trial was ſo moderate, that I was afraid that the crack had ariſen from a want of tenacity, occaſioned by the heat being too low.

The other half was heated a few ſhades higher, and ſubjected to the ſame mode of cutting; before the chiſſel had half way reached the bottom, the piece parted in two in the direction of the depreſſion made by the cutting inſtrument. The additional heat in this inſtance proved an injury, while the cracking of the ſteel in both caſes, particularly the former, was a certain proof of the abundance, or rather of the exceſs of the ſteely principle.

The fractures of both half cakes, now obtained for a ſecond time, were materially different from that obtained by the ſimple diviſion of the cake. The grain was nearly uniform, diſtinctly marked, but of too gray a colour for ſerviceable ſteel. Two of the quarters being drawn into neat bars under hand hammers

The appearances on forging the cake of wootz, No. 1.

mers at a low heat, one of them contained a number of cracks and fiſſures. The fracture was gray, tore out a little in breaking, but was otherwiſe yolky and exceſſively denſe. A ſmall bar of penknife ſize was improved greatly in drawing down, and had only one crack in thirteen inches of length. The grain and fracture were both highly improved by this additional labour; the tenacity of the ſteel was greater, and it ſtood firmly under the hammer at a bright red heat.

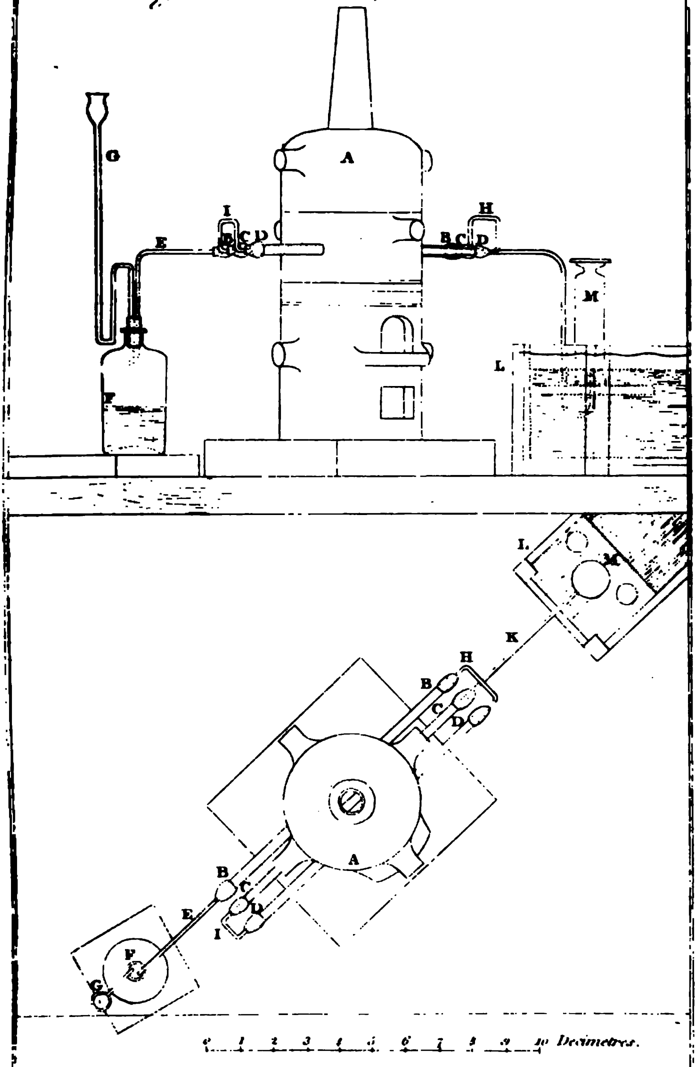
The other two quarters of this cake were ſquared a little, and ſucceſſively put under a tilt hammer, of two hundred weight, going at the rate of three hundred blows per minute, and drawn into ſmall penknife ſize. One of the bars from an outſide piece, always the moſt ſolid, was entirely free from cracks, and had only one ſmall ſcale running upon one ſide.

Theſe bars exhibited a tougher break, than thoſe drawn by hand; the colour was whiter, and the grain poſſeſſed a more regular and ſilky appearance.

(To be continued.)

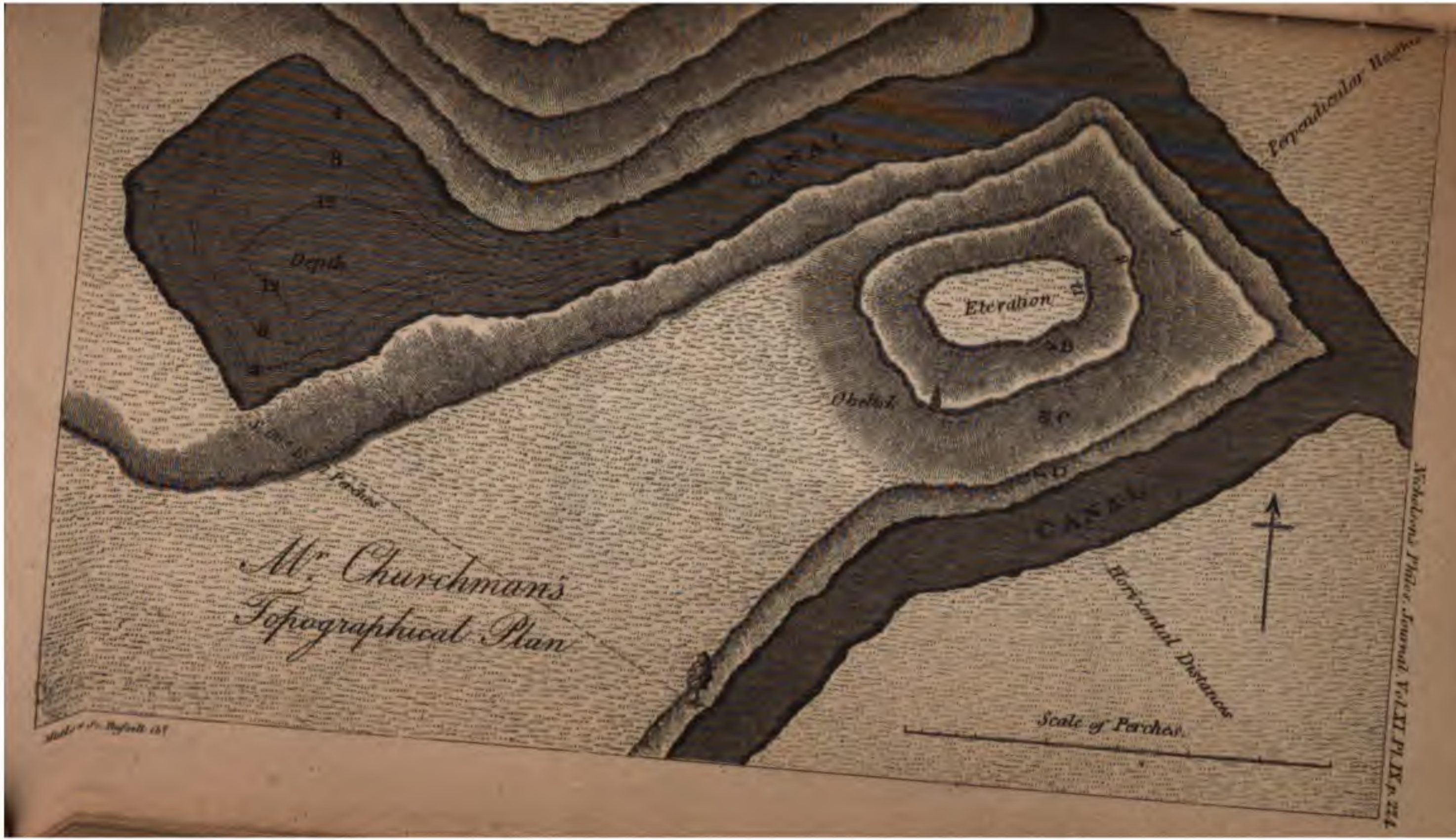
⁂ *I have received a letter from Mr.* Boswell, *in which he expreſſes an opinion, that it is unfair in the Old Correſpondent whoſe letter appeared in our laſt Number to have applied Mr. B's apology to one particular part of his paper; as he conceives, that it ought to have been conſidered as indicating the ſpirit in which his whole communication was written. I have inſerted this ſhort notice out of reſpect to the writer; but have declined inſerting the letter itſelf, becauſe the controverſy can have no farther importance to the readers of this Journal, after the ſubject itſelf has been exhauſted.*

Apparatus of Barruel for making the Gaseous Oxide of Carbon.



Mutlow sc. Russell St.

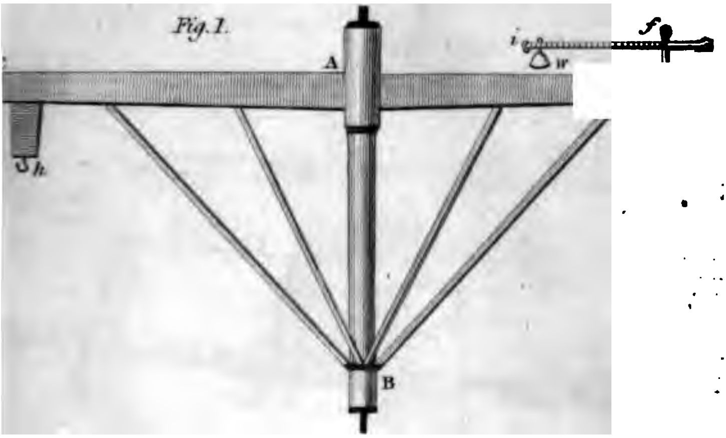
Nicholson's Philos. Journal. Vol. XI. Pl. IX. p. 224.



Mr. Churchman's Topographical Plan

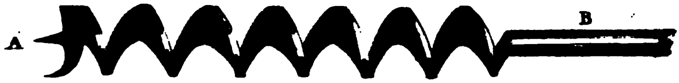
Gregory's Contrivance to determine Horse-power in M

Fig. 1.



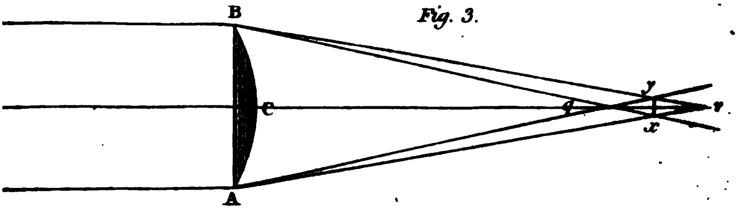
American Borer.

Fig. 2.

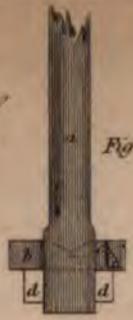


Mr. Ez. Walker on Light.

Fig. 3.



Mr. Seppings Method of suspending Ships.



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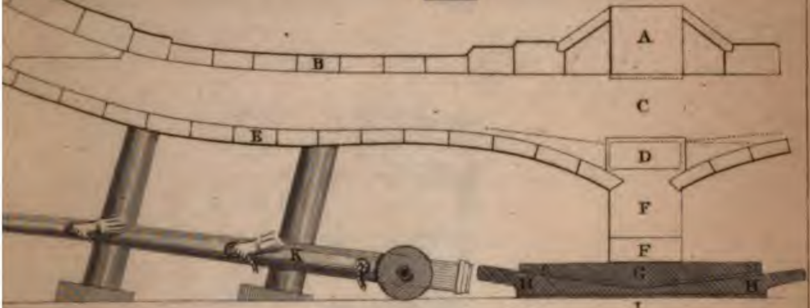

Fig. 2.

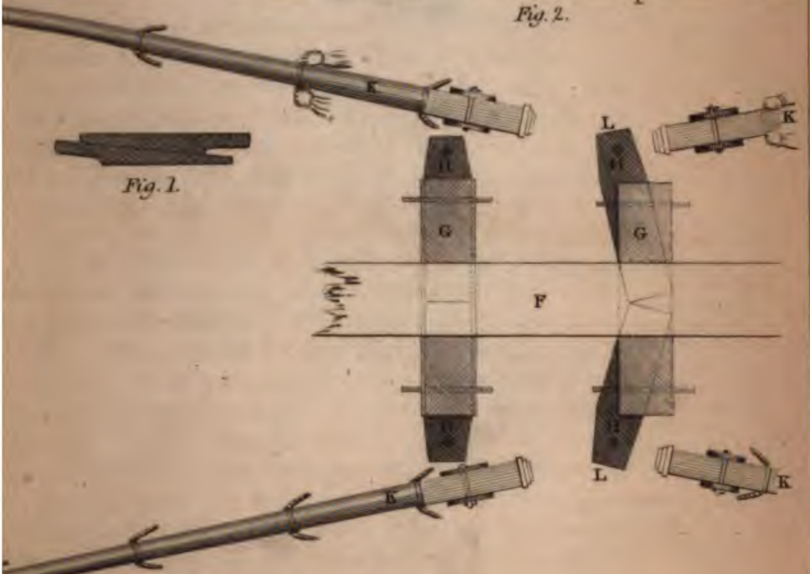

Fig. 1.

Scale 3/4 of an Inch to a Foot.

Mutlow Sc. Russel Ct.

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NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

AUGUST, 1805.

ARTICLE I.

An Account of ſome new Experiments which prove that the Temperature at which the Denſity of Water is a Maximum, is ſeveral Degrees of the Thermometer above the freezing Point. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c. Received July* 16, 1805, *from the Author; with a Letter dated Munich,* 25*th June,* 1805.

IN my ſeventh eſſay, in which I have treated of the propagation of heat in fluids, and alſo in a paper publiſhed in the Philoſophical Tranſactions for the Year 1804, Part I.; in which I have given an account of a curious phenomenon frequently obſerved on the Glaciers of Chamouny, I have aſcribed the melting of ice which is placed (by deſign, or by accident) below the ſurface of ice-cold water, to currents of warmer water, which, in certain caſes, are ſuppoſed to deſcend in that ice-cold liquid: but as this ſuppoſed fact has lately been called in queſtion by ſeveral perſons, and as the explanations I have founded on it muſt fall to the ground, unleſs it can be ſupported, I have been induced to re-conſider the matter, and to give it a careful and thorough inveſtigation.

Phenomena which have been explained from the maximum denſity of water being at higher than freezing temperature:

the fact queſtioned.

It was firſt announced by De Luc.

The fundamental fact on which the ſuppoſition is grounded, which was announced many years ago by Mr. De Luc; namely, that the temperature at which the denſity of water is a maximum, is conſiderably higher than that at which that fluid freezes, is indeed ſo very extraordinary, and appears to be the cauſe of ſo many intereſting phenomena, that too much pains cannot be taken to put it beyond doubt.

New method of proving it.

As the methods hitherto uſed for determining that important point have, by ſome at leaſt, been conſidered as inſufficient, I ſhall take the liberty to propoſe another, by which the fact in queſtion may, I think, be demonſtrated directly; and without any nice calculations, or any very difficult or delicate experiments.

Let the following experiments, (which it will be eaſy to repeat) ſpeak for themſelves.

Apparatus. In the middle of a thin cylindrical braſs veſſel, a thin braſs cup is ſupported.

Having provided a cylindrical veſſel, (A Fig. 1. Plate XII.) open above, made of thin ſheet braſs, which is 5½ inches in diameter, and four inches deep, ſupported on three ſtrong legs, 1¼ inches high; I placed in it a thin braſs cup, (B) two inches in diameter at its bottom, (which is a little convex downwards) 2 $\frac{8}{10}$ inches wide at its brim; and 1 $\frac{3}{10}$ inches deep; which cup ſtands on three ſpreading legs made of ſtrong braſs wire, and of ſuch form and length, that when the cup is introduced into the cylindrical veſſel, it remains firmly fixed in the axis of it, and in ſuch a ſituation, that the bottom of the cup is elevated juſt 1¼ inches above the bottom of the cylindrical veſſel.

In the middle of the braſs cup is ſupported

In the middle of this cup there ſtands a vertical tube of thin ſheet braſs, ½ an inch in diameter, and $\frac{6}{10}$ of an inch in length, open above, which ſerves as a ſupport for another ſmaller cup (C) which is made of cork; the brim of which is on the ſame horizontal level with the brim of the larger braſs cup, in which it is placed.

an hemiſpherical cup of cork,

This cork cup, which is ſpherical, (being ſomething leſs than half of an hollow ſphere) is one inch in diameter at its brim, meaſured within $\frac{4}{10}$ of an inch deep, and ¼ of an inch in thickneſs. It is firmly attached to the vertical tube on which it ſtands, by means of a cylindrical foot, ½ an inch in diameter, and ¼ of an inch high; which, when ſome force is employed, enters into the opening of the vertical tube.

having in its center the bulb of

On one ſide of this cork cup there is a ſmall opening, which receives, and in which is confined the lower extremity of the tube of a ſmall mercurial thermometer, (D). The bulb of this thermometer, which is ſpherical, is juſt $\frac{3}{10}$ of an inch in diameter, and it is ſo fixed in the middle of the cup, that its centre is juſt $\frac{1}{4}$ of an inch above the bottom of the cup; conſequently it does not touch the cup any where, nor does any part of it project above the level of its brim.

a thermometer.

The tube of this thermometer, which is ſix inches in length, has an elbow near its lower end, at the diſtance of one inch from its bulb, which elbow forms an angle of about 110 degrees, and the thermometer is ſo fixed in the cup, that the ſhort branch of its tube, namely, that to the end of which the bulb is attached, lies in an horizontal poſition, while the longer branch (to which a ſcale, made of ivory, and graduated according to Fahrenheit is affixed,) projects obliquely upwards and outwards, in ſuch a manner that the freezing point of the ſcale lies juſt above the level of the top of the cylindrical veſſel in which the cups are placed.

Other particulars.

The cork cup, which was turned in the lathe, is neatly formed, and in order to cloſe the pores of the cork, it was covered, within and without, with a thin coating of melted wax; which was poliſhed after the wax was cold.

The thermometer was fixed to the cork cup by means of wax, and in doing this, care was taken to preſerve the regular form of the cup, both within and without.

The vertical braſs tube which ſupports this cup in the axis of the braſs cup, is pierced with many holes, in order to allow a free paſſage into it, and through its ſides, to the water employed in the experiments.

This apparatus was placed in a pan, and ſurrounded with ice.

Having attached about ſix ounces of lead to each of the legs of the braſs cup, in order to render it the more ſteady in its place, it was now introduced with its contents, into the cylindrical veſſel, and the veſſel was placed in an earthen baſin (E) ſeven inches in diameter below, 11 inches in diameter at its brim, and five inches deep, and was ſurrounded on all ſides with pounded ice.

Ice was placed and ſecured in the cylindrical veſſel; but not in the cups, and ice-cold water was poured

Several flat cakes of ſolid ice were now put into the cylindrical veſſel, and faſtened down upon its bottom, and under the bottom of the braſs cup, and a circular row of other long pieces of ice were placed, in a vertical poſition, round the into all the remaining ſpace.

outſide of the brim of the braſs cup, between it and the vertical ſide of the cylindrical veſſel, which vertical pieces of ice reached upwards to within about $\frac{1}{10}$ of an inch of the top or brim of the cylindrical veſſel; and when this was done, ice-cold water was poured into this veſſel till the ſurface of that cold fluid ſtood juſt one inch above the level of the brim of the cork cup.—Both cups were of courſe ſubmerged and filled with ice-cold water, and ſurrounded on all ſides by ſolid cakes of ice.

The cups remained thus ſubmerged for one hour.

After things had remained in this ſituation more than an hour, during which time the cold water in the cylindrical veſſel, and that in the cups, was frequently agitated with the ſoft end of a ſtrong feather, and the cups and the water in every part of the veſſel appearing to be exactly at the temperature of freezing: I proceeded to make the following deciſive experiment.

Experiment. No. 1.

Experiment I. A cone of metal at 42° was plunged in the ice-cold water, juſt above the cork cup:

A ſolid ball (F) of tin having been provided, two inches in diameter, with a cylindrical projection on the lower ſide of it, one inch in diameter, and half an inch long, ending in a conical point, which projected (downwards) half an inch farther; this ball (to which was fixed a ſtrong iron wire ſix inches in length, which ſerved as a handle to it,) having been made to acquire the temperature of 42° F. by keeping it immerſed near half an hour in a large quantity of water at that temperature, was placed as expeditiouſly as poſſible over the middle of the cork cup, and in ſuch a ſituation that the whole of the deſcending conical point (half an inch in length) was immerſed in the ice-cold water in the cylindrical veſſel, the extremity of that point being juſt half an inch perpendicular above the upper ſide of the bulb of the thermometer which lay in the cork cup.

It was foreſeen, that if the water in contact with the cone became denſer by the heat, it would deſcend and raiſe the thermometer.

I knew that the particles of ice-cold water which were thus brought into contact with the conical point, could not fail to acquire ſome ſmall degree of heat from that relatively warm metal; and I concluded, that if the particles of water ſo warmed ſhould in fact become *heavier* than they were before, in conſequence of this ſmall increaſe of temperature, they muſt neceſſarily *deſcend* in the ſurrounding lighter ice-cold liquid, and as the heated metallic point was placed directly over the cork cup, and fixed immoveably in that ſituation, I foreſaw that the deſcending current of warm water muſt neceſſarily fall into that cup, and at length fill it, and that the preſence of this warm water in the cup would be announced by the riſing of the thermometer.

The reſult of this very intereſting experiment was juſt what I expected: the conical metallic point had not been in contact with the ice-cold water more than 20 ſeconds, when the mercury in the thermometer began to riſe, and in three minutes it had riſen three degrees and a half, namely, from 32° to $35\frac{1}{2}$°, when five minutes had elapſed, it had riſen to 36°, when an end was put to the experiment.

It did, in fact, raiſe the thermometer in the cup;

Another ſmall thermometer, placed juſt below the ſurface of the ice-cold water, and only $\frac{2}{10}$ of an inch from the upper part of the conical point, on one ſide of it, did not appear to be ſenſibly affected by the vicinity of that warm body.

but it did not affect a thermometer near the ſurface of the water;

A third thermometer, the bulb of which was placed in the braſs cup, juſt on the outſide of the cork cup, and on a level with its brim, ſhewed that the water which immediately ſurrounded the cork cup, remained conſtantly at the temperature of freezing, during the whole time that the experiment laſted.

nor another near the outſide of the cup.

As I well knew, from the reſults of the experiments on the propagation of heat in a ſolid bar of metal, of which an account has been given in a memoir preſented to the firſt claſs of the National Inſtitute of France, on the 7th of May 1804, that the ice-cold water in this experiment could not poſſibly acquire from a contact with the conical metallic point, a temperature ſo high as that of 42°, I was by no means ſurpriſed to find that the thermometer belonging to the cork cup roſe no higher.

The metal at 42° could not the water to the ſame temp.

In order to ſee if it could not be made to riſe not only higher, but alſo more rapidly, by employing the metallic ball heated to ſuch a temperature as it might by ſuppoſed would be ſufficient to heat thoſe particles of ice-cold water which ſhould come into contact with its conical point, to the temperature at which the denſity of water is ſuppoſed to be a maximum, I made the following experiments.

A ſomewhat greater heat propoſed to be applied.

Experiment. No. 2.

Having removed the ball, I gently bruſhed away the warm water, which, in the laſt experiment had been lodged in the

Experiment 2.
The former experiment was

cavity

repeated; the cone being heated to 60°.

cavity of the cork cup, (and which ſtill remained there, as was evident from the indication of the thermometer belonging to the cup,) I placed ſeveral ſmall cakes of ice in the cylindrical veſſel, which ice floating on the ſurface of the water in the veſſel, prevented that water from receiving heat from the air of the atmoſphere, which at that time was at the temperature of 76° F. And as the cork cup had been a little heated by the warm water in the foregoing experiment, time was now given it to cool.

As ſoon as the cup, and the whole maſs of the water in the cylindrical veſſel appeared to have acquired the temperature of freezing, I carefully removed the cakes of ice which floated on the ſurface of the water, and introduced once more the projecting conical point belonging to the metallic ball into the ice-cold water in the veſſel, placing it exactly in the ſame place which it had occupied in the foregoing experiment; but this ball, inſtead of being at the temperature of 42° F. as before, was now at the temperature of 60° F.

The effect was more rapid and more conſiderable.

The reſult of this experiment was very ſtriking, and if I am not much miſtaken, affords a direct, unexceptionable, and demonſtrative proof, not only that the maximum of the denſity of water is in fact at a temperature which is ſeveral degrees above the point of freezing; but alſo, that warm currents do actually ſet downwards in ice-cold water, whenever a certain ſmall degree of heat is applied to the particles of that fluid which are at its ſurface.

Particulars.

The conical metallic point had been in its place no more than ten ſeconds when I diſtinctly ſaw that the mercury in the thermometer belonging to the cork cup was in motion; and, when 50 ſeconds had elapſed, it has riſen four degrees, viz. from 32° to 36°.

When two minutes and a half had elapſed, (reckoning from the moment when the metallic point was introduced into the cold water,) the thermometer had riſen to 39°, and at the end of ſix minutes to $39\frac{7}{8}$°, when it began to fall; but very ſlowly however, for at the end of eight minutes and a half it was at $39\frac{3}{4}$°.

A thermometer near the outſide of the cup was not affected.

A ſmall mercurial thermometer, the bulb of which was placed on one ſide of the cork cup, at the diſtance of about $\frac{2}{10}$ of an inch from it, ſhewed no ſigns of being in the leaſt affected by the heat communicated to the ice-cold water by the metallic ball.

This experiment was repeated three times the ſame day, (the 13th of June 1805,) and always with nearly the ſame reſults.

The mean reſults of theſe four experiments were as follows:

Tabulated reſults of the experiments repeated, with the the cone at a low heat.

Time elapſed reckoned from the beginning of the experiment.				Temperature of the water in the cork cup, as ſhewn by the thermometer.
	Min.	Sec.		
	0	0		32°
At	0	10	began to riſe	32+
At	0	23	had riſen to	33
	0	28		34
	0	35		35
	0	48		36
	1	3		37
	1	35		38
	2	32		39
	3	41		$39\frac{1}{2}$
	4	48		$39\frac{3}{4}$
	6	5		$39\frac{7}{8}$

As I had found by ſome of my experiments made in the year 1797, of which an account is given in my ſeventh eſſay, part I. that water at the temperature of about 42° F. and conſequently what we ſhould call very cold, melted conſiderably more ice, when ſtanding on it, than an equal quantity of boiling hot water, in the ſame ſituation, I was very curious to ſee whether the thermometer, the bulb of which lay in the cork cup, would not alſo be leſs heated by the ball when it ſhould be applied *very hot,* to the ſurface of the water, than when its temperature was much lower.

Whether a much greater heat in the metal would affect the water in the cup leſs.

To determine that point I made the following experiment.

Experiment. No. 3.

The cylindrical veſſel with its contents having been once more reduced to the uniform temperature of freezing water, the metallic ball was heated in boiling water, and being as expeditiouſly as poſſible taken out of that hot liquid, its projecting conical point was ſuddenly ſubmerged in the ice-cold water, as in the former experiments.

Experiment 3. The experiment repeated with the cone heated to 212°.

The reſult of this experiment was very intereſting, and it ſeems to me to throw much light on the ſubject of theſe inveſtigations.

Effect on the thermometer in the cup much leſs.

It was not till 50 ſeconds had elapſed that the thermometer began to ſhew any ſigns of riſing, and at the end of one minute and ſeven ſeconds it had only riſen two degrees.

In the foregoing experiments, when the metallic ball was ſo much colder, the thermometer began to riſe in ten ſeconds, and at the end of one minute and three ſeconds it had riſen five degrees.

This difference is very remarkable; and if it does not prove the exiſtence, and great efficacy of currents, in conveying heat in fluids, I muſt confeſs that I do not ſee how the exiſtence of any inviſible mechanical operation, the progreſs of which does not immediately fall under the cognizance of our ſenſes, can ever be demonſtrated.

As the experiment made with the ball heated in boiling water appeared to me to be very intereſting, I repeated it twice, and its reſults were always nearly the ſame.

Tabulated reſults of experiments repeated; with the cone at a conſiderable heat.

The mean reſults of theſe three experiments were as follows:

Time elapſed, reckoned from the beginning of the experiment.				Temperature of the water in the cork cup, as ſhewn by the thermometer.
	Min.	Sec.		
	0	0	- - - - - - - -	32°
At	0	50	the thermometer begun to riſe.	32+
At	1	2	had riſen to - - - - -	33
	1	7	- - - - - - - -	34
	1	18	- - - - - - - -	35
	2	2	- - - - - - - -	36
	3	2	- - - - - - - -	36½
	4	17	- - - - - - - -	37
	6	12	- - - - - - - -	38
	7	17	- - - - - - - -	38⅛
	9	0	- - - - - - - -	38¼
	12	0	- - - - - - - -	38¼
	14	0	- - - - - - - -	38¼

Comparative view of the experiments,

By comparing the mean reſults of theſe experiments with the mean reſults of thoſe in which the ball was at the temperature of 60°, we may ſee how much more rapidly the thermometer in the cork cup acquired heat when the metallic ball was *relatively cold* than when it was at the temperature of boiling water; and it is more than probable that it was not till after the conical metallic point had been conſiderably cooled, by a contact with the ice-cold water, that thoſe ſtreams of moderately warmed water began to deſcend from it, by which the thermometer was at length heated.

When the very hot cone was uſed, a thermometer at the ſurface was heated;—but a lower one was not.

In the experiments made with the ball heated in boiling water, a ſmall thermometer, placed juſt under the ſurface of the ice-cold water, on one ſide of the metallic point, began to riſe very rapidly as ſoon as the hot ball was fixed in its place; but another thermometer placed about half an inch lower, on one ſide of the cork cup, remained to all appearance at perfect reſt, from the beginning of the experiment to the end of it.

Explanation:

The explanation of all theſe appearances is ſo eaſy that it would be a waſte of time to ſay much on that ſubject. It may however be uſeful to recapitulate the principal phenomena, and ſhew in what manner they tend to eſtabliſh the facts which they are brought to prove.

That the ſlightly heated water deſcends;

Every body muſt ſee, at the firſt glance, that in all theſe experiments the heat which cauſed the thermometer to riſe was *carried* down into the cork cup by deſcending currents of warm water; and it is evident that water which deſcends muſt of neceſſity be ſpecifically heavier than that in which it deſcends.

—and the greateſt denſity is rather below 40°.

From the reſults of theſe experiments we may conclude that the denſity of water is a maximum when that fluid is at a temperature ſomewhat lower than that of the *fortieth degree* of Fahrenheit's thermometer.

Hotter water is lighter and aſcends.

In all the foregoing experiments, more or leſs warm water deſcended through the ice-cold water into the cork cup; but the reſults of the experiments which were made with the metallic ball heated in boiling water, ſhew evidently that when the particles of water at or near the ſurface of a quieſcent maſs of ice-cold water, are by any means heated to a temperature ſeveral degrees above that of 40° of the ſcale of Fahrenheit, ſuch particles, ſo heated, become ſpecifically lighter than ice-cold water, and conſequently cannot deſcend in that cold and denſer liquid.

With the hot cone ſome of the fluid aſcended and ſome deſcended.

In the experiments in queſtion, (with the hot ball) ſome of the particles of the water, namely, thoſe which came firſt into contact with the conical metallic point, were heated to a higher temperature than that at which they were diſpoſed to ſink in ice-cold water; and theſe roſe and ſpread themſelves over the ſurface of the cold liquid; but others, which happened to acquire leſs heat, deſcended in it, and after filling the cork cup, overflowed, no doubt its brim, and then deſcending to the bottom of the braſs cup, and coming into contact with that ice-cold metal, were there cooled, and there remained at reſt.

Cork a good non-conductor.

As cork is an excellent non-conductor of heat, the warm water accumulated in the cork cup during an experiment retained its heat a long time after the heated ball was removed, notwithſtanding its being ſurrounded on all ſides, and even covered immediately by ice-cold water; (which by the by is a pretty ſtrong proof that water is by no means a good conductor of heat) care however was taken, not only to remove this warm water after each experiment, by bruſhing it away with the ſoft end of a ſtrong feather or quill, but alſo to cool the cup, and reduce it to the temperature of freezing water; which laſt operation was found to be much accelerated by bruſhing it out frequently with the feather. In order that this feather might itſelf be ice-cold, it was ſuffered to remain conſtantly in the ice-cold water, in the cylindrical veſſel.

Cautions.

Apparatus by which the cone was fixed.

I muſt not forget to give an account of the means uſed for fixing the metallic ball in its place. This was done in a very ſimple and effectual manner. A ſlip of ſtrong tin (G H) ſix inches long, and 2½ inches wide, with a circular hole in the middle of it, one inch in diameter, being laid horizontally on the top or brim of the cylindrical veſſel, in ſuch a manner that the center of that circular hole coincided with the axis of the cylindrical veſſel, the ſhort cylindrical projection belonging to the ball being introduced into that hole, the ball was firmly ſupported in its proper place.

The quantity of ice-cold water in the cylindrical veſſel was ſo regulated that the whole of the conical point being ſubmerged, the ſurface of the water was on a level with the lower end of the cylinder, or, which is the ſame thing, with the baſe of the inverted cone.

When not only the whole of the conical point, but a part alſo of the ſhort cylinder were immerſed in the ice-cold water, the warmed water appeared to be thrown into eddies in its deſcent, which diſperſing about prevented its falling regularly in one continued ſtream into the cork cup.

The conical figure preferable for the immerſed metal.

To *conclude*, I would juſt obſerve, that although the foregoing experiments appear to me to be perfectly unexceptionable, and that their reſults afford demonſtrative proof of the facts which they were contrived to eſtabliſh; yet, when attempts are made by experiments ſimilar to theſe, to determine whether heat can be made to paſs *downwards* in water which is at a higher temperature than that at which its denſity is a maximum, difficulties occur which appear to me to be quite inſurmountable.

It is not eaſy to make concluſive experiments at elevated temperatures;

The fluidity of water is ſo perfect, or the mobility of its particles ſo great, that the liquid at the ſurface, which is firſt heated and rarefied, immediately ſpreads far and wide, and meeting with the ſides of the containing veſſel, heats them, and this heat, ſo acquired, making its way downwards (as well as upwards) in the ſolid ſubſtance of which the veſſel is conſtructed, raiſes the temperature of the lower ſtrata of the fluid in contact with it, which moving towards the axis of the veſſel, communicates heat to a thermometer placed there, below the ſurface of the water.

For the ſides of the veſſel greatly modify the effects by conducting the heat, &c.

That theſe various operations do in fact take place, nobody can doubt: and it appears to me to be the more probable that all the heat which a thermometer placed below the ſurface of warm water acquires when a great degree of heat is applied to the particles of that fluid which are at the ſurface, is in fact received from the ſides of the containing veſſel, not only becauſe the thermometer acquires heat ſo very ſlowly, but alſo, and more eſpecially, becauſe this heat is acquired much more ſlowly when the containing veſſel is wide than when it is narrow; and alſo when it is made of a ſubſtance which is a good conductor of heat than when it is conſtructed of a ſubſtance which is a bad conductor of heat, as I have found by experiment. But as this particular enquiry is foreign to my preſent purpoſe, I ſhall not enlarge on it in this place.

Facts which ſhew the probability that conſiderable heats are chiefly tranſmitted downwards through fluids by the agency of the veſſel.

Deſcription

II.

Deſcription of a Preſs for preſerving botanical Subjects; with an Account of the Succeſs of the Improvement in the Art of Blaſting pointed out by Mr. JESSOP *in this Journal. In a Letter from Mr.* THO. HARRISON.

To Mr. NICHOLSON.

SIR,

Introductory obſ. on preſerving plants.

BEING deſirous to make a collection of the plants which grow in the neighbourhood of Kendal, and to preſerve them in an herbarium, I was naturally led to a conſideration of the different methods, which have been uſed by botaniſts, of drying and preſerving ſpecimens; but finding none ſo complete and expeditious as I could wiſh, I therefore adopted a plan of my own, which I am perſuaded will be found to anſwer every purpoſe that the botaniſt can require. The real utility which is to be derived by a young botaniſt from the preſervation of plants, is the power of future examination, and to anſwer the purpoſe the natural appearance of each ſpecimen ought to be preſerved as calculated to produce this effect; for ſince the ſeed-veſſels and ſtems of plants occupy much more ſpace in thickneſs than the leaves, in order to preſerve the latter from ſhrivelling, the two former muſt generally be bruiſed in ſuch a manner as totally to preclude any accurate inveſtigation afterwards: and this on trial is found to be the caſe: beſides, all the plants that happen to be in the preſs at the ſame time, however various their texture, are ſubjected to the ſame degree of preſſure.

Remarks on the methods now in uſe.

The plan recommended by T. Velley, Eſq. ſeems equally exceptionable; I mean the method of placing the plant when freſh between ſeveral ſheets of blotting-paper, and ironing it with a large ſmooth heater pretty ſtrongly warmed, till all the moiſture is diſſipated. By this method it is evident that the parts of fructification muſt be much bruiſed: it may preſerve the colour of the bloſſoms better than any other, but this in the ſcience of botany is not very eſſential, at leaſt if it was, I believe that no method hitherto diſcovered will ſucceed univerſally in arreſting the fading beauty of delicate flowers. The next plan which is given by Dr. Withering, is one uſed by a Mr. Whateley a ſurgeon, but his contrivance, though certainly better adapted to preſerve the parts of fructification and the ſhape of the ſtem entire than the two former, I found would take up much more time than a country ſurgeon can well ſpare; therefore this led me to make what I thought an improvement on Mr. Whateley's plan, and the experienced botaniſt is left to judge of my ſucceſs from the following deſcription; this I can ſay for it, that it anſwers completely (of courſe) to my own wiſh; but further, that Mr. John Gough, of Middleſhaw, who has the beſt collection of plants in this county, regrets that he had not the ſame contrivance, and has urged me much to ſend you the following deſcription of my preſs.

This inſtrument conſiſts of 17 oblong boxes, each, excepting the uppermoſt and lowermoſt, is made of four ſides of well-ſeaſoned oak wood, two inches deep and one-fourth of an inch thick, dove-tailed together; the two end-pieces of which have two notches each in the middle wherein to place the fingers in lifting it; and the bottom conſiſts of canvas glued and nailed to the wooden frame: in each corner is fixed a ſmall triangular piece of wood reaching half an inch from the bottom upwards (for a purpoſe hereafter to be explained). Theſe boxes are made ſo as to be placed one within another ſucceſſively upwards, the loweſt (I am ſpeaking only of the 15 with canvas bottoms) meaſures 20 inches by 16 on the outſide, and the higheſt 12 inches by 8. The bottomeſt, or carriage on which all the others are ſupported, is much ſtronger, the ſides of it being two inches deep and three quarters of an inch thick: its bottom is of wood of the ſame thickneſs, all over perforated with holes one-third of an inch in diameter, and reſts on four iron caſtors, one at each corner, to render the whole more eaſily moveable; at each end of it there is an iron handle: This box is 20 inches by 16 within, and will therefore barely receive the largeſt of the boxes with canvas bottoms. The uppermoſt, or 17th box, is of the ſame conſtruction as the canvas boxes, excepting that its bottom is of wood of the ſame thickneſs as its ſides, with a number of holes pierced through of the ſame magnitude as thoſe in the carriage, and canvas glued to the under ſide of it. To complete this preſs, two folds of blotting-paper are to be placed

The author's new preſs deſcribed. It conſiſts of a ſeries of trays or boxes, having (all but the loweſt) canvas bottoms. They contain ſand, and the plants are laid between box and box.

placed over the holes in the bottom of the carriage, in which, as well as all the canvas-boxes, muſt be put an inch in depth of the fineſt ſand, waſhed and ſifted; river ſand is moſt eligible, as being leaſt angular; which ſand muſt be made level on the top by drawing over it a rule that reaches one inch into each box: theſe boxes are placed one within another, and each by containing an inch depth of ſand, raiſes the incumbent frame one inch, ſo that when they are thus charged and placed one upon another, the whole makes a truncated pyramid of 18 inches in height, and weighing about 1 cwt. The uppermoſt box may be filled with ſand, or, if more weight be neceſſary, with ſhot. To this box I have added (what is by no means eſſential) a cap, conſiſting of the ſides of three boxes fixed one within another, each projecting one inch above its inferior, for the ſake of uniformity; the top is covered by a thin board: when this cap is placed upon the uppermoſt box, the whole has not an inelegant appearance.

Reference to the engraving.

Leſt the deſcription ſhould not be ſufficiently clear, I have ſketched the following figure, repreſenting a perpendicular ſection of the preſs viewed on one ſide of it. *Plate XV.*

Fig. 1. repreſents a perpendicular ſection of the preſs made lengthwiſe.

a. The end of the bottomeſt box or carriage.

b. Its bottom part perforated with holes.

c. The end of the firſt box with a canvas bottom.

d. The canvas bottom.

e. The ſand which but half fills the box.

f. The 17th or uppermoſt box, with a perforated wooden bottom.

g g g. Repreſent the end of the cap formed of the frames of three boxes, in the empty ſpace of which may be placed the botaniſt's pocket box.

Fig. 2. repreſents the rule wherewith the ſand is made level in the boxes; it is 18 inches long.

h. A notch one inch deep.

i. The edge made thin.

k. A notch an inch long.

Fig. 3. repreſents the end of a canvas box.

l l. The two notches to place the fingers in when the box is to be lifted.

The Method of uſing it.

Method of uſing it.

The plants are laid between blotting-paper, and the preſſure regulated by the number of boxes above each, &c.

After a ſpecimen is ſelected, a ſlip of paper is to be attached to it, containing its name, place of growth, and time of gathering. The boxes are then to be taken off by three or four at a time, until the number removed may be judged to be of ſufficient weight to produce the neceſſary preſſure. On the ſand in the box thus ſelected for uſe, a ſingle fold of blotting-paper muſt be placed; upon that the plant, taking care to preſerve as much as poſſible the natural poſition of its characteriſtic parts: Over the plant a ſecond fold of paper muſt be laid, and then the boxes are to be replaced. If the plant be woody and require much preſſure, place it near the bottom; but if ſucculent, near the top; and herein conſiſts one great advantage of this preſs over every other, ſince the preſſure can be ſelected or varied at pleaſure, and all the parts of the plant preſſed equally; for if the plant be ſucculent, the preſſure can ſcarcely be too ſlight at firſt, and may be gradually increaſed without ever bruiſing the tendereſt part; and if, on the other hand, ſtrong preſſure be required, the plant may be placed in the bottomeſt box; and if the weight of ſand be not thought ſufficient, any number of the boxes next above it may be loaden with ſmall ſhot, by which the preſſure may be increaſed until the boxes be full, when the preſſure exerted would be much greater than the ſtrongeſt plants could bear without having ſome of its eſſential parts defaced. Another advantage which led me to give a preference to this method of preſſing plants, was the great ſaving of time; for though my profeſſional avocations ſhould require my immediate preſence at a diſtance, and thus drag me from this innocent amuſement when in the buſieſt part of claſſing and arranging my ſpecimens, and though my preſs ſhould be completely ſeparated into its conſtituent parts, yet the whole can be arranged, in the ſpace of one minute, to remain until the next leiſure time that may occur; ſo that, in fact, with this contrivance, botany is a ſtudy that can be entered into or given up as it may ſuit the convenience of a moment; which is not the caſe where any of the plans deſcribed in Dr. Withering's Introduction to the Study of Botany * are followed.

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* Third Edition.

Remarks on the effects and advantages of this contrivance.

It may be neceſſary to explain ſome parts of the ſtructure of the preſs. The holes in the bottomeſt and uppermoſt boxes are to admit of the circulation of air and the evaporation of moiſture, which will be conſiderable when the preſs without its cap is placed within the influence of the ſun or fire, as it always ought to be: and the ſmall triangular pieces of wood in the corners of each box, are to prevent the upper boxes from preſſing ſolely on the canvas bottom of the loweſt box, when any number are lifted together: theſe may at firſt ſight appear likely to impede the preſſure of the ſand when placed in the preſs, but this objection will vaniſh when it is recollected that each box contains an inch depth of ſand: and it is evident, on examination of the preſs, that the ſides of the boxes ſerve no other purpoſe than to keep the ſand together; for they are perceptibly moveable in a perpendicular direction, and ſuſtain no preſſure from the ſuperior boxes; therefore the plant is placed as if in a heap of ſand.

The beſt method of preſerving dried plants is to ſtitch them to a paper.

I am afraid I have already treſpaſſed too long on the attention of the reader, but I cannot diſmiſs the ſubject without offering a few obſervations on the manner of preſerving plants after they are dried. There have been at leaſt three methods uſed: firſt, fixing them to paper by the aid of gum-water: ſecond, placing them looſely in a book: third, ſtitching them with thread to a ſheet of paper. The firſt plan appears to me the leaſt eligible, for it cannot be neatly executed unleſs the plant be preſſed nearly flat, and by fixing it to the paper it can never admit of an accurate examination in future, but becomes a mere picture, and a very imperfect one; for almoſt every diſtinguiſhing character, eſpecially the parts of fructification, are injured or defaced; even the general habit is often ſo much altered that the plant could not be aſcertained, were it not for the name generally ſubſcribed at its root. The ſecond plan is alſo objectionable; for though the plant may at any time be ſubjected to examination, yet by lying looſe in a book, it is very apt to be broken by the leaſt motion, particularly when kept dry, which all preſerved plants ought to be. The third appears to me the leaſt liable to objection; for if the plant be neatly ſtitched down, it will not be ſubject to ſuch motion as to injure its parts; and by cutting the threads it may be detached from the paper, and ſubjected to any future inveſtigation.

I am, Sir,
Your's, &c.
Kendal, July 8, 1805. THOMAS HARRISON.

Account of the ſucceſsful application of the method of blaſting rocks indicated in this Journal by W. Jeſſop, Eſq.

P. S. I cannot refrain from calling to the recollection of your readers a valuable paper in the 9th Vol. p. 230, of your Journal, recommending the uſe of ſand in the blaſting of rocks. The effects there related of this ſimple agent, I confeſs did aſtoniſh me more than any thing I ever read: and as very ſerious and even fatal accidents have happened by the method of ramming blaſts with ſtone in the limeſtone and ſlate quarries in this neighbourhood, I was determined to repeat the experiment the firſt opportunity, and thus ſhew its effects to the men who work in thoſe places. Accordingly no long period elapſed before I had occaſion to be in the neighbourhood of the ſlate quarries in Longſliddale, about ten miles from Kendal; when I went to one of thoſe quarries, taking with me a bag of ſmall ſand, conſiſting of powdered freeſtone uſed in this country to ſcour pans with, and ſtrew upon the ſtone floors. I mentioned my buſineſs to one of the workmen who was then engaged in boring a hole 24 inches deep and about an inch diameter in a ſlate rock; the ſtratum was about 27 inches thick, and reclined from the perpendicular about 20 degrees; and this being the firſt blaſt, it was conſequently firmly ſurrounded and fixed in on all ſides by ſolid rock: The part that he wanted to throw out by the blaſt was ſuppoſed to be five ton weight: The direction of the hole which he was boring was perpendicular to the ſtratum, and therefore elevated above the horizon about 20 degrees. He ſmiled at my propoſal, but ſaid he would try the experiment to ſatisfy me, provided I would pay for the powder if it failed; but he ſeemed to think that I had a very poor idea of blaſting, to believe that a little light ſand would anſwer the purpoſe of the laborious and dangerous proceſs of ramming which he had been accuſtomed to uſe. However, he charged his blaſt with powder in the manner he had been uſed to do; *i. e.* into this hole

Safe and eaſy method of blaſting rocks.

of 24 inches in depth, he put as much powder, together with two or three pieces of broken ſticks, of half an inch diameter and about four or five inches long, as reached 12 inches upwards: The ſticks he introduced for the purpoſe of keeping the powder looſe in the hole, ſo that it might take fire more nearly all at the ſame inſtant: This done, he put in a charged ſtraw, into the outer end of which was fixed a match-paper; and laſtly, he (with a ſmirking countenance, anticipating the joy he expected to feel at my diſappointment) filled up the hole with ſand. The match was then fired, upon which we removed to a diſtance, and the powder ſoon exploded; when he immediately exclaimed, from the peculiarity of the report, though the effect was unſeen by him, that the rock was as completely ſhattered as if the hole had been rammed with ſtone; and a view of the place verified the prediction. Since that time the plan has been adopted in that and moſt of the neighbouring quarries, with the uſe of ſmall ſand which is waſhed out of the river in floods, and the method anſwers as well as the old one which was attended with ſo much danger. In one caſe the ſand was thrown out twice without any effect on the rock; the third time, the hole was rammed in the uſual manner with ſtone, which was thrown out likewiſe; ſo that the new proceſs fortunately loſt no reputation.

I have detailed thus much in confirmation of the valuable paper before mentioned, and in hopes that it may induce ſome of your readers who reſide in the neighbourhood of mines or quarries, to endeavour to introduce this ſafe and eaſy method in lieu of the dangerous and tedious one of ramming with ſtone, in which proceſs we have had repeated inſtances of the loſs of limb, viſion, and even life itſelf: In the uſe of ſand there is comparatively no danger, and by adopting this plan many a valuable life will be ſaved.

III.

Deſcription of a Safety Valve, containing a Vacuum Valve in the ſame Hole of the Boiler. By Sir A. N. Edelcrantz.*

Large boilers require a ſafety valve and a vacuum valve.

IN large boilers or coppers, where boiling fluids are encloſed, a *ſafety valve* is generally uſed to prevent their burſting, from an unexpected exceſſive force of the elaſtic ſteam, and, beſides, a *vacuum valve*, to prevent their being compreſſed or cruſhed by the weight of external air, in the caſe of a ſudden condenſation of the vapours. Theſe two valves are commonly fitted in two different holes in the boiler; but as a more ſimple, and conſequently more eligible, method ſeems to be that of joining them together, I take the liberty to ſubmit to the Society for the Encouragement of Arts, &c. the following contrivance for that purpoſe:—

Deſcription of a valve that performs both offices.

a b, *Plate XII. Fig.* 2. is a common conical ſafety valve, fixed in the boiler *c d*, having four openings, *i i*, which are repreſented in a plan view in *Fig* 3: *e f* is the metallic rod, bearing the weight K K, with which the ſafety valve is loaded, and extending itſelf under that valve to *f*: *g h* is the *vacuum valve* conſiſting in a plane circular plate, with a braſs tube ſliding along the rod, and preſſed by a ſpiral ſpring to the ſafety valve *a b* (againſt which it has been well ground in making it), cloſing in that ſituation the openings *i i*.

It is a loaded valve opening upwards; having in its plate a ſpring-valve opening downwards.

Such being the conſtruction of the whole, it is evident, that when the elaſticity of the ſteam increaſes, the two valves, joined together, with the holes *i i* ſhut, make but one, oppoſing to the elaſticity of the ſteam an united reſiſtance, which is regulated by the weight *k k*, in the common way; but, on the contrary, when by condenſation of the vapours a vacuum is produced, the external air in preſſing through *i i*, upon the vacuum valve *g h*, forces it down, and opens to itſelf a paſſage into the boiler.

The valve *g h* may eaſily be made conical, like the other, if that form ſhould be preferred; but in different trials, I have found planes, if well turned and ground together, join as perfectly as can be deſired, being preſſed by the united elaſticity of the ſpring and the ſteam.

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* From the Memoirs of the Society of Arts for 1804. The ſilver medal was awarded for this invention.

The ſame applied to a former contrivance.

Fig. 4. is the ſame contrivance adapted to a new kind of *ſafety valve* or *piſton*, which, though I originally intended it for the uſe of Papin's digeſters of a new conſtruction *, has been, in a larger ſize, applied by me even to ſteam engines, and is deſcribed in the Philoſophical Magazine of December, 1803.†

Other experiments.

I have lately begun, and ſhall purſue, a ſet of experiments, with the intention of regulating by this ſafety piſton, the quantity of admitted air to fire-grates, and to effect, by that means, a new mode of regulating the fire, and the elaſticity of ſteam in boilers, with leſs expenditure of fuel and of force than uſual; of which idea a hint is given in the work and place above mentioned. The reſult of theſe reſearches I ſhall at ſome future period do myſelf the honour of communicating to the Society.

IV.

Obſervations and Experiments for the Purpoſe of aſcertaining the definite Characters of the primary Animal Fluids, and to indicate their preſence by accurate chemical Teſts. By JOHN BOSTOCK, *M. D.* *Communicated by the Author.*

Great want of preciſion in the animal analyſis; particularly of the fluids.

THE preciſion which the analyſis of mineral and vegetable ſubſtances has attained, does not appear to be yet extended to the products of the animal kingdom. This remark may be applied both to the ſolids and fluids which compoſe the animal body, but it is the moſt applicable to the latter claſs of ſubſtances. The terms ſerous, mucilaginous, gelatinous, &c. are employed, even by the moſt eſteemed medical and phyſiological writers *, in a vague and indeterminate manner, without attending either to the original import of the word, or to the reſtricted meaning, which it is neceſſary to impoſe upon popular expreſſions when they are adopted in ſcientific reſearches. The object of the preſent paper is to aſcertain a definite character for what I propoſe to call the primary animal fluids, and to diſcover delicate and accurate teſts, by which their preſence may be eaſily and certainly indicated. By primary animal fluids, † I mean thoſe into which the compound fluids exiſting in the animal body are capable of being reſolved by the application of different re-agents, without decompoſing them into their ultimate elements.

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* Nicholſon's Journal, March, 1804.

† The deſcription of this contrivance being already publiſhed, it would be ſuperfluous to repeat it. I only beg leave to add the following practical remark. A metallic piſton, if well turned and fitted into a cylinder of exactly the ſame kind of metal, will probably have the ſame degree of expanſion, eſpecially if hollow, and conſequently will not increaſe its friction in any increaſed degree of temperature. But as in practice the cylinder is commonly expoſed to a lower temperature than the piſton, heated by the ſteam, a little increaſe of friction will take place by an increaſe of heat. To prevent the effect of this, I have found it uſeful to employ for the piſton a metal of ſomewhat leſs expanſive power than the cylinder; and the expanſion of red copper being to that of braſs nearly as 10 to 11, I prefer making the piſton of the former metal, when the cylinder is made of braſs.

Primary animal fluids.

Albumen.

The firſt of the animal fluids which I propoſe to make the ſubject of my inveſtigation is the albumen. With the exception of water, no fluid appears to enter ſo largely into the compoſition of the animal body. It forms a very conſiderable proportion of the blood, and is found in greater or leſs quantity in nearly all the ſecretions. It is alſo capable of aſſuming the ſolid form, without undergoing any change in its chemical properties; in this ſtate it conſtitutes the baſis of all the membranous ſubſtances, which are ſo extenſively diſperſed through every part of the ſyſtem; it compoſes the cellular tiſſue into which the earth of the bones, and the fibrous matter of the muſcles are depoſited, while it enters largely into the ſtructure of the ſkin, the glands and the veſſels. At preſent, however, we ſhall direct our attention to it while in the liquid form.

Albumen, a large component part of animals.

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* Even Mr. Abernethy, in his late valuable work on tumors, ſpeaks of the gelatinous part of the blood, where I conceive from the context, there can be no doubt that he intended to deſignate the fibrine.

† All the animal fluids, both primary and compound, are merely ſolutions of a ſolid body in water; but thoſe ſubſtances which are moſt frequently ſeen in a ſtate of ſolution, have very generally obtained the title of fluids.

In

Moſt conveniently obtained from white of egg.

In order to obtain it in a ſtate of purity, I had recourſe to the white of the egg, a ſubſtance to which the name of albumen was originally applied, and which is ſtill conſidered by the moſt eminent chemiſts * as compoſed entirely of this ſubſtance. In order to aſcertain how far this opinion was correct, I kept a quantity of the white of the egg in a temperature of 212°, until it was firmly coagulated. It was then cut into ſmall pieces and placed in the upper part of a narrow-necked funnel, when a few drops of a browniſh viſcid fluid were ſeparated from it. Other pieces of the ſame coagulum were kept for ſome time in boiling water, the fluid being then paſſed through a filtre, had acquired a light browniſh colour, and a faint odour; when agitated it was ſlightly mucilaginous. By ſlow evaporation, a ſmall quantity of a brittle, ſemi-tranſparent ſubſtance was obtained. It appeared therefore evident, that the white of the egg contains a ſmall quantity of a ſubſtance incapable of coagulation, and therefore eſſentially different from albumen. I ſtill, however, continued to employ it for the purpoſe of aſcertaining the properties of albumen, as it affords this ſubſtance in a ſtate of greater purity than it can be obtained from any other ſource.

White of egg is not pure albumen; it contains a little of matter not coagulable.

White of egg contains 80 parts water; 15,5 ſolid albumen and 4,5 uncoagulable matter.

It was an object of ſome importance to aſcertain the proportion which this foreign ingredient bore to the albumen itſelf; 100 grains of firmly coagulated albumen were kept for ſome time in boiling water, and this being poured off, a freſh quantity was added, and this proceſs repeated until the water appeared to contain no farther impregnation. The whole of the fluid was then evaporated, and a reſiduum was left which amounted to 4½ grains. Beſides the admixture of this peculiar ſubſtance, the white of the egg contains a conſiderable quantity of water, not only in its liquid ſtate, but after it is coagulated. By a gentle heat the water may be evaporated, and the ſolid matter is left behind in the form of a hard, brittle, tranſparent ſubſtance; I have found that upon an average, ⅘ of recently coagulated albumen may be conſidered as conſiſting of water. Hence it will appear, that 100 grains of the white of egg conſiſt of 80 grains water, 4.5 grains of uncoagulable matter, and 15.5 grains only of pure albumen.

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* Hatchett, Phil. Tranſ. 1800, p. 375.
Thomſon's Chemiſtry, IV. p. 484.

The

Diſtinguiſhing character of albumen coagulated by heat.

The moſt diſtinguiſhing characteriſtic of albumen is the property of being coagulated by heat, which forms an obvious and eaſy teſt of its preſence, when it exiſts in a compound animal fluid in any conſiderable proportion. In order to aſcertain how ſmall a quantity of albumen could by this means be rendered viſible, I added 13 grains of the white of the egg to 87 grains of water, and thus formed a ſolution, one grain of which contained $\frac{1}{10}$ grain of pure albumen. Five grains of this ſolution were then added to 95 grains of water, ſo that 100 grains of water contained $\frac{1}{10}$ grain only, or $\frac{1}{1000}$ of its weight of pure albumen. This was expoſed to the heat of boiling water, and a perceptible opacity was produced in the fluid.

One thouſandth part in water will coagulate;

—oxymuriate of mercury precipitates it;

The effects of the oxymuriate of mercury were next tried. One drop of a ſaturated ſolution of this ſalt, added to 100 grains of water, containing $\frac{1}{1000}$ of its weight of albumen, produced a very evident milkineſs; after ſome hours a curdy precipitate ſeparated and fell to the bottom. A ſolution of half the ſtrength, containing only $\frac{1}{2000}$ of its weight of albumen, was then tried by the ſame re-agent, and even in this inſtance, a ſufficiently obvious effect was produced.

—as does nitromuriate of tin,

The nitro-muriate of tin is a powerful coagulater of albumen in its unmixed ſtate, but I found it not to be ſo delicate a teſt as the oxymuriate of mercury. One hundred grains of water, containing $\frac{1}{5}$ grain of albumen, *i. e.* $\frac{1}{500}$ of its weight, was not affected by this re-agent until after ſome hours, when the fluid exhibited a degree of milkineſs.

—and tannin.

In order to aſcertain the effects of tannin upon albumen, I macerated half an ounce of powdered galls in half a pint of water for ſome hours, and filtered the fluid. A deep brown tranſparent liquor was produced, 100 grains of which I found by evaporation to contain 2½ grains of ſolid reſiduum. Equal parts of this preparation of galls and of a ſolution of albumen, in the proportion of one part to 1000 parts of water, were mixed together; at firſt no effect could be perceived, but after ſome time an evident precipitate was formed and ſlowly ſubſided.

Goulard throws it down;

The aqua lithargyri acetati, or extract of Goulard, is an active precipitant of ſeveral animal fluids; when dropped into a ſtrong ſolution of albumen, a very copious and denſe precipitate is immediately formed.

—but doubtfully.

Aqua lithargyri acetati plentifully precipitates it.

It is, however, ſomewhat difficult to determine how far this effect depends upon the albumen itſelf, as Goulard has been conſidered to be the appropriate teſt of the uncongelable part of the ſerum of the blood, which, it may be ſuppoſed, reſembles the uncoagulable part of the white of the egg. The aqua lithargyri acetati is likewiſe decompoſed by ſeveral of the ſaline bodies which are found to exiſt in almoſt all the animal fluids. I have ſometimes found it to yield a precipitate even when added to diſtilled water, and in all caſes, after expoſure to the atmoſphere for a few hours, the water to which it has been added becomes turbid, and is covered with a thin film. In order to try the effect of this re-agent on albumen, I added one drop of it to 200 grains of water, and upon obſerving that the tranſparency of the fluid was not affected, a ſingle drop of the ſolution of albumen, of the ſame ſtrength with that mentioned above, was added. It formed a denſe precipitate as it fell through the fluid, and upon agitation, the whole was rendered ſlightly milky. In this caſe the proportion of the water to the albumen was as 10.000 to 1; to the uncoagulable part of the white of the egg, it would be about as 30,000 to 1.

Nitrate of ſilver alſo has a like effect;

The next re-agent which I employed was the nitrate of ſilver. A ſingle grain of a ſaturated ſolution of this ſalt produced an evident turbidneſs in 100 grains of water, containing $\frac{1}{10}$ gr. of albumen, and after ſome hours a ſmall quantity of a curdy precipitate fell to the bottom of the veſſel. It might, however, be ſuſpected that in this caſe, the effect produced depended upon a quantity of muriate of ſoda contained in the albumen. I found that 100 grains of water, containing only $\frac{1}{100,000}$ gr. of common ſalt was rendered evidently turbid by one drop of the nitrate of ſilver;* but the precipitate which is formed by the nitrate of ſilver acting on the muriate of ſoda is in the form of a greyiſh powder, and ſubſides more rapidly than it did in the former inſtance, where it produced a white, flaky precipitate.

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* I weighed very exactly a grain of ſalt, and diſſolved it in 200 grains of water. One grain of this ſolution was afterwards added to 99 grains of water, and by repeating the operation for three ſucceſſive times, I obtained 100 grains of water, containing only $\frac{1}{10,000}$ of its weight of ſalt. I then took 99 grains of diſtilled water, and poured into it one drop of the nitrate of ſilver: after waiting for ſome time, until I was ſatisfied that no effect would take place, I added a ſingle drop of the laſt ſolution of ſalt, thus making it $\frac{1}{100,000}$ part of the mixture; a faint but perceptible opacity was almoſt immediately produced.

—and nitro- riate of gold

I found that a ſolution of albumen of the ſame ſtrength with that employed in the laſt experiment, was immediately decompoſed by the nitro-muriate of gold. One drop of this metallic ſolution inſtantly produced a denſe white precipitate in 100 drops of water, containing only $\frac{1}{10}$ of a grain of albumen.

—and alum.

Albumen in a concentrated ſtate is powerfully coagulated by alum; I found however, that this re-agent is not ſo accurate a teſt of its preſence when in a diluted ſtate, as ſome of thoſe which I had already employed: $\frac{1}{5}$ grain of albumen, diſſolved in 100 grains of water, was indeed rendered ſlightly turbid by the addition of a few drops of a ſaturated ſolution of alum, but no precipitate was formed.

Obs. on the lution of alb men.

Before I conclude my account of theſe experiments I muſt obſerve, that the ſtrength of the ſolution of albumen was in all caſes rather leſs than my eſtimate. When I added the albumen to the water, a ſmall portion of it always remained inſoluble, and this was ſeparated from the fluid by filtration before the experiments were performed. This inſoluble part I ſuppoſed to conſiſt of the membranous matter, with which it is ſaid that the white of the egg is interſected. The quantity was indeed almoſt too ſmall to be appreciated, but where it is deſirable to attain as much accuracy as poſſible, I think it neceſſary to mention every circumſtance which may in the ſmalleſt degree affect the reſult.

Remarks. coagulation b heat is a good diſtinctive ch racter of alb men.

The experiments related above will, I conceive, indicate with a ſufficient degree of accuracy, the preſence of albumen as a conſtituent of an animal fluid. The property of being coagulated by heat is a characteriſtic of this ſubſtance, which will always ſerve as a mark of diſcrimination, and we have found that this property is not deſtroyed by dilution with 1000 times its weight of water. This therefore may be conſidered as a teſt of its preſence minute enough for all practical purpoſes. We have alſo found that there are ſeveral re-agents which poſſeſs the power of precipitating it from its ſolution in water, while exiſting only in the ſame proportion. It will be neceſſary, however, to obſerve their operation upon the other animal ſubſtances before we can determine their uſe in the analyſis or compound fluids.

Jelly.

Jelly.

Jelly. It is liquifiable by heat and becomes concrete by cold; and is a large component part of animals.

The next ſubſtance which I propoſe to examine is jelly. The peculiar characteriſtic of this body is its property of becoming concrete by cold, and being liquified by the application of a gentle heat. It enters into the compoſition of the blood, though leſs largely than the albumen. It is alſo an ingredient in the ſkin, membranous texture, ligaments, cartilages and tendons. By boiling it is eaſily extracted from theſe ſubſtances, and by evaporation and cooling the whole is reduced to a maſs of greater or leſs ſolidity, in proportion to the previous degree of concentration. By a proceſs of this nature, iſinglaſs is prepared from the bones and cartilages of fiſh; as this ſubſtance has been conſidered to conſiſt of jelly nearly in a ſtate of purity, I employed it for the following experiments.

Obtained by the ſolution of iſinglaſs. One fiftieth part in water will coagulate.

Four grains of iſinglaſs were diſſolved in 200 grains of water, and thus a ſtandard fluid was formed, one grain of which contained $\frac{1}{50}$ gr. of jelly. This ſolution became perfectly concrete by cooling. In the firſt place I wiſhed to aſcertain how ſmall a proportion of jelly diſſolved in water was capable of aſſuming the concrete ſtate. Equal parts of the ſtandard fluid and of water, *i. e.* one part of jelly to 100 parts of water, produced a compound which was completely ſtiffened by cooling; but I found that two parts of water to one of the ſtandard, *i. e.* one part of jelly to 150 parts of water, produced a compound, which though evidently gelatinous, did not aſſume the concrete form.

It is actively precipitated by tanin.

One of the moſt active precipitants of jelly is the tanning principle. I found that a mixture of 5 grains of the ſtandard ſolution and 95 of water, produced a copious precipitate when added to an equal quantity of an infuſion of galls of the ſame ſtrength with that employed in the experiments upon albumen. In this inſtance the jelly was to the water as 1 to 1000. I afterwards reduced the quantity of jelly until it compoſed $\frac{1}{3000}$ part only of the ſolution, and in this caſe a conſiderable precipitate was ſtill produced by the infuſion of galls.

—but not by aq. lith. acet.

A quantity of the ſtandard ſolution had a few drops of the aqua lithargyri acetati added to it, but no more effect appeared to be produced than would have enſued from mixing it with an equal quantity of pure water.

No effect was produced by adding the oxymuriate of mercury to the ſtandard ſolution. The nitrate of ſilver and the nitro-muriate of tin were each employed, and produced a very ſlight and almoſt imperceptible opacity.

—nor by ox. m. of merc. nor n. of ſilv. nor n. m. of tin.

The addition of the nitro-muriate of gold cauſed a ſmall quantity of a denſe precipitate when added to the ſtandard ſolution; but when this was ſo far diluted as to contain one grain of jelly in 500 of water, the effect was no longer perceptible.

N. m. of gold gave precip.

Mucus.

Animal mucus or mucilage enters largely into the conſtitution of many parts of the body, and forms a conſiderable proportion of ſeveral of the ſecretions. The term mucus had been generally employed in a vague and unreſtricted ſenſe, until Mr. Hatchet, in his valuable paper on the membranous parts of animals, inſerted in the Phil. Tranſ. for 1800, attempted to aſſign to it a more appropriate and definite meaning. He conceives that jelly and mucus are only modifications of the ſame ſubſtance, and do not eſſentially differ from each other; he conſiders it to be entitled to the appellation of mucus, when it is ſoluble in cold water and cannot be brought to the gelatinous ſtate *. Dr. Thomſon adopts in general the idea of Mr. Hatchett, and lays down the following as the characteriſtic properties of animal mucilage. It is ſoluble in cold water, inſoluble in alcohol, neither coagulable by heat nor generating into a jelly, precipitable by tan and the nitro-muriate of tin †. I have been induced from the reſults of my obſervations, to form a different opinion reſpecting the relation of jelly and mucus, but I ſhall defer the ſtatement of it until I have related the experiments which have led me to diſſent from ſuch high authority.

Mucus; a term not diſtinctly applied by chemiſts.

By agitating for a ſhort time ſome recent ſaliva in cold water, part of it was diſſolved, and after being paſſed through a filter, was made the ſubject of experiment, being, as I conceived, a ſolution of nearly pure mucus. By a careful evaporation I found that the water had diſſolved $\frac{1}{140}$ part of its weight.

Mucus obtained from ſaliva.

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* Hatchett, Phil. Tranſ. 1800. 369 and 381.

† Thomſon, IV. 503.

No

Effect of reagents. No coagulation by moderate heat nor did it gelatiniſe by cooling.

No effect was produced by the addition of the oxymuriate of mercury, and the nitro-muriate of tin cauſed only a ſlight opacity. No effect was produced by the addition of equal parts of this ſolution and the infuſion of galls. The aqua lithargyri acetati added to the ſolution produced an immediate opacity, and after ſome time a white, fleaky precipitate fell to the bottom of the glaſs. No appearance of coagulation was produced by expoſing the fluid for ſome time to the heat of boiling water, nor was there any tendency to gelatinize by evaporating and afterwards cooling the fluid.

Mucus from an oyſter.

I afterwards endeavoured to obtain mucus in a ſtate of purity from another ſource. For this purpoſe an oyſter was agitated for a few minutes in cold water; the fluid was filtered and appeared ſlightly opake and glutinous.

By evaporation it appeared that it had diſſolved about $\frac{1}{10}$ of its weight of animal matter. A quantity of this ſolution, diluted with an equal bulk of water, was employed in the following experiments.

Reagents.

The oxymuriate of mercury being added to it produced no effect. The infuſion of galls after ſome time produced a ſlight degree of turbidneſs, and at length a precipitate was formed in ſmall quantity. The aqua lithargyri acetati cauſed an immediate opacity, and after ſome time a denſe precipitate.

The Goulard indicates its preſence.

Theſe experiments nearly coincide with the former. In both caſes no effect was produced by the oxymuriate of mercury, thus proving the abſence of albumen. The ſmall precipitate cauſed by the galls indicate the exiſtence of only a very minute quantity of jelly. The effect was ſcarcely as great in this inſtance, where the animal matter compoſed $\frac{1}{100}$ part of the weight of the ſolution, as was produced by the ſame reagent upon a ſolution of jelly, where it compoſed only $\frac{1}{1000}$ part of the weight of the fluid. Very nearly the whole therefore of the animal matter probably conſiſted of mucus, the preſence of which was indicated by the Goulard.

Excluſive teſts; tan for jelly, aq. lith. acet. for mucus, ox. m. of merc. for albumen.

I apprehend that theſe experiments will be deemed ſufficient to eſtabliſh a decided and eſſential difference between mucus and jelly, independent of the gelatinizing property of the latter, the effects produced upon them by the tanning principle and by the aqua lithargyri acetati are exactly oppoſite. Tan is a moſt delicate teſt of jelly, but does not in any degree affect mucus. Aqua lithargyri acetati is a delicate teſt of mucus, but does not in any degree affect jelly. The oxymuriate of mercury, on the contrary, which is one of the moſt accurate teſts of albumen, does not appear to be affected either by jelly or by mucus.

These are the primary and general fluids.

Albumen, jelly, and mucus, I am inclined to conſider as the only primary fluids which are diſperſed through the different parts of the body. Particular veſſels or glands contain and ſecrete particular fluids, which cannot be reſolved into other fluids without decompoſition, as the fibrine of the blood, the reſin of the bile, the urée of the kidney, &c. but theſe are in all inſtances confined to their appropriate organs, and do not neceſſarily enter into the preſent inveſtigation.

Reſumption. Albumen is known by its coagulability, and precip. by ox. m. merc.

From the above experiments I think we may be entitled to lay down, with a conſiderable degree of accuracy, the leading characteriſtics of the three primary animal fluids, and to eſtabliſh teſts by which their preſence may be minutely aſcertained. The moſt remarkable property of albumen is its becoming coagulated by heat, a property which it retains ſo far as to communicate a degree of opacity to its ſolution in water, when it forms only $\frac{1}{1000}$ part of its weight. A ſolution of the ſame ſtrength has its albumen precipitated by the oxymuriate of mercury, and this teſt will indicate its preſence when compoſing no more than $\frac{1}{2000}$ of the mixture. The tanning principle, the aqua lithargyri acetati, the nitrate of ſilver, and the nitro-muriate of gold, are all teſts of the preſence of albumen nearly as minute as the oxymuriate of mercury, but they are leſs valuable, becauſe their effects are not confined to albumen. The nitro-muriate of tin and alum are alſo precipitants of albumen, but they are leſs delicate in their operation than the reagents enumerated above.

Jelly is known by its concretion on cooling and its precip. by tan.

The peculiar characteriſtic of jelly is its property of becoming concrete by cold, and being again rendered fluid by a gentle heat: we have found that its ſolution in water retains this property when it compoſes $\frac{1}{100}$ part only of the weight of the fluid. Tan is a ſtill more minute teſt of jelly than of albumen, but jelly is not in the leaſt degree affected by the oxymuriate of mercury, and may thus in all caſes be eaſily diſtinguiſhed from it. No effect is produced in jelly by Goulard, and ſcarcely any by the nitrate of ſilver, and the nitro-muriate of tin, when it is in a ſtate of ſuch dilution. By means of tan, jelly may be eaſily detected in a fluid of which it forms only $\frac{1}{5000}$ part.

Mucus is negative as to the preceding characters; but precip. by aq. lith. acet.

The properties of mucus are principally negative; it is no coagulable by heat, nor capable of becoming gelatinized; i is not precipitable either by the oxymuriate of mercury or b tan, but it may be detected with conſiderable minuteneſs b the aqua lithargyri acetati.

Other teſts.

It appears therefore that the oxymuriate of mercury, tan, and the aqua lithargyri acetati are the three moſt valuable teſts. The nitro-muriate of tin is a leſs delicate teſt of albumen than the oxymuriate of mercury, and is alſo in ſome degree affected by jelly. The nitrate of ſilver appears to be a very nice teſt of albumen, but it is objectionable in conſequence of its being decompoſed by the muriate of ſoda, a ſalt which is ſuppoſed to exiſt in moſt of the animal fluids. The nitro-muriate of gold is a delicate teſt of albumen, but it likewiſe precipitates jelly.

Order of analyſis.

In the analyſis of a fluid which is ſuppoſed to contain either albumen, jelly, or mucus, the firſt ſtep is to obſerve the effect of the oxymuriate of mercury; if this produce no precipitate, we may be certain that the fluid in queſtion contains no albumen. We muſt next employ the infuſion of galls, and if this alſo cauſe no precipitate, we may conclude that the animal matter held in ſolution conſiſts of mucus alone.

Remark.

I have before remarked, that the ideas which I have formed of the nature of jelly and mucus, and the relation which theſe ſubſtances bear to each other, differ materially from thoſe of Mr. Hatchett. It is not indeed without a degree of diffidence that I diſſent from ſo diſtinguiſhed a chemiſt; but I conceive that I am juſtified by the experiments related in this eſſay.—Mr. Hatchett, in the valuable paper to which I have already referred, ſpeaks of the white of the egg as conſiſting of pure albumen; but I believe that in this particular he will be found not perfectly accurate.

Animal mucus reſembles veget. gum.

There is a great reſemblance between the mechanical properties of animal mucus and vegetable gum, and I found that they ſtrongly reſemble each other alſo in their chemical qualities. A ſolution of gum arabic, containing one grain of gum to 200 grains of water, was not affected either by the oxymuriate of mercury or by tan. With the nitro-muriate of tin and with the nitrate of ſilver there was only a ſlight degree of opacity, but with the aqua lithargyri acetati there was a denſe precipitate inſtantly formed.

V.

On muſcular Motion. By ANTHONY CARLISLE, *Eſq. F. R. S. being the Croonian Lecture, read before the Royal Society, November* 8, 1804.

(Concluded from Page 201.)

Cold deſtroys mobility and ſenſation.

THE loſs of motion and ſenſation from the influence of low temperature, accompany each other, and the capillaries of the vaſcular ſyſtem appear to become contracted by the loſs of animal heat, as in the examples of numbneſs from cold. Whether the ceſſation of muſcular action be owing to the impeded influence of the nerves, or to the lowered temperature of the muſcles themſelves, is doubtful; but the known influence of cold upon the ſenſorial ſyſtem, rather favours the ſuppoſition that a certain temperature is neceſſary for the tranſmiſſion of nervous influence, as well as ſenſation.

Hybernating animals not eaſily drowned. Experiment with the hedge-hog.

The hybernating animals require a longer time in drowning than others. A full grown hedge-hog was ſubmerſed in water at 48°, and firmly retained there; air-bubbles began inſtantly to aſcend, and continued during four minutes; the animal was not yet anxious for its liberty. After ſeven minutes it began to look about, attempting to eſcape; at ten minutes it rolled itſelf up, only protruding the ſnout, which was haſtily retracted on being touched with the finger, and even the approach of the finger cauſed it to retract. After fifteen minutes complete ſubmerſion, the animal ſtill remained rolled up, and withdrew its noſe on being touched. After remaining thirty minutes under water, the animal was laid upon flannel, in an atmoſphere of 62°, with its head inclined downwards; it ſoon began to relax the ſphincter muſcle which contracts the ſkin, ſlow reſpirations commenced, and it recovered entirely, without artificial aid, after two hours. Another hedge-hog ſubmerſed in water at 94°, remained quiet until after five minutes; about the eighth minute it ſtretched itſelf out, and expired at the tenth. It remained relaxed, and extended, after the ceſſation of the vital functions; and its muſcles were relaxed, contrary to thoſe of the animal drowned in the colder water.

Connection of irritability with reſpiration, &c.

The irritability of the heart is inſeparably connected with reſpiration. Whenever the inhaled gas differs in its properties from the common atmoſphere, the muſcular and ſenſible parts of the ſyſtem exhibit the change: the actions of the heart are altered or ſuſpended, and the whole muſcular and ſenſorial ſyſtems partake of the diſorder: the temperature of animals, as before intimated, ſeems altogether dependant on the reſpiratory functions, although it ſtill remains uncertain in what manner this is effected.

Diſtribution of heat by the blood.

The blood appears to be the medium of conveying heat to the different parts of the body; and the changes of animal temperature in the ſame individual at various times, or in its ſeveral parts, are always connected with the degree of rapidity of the circulation. It is no very wide ſtretch of phyſiological deduction to infer, that this increaſed temperature is produced by the more frequent expoſure of the maſs of blood to the reſpiratory influence, and the ſhort time allowed in each circuit for the loſs of the acquired heat.

Coagulation of that fluid.

The blood of an animal is uſually coagulated immediately after death, and the muſcles are contracted; but, in ſome peculiar modes of death, neither the one, nor the other of theſe effects are produced: with ſuch exceptions, the two phenomena are concomitant.

Heat delays the laſt coagulation and the contractions of the muſcles, &c.

A preternatural increaſe of animal heat delays the coagulation of the blood, and the laſt contractions of the muſcles: theſe contractions gradually diſappear, before any changes from putrefaction are manifeſted; but the cup in the coagulum of blood does not relax in the ſame manner; hence it may be inferred, that the final contraction of muſcles is not the coagulation of the blood contained in them; neither is it a change in the reticular membrane, nor in the blood-veſſels, becauſe ſuch contractions are not general throughout thoſe ſubſtances. The coagulation of the blood is a certain criterion of death. The reiterated viſitations of blood are not eſſential to muſcular irritability, becauſe the limbs of animals, ſeparated from the body, continue for a long time afterwards capable of contractions, and relaxations.

The chemical combinations of living matter are tranſient, and not renewable.

The conſtituent elementary materials of which the peculiar animal and vegetable ſubſtances conſiſt, are not ſeparable by any chemical proceſſes hitherto inſtituted, in ſuch manner as to allow of a recombination into their former ſtate. The compoſition of theſe ſubſtances appears to be naturally of tranſient duration, and the attractions of the elementary materials which form the groſs ſubſtances, are ſo looſe and unſettled, that they are all decompoſed without the intervention of any agent, merely by the operation of their own elementary parts on each other.

Difficulties attending chemical reſearch into theſe objects.

An extenſive diſcuſſion of the chemical properties attaching to the matter of muſcle would be a labour unſuited to this occaſion; I ſhould not, however, diſcharge my preſent duty, if I omitted to ſay, that all ſuch inveſtigations can only be profitable when effected by ſimple proceſſes, and when made upon the raw materials of the animal fabric, ſuch, perhaps, as the albumen of eggs, and the blood. But, until by ſynthetical experiments the peculiar ſubſtances of animals are compoſed from what are conſidered to be elementary materials, or the changes of organic ſecretion imitated by art, it cannot be hoped that any determinate knowledge ſhould be eſtabliſhed upon which the phyſiology of muſcles may be explained. Such reſearches and inveſtigations promiſe, however, the moſt probable ultimate ſucceſs, ſince the phenomena are neareſt allied to thoſe of chemiſtry, and ſince all other hypotheſes have, in their turns, proved unſatisfactory.

Facts and Experiments tending to ſupport and illuſtrate the preceding Argument.

Temperature of the primary fluids in different animals. The horſe.

An emaciated horſe was killed by dividing the medulla ſpinalis, and the large blood-veſſels under the firſt bone of the ſternum.

The temperature of the flowing blood was	103°
Spleen - -	103
Stomach - -	101
Colon - -	98
Bladder of urine	97
Atmoſphere -	30.

Pigs.

Three pigs, killed by a blow on the head, and by the immediate diviſion of the large arteries and veins, entering the middle of the baſis of the heart, had the blood flowing from theſe veſſels of 106, 106½, and 107°; the atmoſpheric temperature being at 31°.

An ox.

An ox, killed in a ſimilar manner, the blood 103; atmoſphere 50°.

Sheep.

Three ſheep, killed by dividing the carotid arteries, and internal jugular veins: their blood 105, 105, 105½°; atmoſphere 41°.

Frogs.

Three frogs, kept for many days in an equable atmoſphere at 54°; their ſtomachs 62.°

Fluid of dropſy.

The watery fluid iſſuing from a perſon tapped for dropſy of the belly 101°: the atmoſphere being 43°, and the temperature of the ſuperficies of the body at 96°.

Theſe temperatures are conſiderably higher than the common eſtimation.

Experiment to ſhew that the volumes of muſcles are increaſed during action.

A man's arm being introduced within a glaſs cylinder, it was duly cloſed at the end which embraced the head of the humerus; the veſſel being inverted, water at 97° was poured in, ſo as to fill it. A ground braſs plate cloſed the lower aperture, and a barometer tube communicated with the water at the bottom of the cylinder. This apparatus including the arm, was again inverted, ſo that the barometer tube became a gage, and no air was ſuffered to remain in the apparatus. On the ſlighteſt action with the muſcles of the hand, or fore-arm, the water aſcended rapidly in the gage, making librations of ſix and eight inches length in the barometer tube, on each contraction and relaxation of the muſcles.

Crimping of fiſh cannot be effected after rigidity of death.

The remarkable effects of crimping fiſh by immerſion in water, after the uſual ſigns of life have diſappeared, are worthy attention; and whenever the rigid contractions of death have not taken place, this proceſs may be practiſed with ſucceſs. The ſea fiſh deſtined for crimping are uſually ſtruck on the head when caught, which, it is ſaid, protracts the term of this capability; and the muſcles which retain this property longeſt are thoſe about the head. Many tranverſe ſections of the muſcles being made, and the fiſh immerſed in cold water, the contractions called crimping take place in about five minutes; but, if the maſs be large, it often requires thirty minutes to complete the proceſs.

Experiment. Two fiſh were ſcored and one of them crimped. Its ſpecific gravity, and alſo its abſolute weight, was increaſed by crimping.

Two flounders, each weighing 1926 grains, the one being in a ſtate for crimping, the other dead and rigid, were put into water at 48°, each being equally ſcored with a knife. After half an hour, the crimped fiſh had gained in weight 53 grains, but the dead fiſh had loſt 7 grains. The ſpecific gravity of the crimped fiſh was greater than that of the dead fiſh, but a quantity of air-bubbles adhered to the ſurfaces of the crimped muſcles, which were rubbed off before weighing; this gas was not inflammable.

The ſpecific gravity of the crimped fiſh - - 1,105
of the dead fiſh, after an equal
immerſion in water - - 1,090.

So that the acceſſion of water ſpecifically lighter than the muſcle of fiſh, did not diminiſh the ſpecific gravity of crimped muſcle, but the contrary: a proof that condenſation had taken place.

Other ſubſtances crimped with like effect.

A piece of cod-fiſh weighing twelve pounds, gained in weight, by crimping, two ounces avoirdupois; and another leſs vivacious piece, of fifteen pounds, gained one ounce and half*.

The hinder limb of a frog, having the ſkin ſtripped off, and weighing $77\frac{1}{10}$ grains, was immerſed in water at 54°, and ſuffered to remain nineteen hours, when it had become rigid, and weighed $100\frac{1}{4}$ grains. The ſpecific gravity of the contracted limb had increaſed, as in the crimped fiſh.

Six hundred and thirty grains weight of the ſubſcapularis muſcle of a calf, which had been killed two days from the 10th of January, was immerſed in New River water at 45°. After ninety minutes, the muſcle was contracted, and weighed in air 770 grains: it had alſo increaſed in ſpecific gravity, but the quantity of air-bubbles formed in the interſticial ſpaces of the reticular membrane made it difficult to aſcertain the degree.

Fibrils of muſcles contract by application of water.

Some of the ſmalleſt faſciculi of muſcular fibres from the ſame veal, which had not been immerſed in water, were placed on a glaſs plate, in the field of a powerful microſcope, and a drop of water thrown over them, at the temperature of 54°, the atmoſphere in the room being 57°. They inſtantly began to contract, and became tortuous.

On confining the ends of another fibril with little weights of glaſs, it contracted two-thirds of its former length, by ſimilar treatment. The ſame experiment was made on the muſcular fibres of lamb and beef, twelve hours after the animals had been killed, with the like reſults. Neither

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[*] I am informed that the crimping of freſh water fiſhes requires hard water, or ſuch as does not ſuit the purpoſes of waſhing with ſoap. This fact is ſubſtantiated by the practice of the London fiſhmongers, whoſe experience has taught them to employ pump water, or what is commonly called hard water.

vinegar,

but not by acid or ſaline water.

vinegar, nor water ſaturated with muriate of ſoda, nor ſtrong ardent ſpirit, nor olive oil, had any ſuch effect upon the muſcular fibres.

Cold renders muſcles torpid.

The amphibia, and coleopterous inſects, become torpid at 34°. At 36° they move ſlowly, and with difficulty; and, at a lower temperature their muſcles ceaſe to be irritable. The muſcles of warm-blooded animals are ſimilarly affected by cold.

Muſcles of frogs irritable after freezing.

The hinder limbs of a frog were ſkinned and expoſed to cold at 30°, and the muſcles were kept frozen for eight hours, but on thawing them, they were perfectly irritable.

The ſame proceſs was employed in the temperature of 20°. and the muſcles kept frozen for twelve hours, but that did not deſtroy the irritability.

Heat deprives muſcles of their irritability;

In the heat of 100°, the muſcles of cold-blooded animals fall into the contractions of death; and at 110°, all thoſe of warm blood, as far as theſe experiments have been extended. The muſcles of warm-blooded animals, which always contain more red particles in their ſubſtance than thoſe of cold blood, are ſoon deprived of their irritability, even although their relative temperatures are preſerved; and reſpiration in the former tribe is more eſſential to life than in the latter.

as do poiſons,

Many ſubſtances accelerate the ceſſation of irritability in muſcles when applied to their naked fibrils, ſuch as all the narcotic vegetable poiſons, muriate of ſoda, and the bile of animals; but they do not produce any other apparent change in muſcles, than that of the laſt contraction. Diſcharges of electricity paſſed through muſcles, deſtroy their irritability, but leave them apparently inflated with ſmall bubbles of gas; perhaps ſome combination obtains which decompoſes the water.

and electricity.

Experiments on muſcles immerſed in fluids.

The four ſeparated limbs of a recent frog were ſkinned, and immerſed in different fluids; *viz.* No. 1, In a phial containing ſix ounces by meaſure of a ſaturated aqueous ſolution of liver of ſulphur made with potaſh; No. 2, In a diluted acetic acid, conſiſting of one drachm of concentrated acid to ſix of water; No 3, in a diluted alkali, compoſed of cauſtic vegetable alkali one drachm, of water ſix ounces; No. 4, in pure diſtilled water.

The phials were all corked, and the temperature of their contents was 46°.

The limb contained in the phial No. 1, after remaining twenty minutes, had acquired a pale red colour, and the muſcles were highly irritable.

The limb in No. 2, after the ſame duration, had become rigid, white, and ſwollen; it was not at all irritable. By removing the limb into a diluted ſolution of vegetable alkali, the muſcles were relaxed, but no ſigns of irritability returned.

No. 3, under all the former circumſtances, retained its previous appearances, and was irritable, but leſs ſo than No. 1.

No. 4 had become rigid, and the final contraction had taken place.

Muſcular irritability deſtroyed by other cauſes.

Other cauſes of the loſs of muſcular irritability occur in pathological teſtimonies, ſome examples of which may not be ineligible for the preſent ſubject. Workmen whoſe hands are unavoidably expoſed to the contact of white lead, are liable to what is called a palſy in the hands and wriſts, from a torpidity of the muſcles of the fore arm. This affection ſeems to be decidedly local, becauſe, in many inſtances, neither the brain, nor the other members, partake of the diſorder; and it ofteneſt affects the right hand. An ingenious practical chemiſt in London has frequently experienced ſpaſms and rigidity in the muſcles of his fore arms, from affuſions of nitric acid over the cuticle of the hand and arm. The uſe of mercury occaſionally brings on a ſimilar rigidity in the maſſeter muſcles.

Leſs blood flows through a contracted muſcle.

A ſmaller quantity of blood flows through a muſcle during the ſtate of contraction, than during the quieſcent ſtate, as is evinced by the pale colour of red muſcles when contracted. The retardation of the flow of blood from the veins of the fore arm, during venæſection, when the muſcles of the limb are kept rigid, and the increaſed flow after alternate relaxations, induces the probability, that a temporary retardation of the blood in the muſcular fibrils takes place during each contraction, and that its free courſe obtains again during the relaxation. This ſtate of the vaſcular ſyſtem in a contracted muſcle, does not, however, explain the diminution of its bulk, although it may have ſome influence on the limb of a living animal.

When

A contracted muſcle is leſs ſenſible.

When muſcles are vigorouſly contracted, their ſenſibility to pain is nearly deſtroyed; this means is employed by jugglers for the purpoſe of ſuffering pins to be thruſt into the calf of the leg, and other muſcular parts with impunity: it is indead reaſonable to expect, *a priori*, that the ſenſation, and the voluntary influence, cannot paſs along the nerves at the ſame time *.

Moral cauſes influence the muſcles in the human ſpecies.

In addition to the influences already enumerated, the human muſcles are ſuſceptible of changes from extraordinary occurrences of ſenſible impreſſions. Long continued attention to intereſting viſible objects, or to audible ſenſations, are known to exhauſt the muſcular ſtrength: intenſe thought and anxiety, weaken the muſcular powers, and the paſſions of grief and fear produce the ſame effect ſuddenly: whilſt the contrary feelings, ſuch as the proſpect of immediate enjoyment, or moderate hilarity, give more than ordinary vigour.

Mental as well as muſcular actions may by habit become automatic,

It is a very remarkable fact in the hiſtory of animal nature; that the mental operations may become almoſt automatic, and, under ſuch habit, be kept in action, without any interval of reſt, far beyond the time which the ordinary ſtate of health permits, as in the examples of certain maniacs, who are enabled without any inconvenience, to exert both mind and body for many days inceſſantly. The habits of particular modes of labour and exerciſe are ſoon acquired, after which, the actions become automatic, demand little attention, ceaſe to be irkſome, and are effected with little fatigue: by this happy proviſion of nature, the habit of induſtry becomes a ſource of pleaſure, and the ſame appears to be extended to the docile animals which co-operate with man in his labours.

and then give little fatigue.

Voluntary and involuntary muſcles.

Three claſſes of muſcles are found in the more complicated animals. Thoſe which are conſtantly governed by the will, or directing power of the mind, are called voluntary muſcles. Another claſs, which operate without the conſciouſneſs of the mind, are denominated involuntary; and a mixed kind occur in the example of reſpiratory muſcles, which are governed by the will to a limited extent; nevertheleſs the exigencies of the animal feelings eventually urge the reſpiratory movements in deſpight of the will. Theſe laſt muſcles appear to have become automatic by the continuance of habit.

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* I have often obſerved that a ſmall electric ſhock may be received without pain through the muſcles of the fore arm; but I imagined it to be owing to the want of power in ſuch a ſhock to increaſe the contraction.—N.

The uſes of voluntary muſcles are attained by experience, imitation, and inſtruction: but ſome of them are never called into action among Europeans, as the muſcles of the external ears, and generally the occipito-frontalis. The purely involuntary muſcles are each acted upon by different ſubſtances, which appear to be their peculiar ſtimuli; and theſe ſtimuli co-operate with the ſenſorial influence in producing their contractions: for example, the bile appears to be the appropriate ſtimulus of the muſcular fibres of the alimentary canal below the ſtomach, becauſe the abſence of it renders thoſe paſſages torpid. The digeſted aliment, or perhaps the gaſtric juice in a certain ſtate, excites the ſtomach. The blood ſtimulates the heart, light the iris of the eye, and mechanical preſſure ſeems to excite the muſcles of the œſophagus. The laſt cauſe may perhaps be illuſtrated by the inſtances of compreſſion upon the voluntary muſcles, when partially contracted, of which there are many familiar examples. Probably the muſcles of the oſſicula auditûs are awakened by the tremors of ſound; and this may be the occaſion of the peculiar arrangement obſervable in the chorda tympani, which ſerves thoſe muſcles.

Voluntary actions require education; the involuntary are cauſed by ſtimuli.

Theſe extraneous ſtimuli ſeem only to act in conjunction with the ſenſorial power, derived by thoſe muſcles from the gangliated nerves, becauſe the paſſions of the mind alter the muſcular actions of the heart, the alimentary canal, the reſpiratory muſcles, and the iris; ſo that probably the reſpective ſtimuli already enumerated, only act ſubſerviently, by awakening the attention of the ſenſorial power, (if that expreſſion may be allowed,) and thereby calling forth the nervous influence, which, from the peculiar organization of the great chain of ſympathetic nerves, is effected without conſciouſneſs: for, when the attention of the mind, or the more intereſting paſſions prevail, all the involuntary muſcles act irregularly, and unſteadily, or wholly ceaſe. The movements of the iris of the common parrot is a ſtriking example of the mixed influence.

Stimuli ſeem to excite ſenſorial power in ſome reſpect reſembling the paſſions.

The muſcles of the lower tribes of animals, which are often entirely ſupplied by nerves coming from ganglions, appear of this claſs; and thus the animal motions are principally regulated

Lower tribes of animals act by external excitements.

lated by the external ſtimuli, of which the occurrence ſeems to agree with the animal neceſſities: but the extenſive illuſtrations which comparative anatomy affords on this point, are much too copious for any detail in this place.

The nervous power ſeems conſtantly active through life.

There are two ſtates of muſcles, one active, which is that of contraction, the other, a ſtate of ordinary tone, or relaxation, which may be conſidered paſſive, as far as it relates to the mind; but the ſenſorial or nervous power ſeems never to be quieſcent, as it reſpects either the voluntary or involuntary muſcles during life. The yielding of the ſphincters appears to depend on their being overpowered by antagoniſt muſcles, rather than on voluntary relaxation, as is commonly ſuppoſed.

I have now finiſhed this endeavour to exhibit the more recent hiſtorical facts connected with muſcular motion.

Concluſion.

It will be obvious to every one, that much remains to be done, before any adequate theory can be propoſed. I have borrowed from the labours of others, without acknowledgement, becauſe it would be tedious to trace every fact, and every opinion to its proper authority: many of the views are perhaps peculiar to myſelf, and I have adduced many general aſſumptions and concluſions, without offering the particular evidence for their confirmation, from a deſire to keep in view the remembrance of retroſpective accounts, and to combine them with intimations for future reſearch. The due cultivation of this intereſting purſuit cannot fail to elucidate many of the phenomena in queſtion, to remove premature and ill-founded phyſiological opinions, and eventually to aid in rendering the medical art more beneficial, by eſtabliſhing its doctrines on more extenſive and accurate views of the animal economy.

VI.

On the Meaſure of Mechanic Power. In a Letter from Mr. J. C. HORNBLOWER.

To Mr. NICHOLSON.

DEAR SIR,

Meaſure of mechanic power or effect.

I AM glad to find that ſomebody has ſeconded my motion concerning *horſe power*, and I hope the ſubject will not be diſmiſſed

Meaſure of mechanic power or effect.

miſſed until it has paſſed the unanimous aſſent of both theoretic and practical mechanics; and I muſt here expreſs my acknowledgments to Mr. Gregory for his improved method of deciding the queſtion. It is of abſolute importance, that the draught be in a circular direction, and alſo that the radius of the circle be given; for no poſition was ever more demonſtrable than that the leſs the radius the leſs can a horſe (whoſe ſides are equal) exert his faculty of traction.

But I muſt beg leave to uſe a little freedom (in no wiſe unbecoming, I hope,) in adverting to what Mr. Gregory in conjunction with Profeſſor Robiſon has advanced on the ſubject of Mr. Smeaton's mode of defining *mechanic powers* and *mechanical effect;* and I am ſurpriſed that among men of talent and aſſiduity there can be a difference of opinion. It ſeems to me, that if any reaſon can be found, it muſt be that we do not underſtand the ſubject, and, perhaps I may give a decided proof of it in what I ſhall advance concerning it.

However, I am ſure that when a ball of caſt iron, of twenty inches diameter is elevated by means of a pinion and wheel connected to the ſides of a pair of ſheers, and left to hang there a little while,—if I cut the rope that ſuſtains it, it will fall freely a certain height, in a certain portion of time, and would daſh a faulty cylinder of a ſteam engine in pieces. And I am ſo well ſatisfied that there was a certain tendency in this ball to fall towards the centre of the earth, that I need not take a moment to examine the truth of it; but that the deſtruction of the cylinder was occaſioned by the ball falling from the point to which it was raiſed, (how it was raiſed is no part of the ſubject.) Then I ſay, that if the ball had not fallen from the height it was, or if the ball had not been ſo heavy as it was, or if it had had its velocity retarded by any means, the cylinder would not have been broken. I will add, that had this ball been a true ſphere, and the cylinder had not been there, but a certain curved ſurface in its place, which ſhould have received the ball to prevent it impinging in the line of deſcent, it would have been turned out of its courſe conformably to the nature and poſition of the curve: and all that is above common apprehenſion in this matter, is, that while the ball was falling, its velocity was increaſing every inſtant, and that when it met with the curve (if it gave it an horizontal courſe) it would proceed with uniform velocity juſt

Meaſure of mechanic power or effect.

juſt twice the ſpace through which it fell, allowing the ſame time for this horizontal courſe as it had in falling its perpendicular height, and that its tendency is to continue that courſe for ever.

From this ſtatement of the law of bodies in motion, (ſo far as it goes) I think we need not be very diffident in ſaying, that it is from ſimilar facts, (though leſs philoſophically obſerved) that we obtain our primary ideas of motion. The apple falling from the tree is a very good inſtance to the point, and it would not require a very extraordinary ſtretch of genius to apply ſuch an accident as that to any thing like the pile engine, ſtamping preſs, &c.

But there are certain ſpeculative mechanics, who in their mode of accounting for effects like what are here ſtated, have choſen to adopt terms of a very different import, and for ſome latent reaſon wiſh to keep gravitation out of ſight. The writer of the article Dynamics in the Supplement of the Encyclopedia Britannica, calls it preſſure, and by his way of philoſophiſing in the explanation of the meaſure of mechanic power, has (in my opinion) laboured to make it as myſterious as poſſible.

I muſt for the ſake of thoſe of your readers who have not the work to refer to, quote a few paſſages now and then from the above popular work, and here I would refer them to the article on Machinery, where he begins by ſtating, that different notions have been entertained on this ſubject by Leibnitz, des Cartes, and other eminent mechanics of the laſt century, and adds, "*that ſome of the moſt eminent practitioners of the preſent times (for we muſt include Mr. Smeaton in the number) have given meaſures of mechanical power in machinery, which we think inaccurate, and tending to erroneous concluſions and maxims.*"

He then proceeds to explain and demonſtrate the true meaſure of mechanic power, and he begins by ſuppoſing a man preſſing uniformly on a maſs of matter for a certain time, and going on with the ſubject takes occaſion to diſtinguiſh between *the weight of a body* and *its heavineſs!* and, towards the latter end of that ſection he comes to ſome ſort of a concluſion of the ſubject, ſo far as to ſay what is the real meaſure of mechanic power: I ſee I muſt make endleſs quotations if I regard the very letter of his argument, but I hope I ſhall be excuſed; however I will quote the laſt paragraph verbatim.

Relating

Meaſure of mechanic power or effect.

Relating to the preſſure of the man juſt mentioned, he ſays, "but farther we know, that this preſſure is the exertion *, we have no other notion of our own force, and our notion of gravity, of elaſticity, or any other natural force is the ſame. We alſo know, that the continuance of this exertion fatigues and exhauſts our ſtrength as completely as the moſt violent motion. A dead pull as it is called of a horſe at a poſt fixed in the ground, is a uſual trial of his ſtrength. No man can hold out his arm horizontally for much more than a quarter of an hour, and the exertion of the laſt minutes gives the moſt diſtreſſing fatigue, and diſables the ſhoulder for action for a conſiderable time after. *This is therefore an expenditure of mechanical power in the ſtrict primitive ſenſe of the word.* Of this expenditure we have an exact and adequate effect and meaſure in the quantity of motion produced, that is in the product of the quantity of matter by the velocity generated in it by this exertion. And it muſt be particularly noticed, that the meaſure is applicable even to caſes where no motion is produced by the exertion; that is, if we know that the exertion which is juſt unable to ſtart a block of ſtone lying on a ſmooth pavement, but would ſtart it if increaſed by the ſmalleſt addition, and if we know that this would generate in a ſecond 32 feet of velocity in 100 pounds of matter, we are certain that it was a preſſure equal to the weight of this 100 pounds. It is a good meaſure, though not immediate, and may be uſed without danger of miſtake when we have no other."

I ſhould not have quoted ſo much of this ſection, if it had not been that I think it contains an unequivocal interpretation of the writers notion of the true meaſure of mechanic power, and at once exhibiting, in my proud opinion, the fallacy of the doctrine in *toto*. What! ſhall mere muſcular exertion, whether of horſe or man, be eſteemed even an auxiliary to get the conception of the nature of the thing; how much leſs then ſhall it be ſet forth as the thing itſelf? In perfect conſonance with this writer, Mr. Gregory, page 152, Philoſophical Journal, Vol. XI. ſays, ſuppoſe that a horſe while ſtanding ſtill, ſuſtains by means of a rope and ſimple fixed pulley, a maſs of an hundred weight, and thus keeps it ſuſpended at the top of a well for the ſpace of a minute; neither the animal nor the weight moves: but ſhall we ſay, in conformity

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* Article Machinery, Supplement, 4th ſec.

Meaſure of mechanic power or effect.

as it would ſeem with Mr. Smeaton's meaſure, that there is no power expended, no effect produced. On the contrary we know there is a power expended, and that effect, if ſufficiently long continued, would completely tire the horſe. Then let us have a *poſt* inſtead of the horſe, and ſurely that will not tire, and what will be the conſequence then? why then there will be no power expended, and no effect produced; and I beg leave to aſk my opponents, what is the power expended when a horſe or other animal is placed there to ſuſtain the weight? is it any more than the expence of nervous or muſcular action; and has that any analogy with a weight deſcending through a given ſpace, either uniformly or accelerated? and, I aſk again, what is the effect produced more than what is produced by the poſt? the horſe does but keep the weight from dropping into the well, and the poſt will do the ſame; indeed you may ſay, that when you hang up your hat, that the pin which ſuſtains it, prevents it from falling, as does the horſe the maſs in the well, and that therefore there muſt be ſome power expended on the pin.

It is really difficult to be grave on this occaſion; but I feel myſelf reſtrained by the magnitude of the ſubject, and its importance to the community. Profeſſor Robiſon ſays, when a man holds out his arm horizontally, the exertion towards the end of a quarter of an hour gives the moſt diſtreſſing fatigue, and then ſays this is an expenditure of mechanical power, which I ſhall take the liberty to deny for the preſent. But is it ſuch a mechanical power as Smeaton's, or in fine, is it a power made up of a maſs of matter moving with any determinate velocity either uniform or accelerated? If the learned Profeſſor intended to familiarize the doctrine to people of common ſenſe, he could not have choſen a more indirect and perplexing example.

But let us attend a little further to the ſubject in the fourth ſection of the article Machinery, Sup. Ency. Brit. There he ſays, that "when a man ſupports a weight for a ſingle inſtant, he certainly balances the preſſure or action of gravity on that body," by the way here is a great want of preciſion in the expreſſion, "*preſſure or action of gravity*," as if they were ſynonimous terms, whereas *preſſure* certainly denotes *repulſion*, if the term will bear any definition at all, and to explain the term *gravity* if it will not admit of *attraction*, I am ſure it cannot be called repulſion, but to proceed, "and he continues this action as long as he continues to ſupport it, and we know, that if this body were at the end of a horizontal arm turning round a vertical axis, *the ſame effort* which the man exerted in merely carrying the weight, if now exerted on the body by puſhing it horizontally round the axis, will generate in it the ſame velocity which gravity would generate by its falling freely."

Meaſure of mechanic power or effect.

A more erroneous propoſition was never introduced to the theory or practice of mechanics. What, is there no difference in a man carrying a load on his ſhoulder, and putting it into a truck? or to come nearer the Profeſſor's propoſition, let the man who has to carry two hundred pounds for one mile be permitted to take the weight from his ſhoulders, and reſt it on the arm of any thing like a horſe wheel, perfectly detached from the mill gear, let the gudgeons be oiled, and then let him "*puſh*" it horizontally round its axis until he has travelled a mile.

Now without aſking the man which he likes beſt, let us ſee what he does by placing his load on the arm of the horſe-wheel, and puſhing it round. Why, he certainly overcomes the additional friction which his load has added to the weight of the wheel, and that is all, and if you will let us have gudgeons which have no friction, the man need not to walk far to puſh the horizontal arm into perpetual motion.

But now for the monſtrous concluſion by this propoſition. "If the man's exertion was employed to generate motion inſtead of counteracting gravity, he would generate during that minute the ſame motion that gravity would; that is 60×32 feet velocity per ſecond in a maſs of 30 pounds. There would be 30 pounds of matter moving with the velocity of 1920 feet per ſecond. We would expreſs this production or effect by 30×1920, or by 57600 as the meaſure of the man's exertion during the minute."

Here is evidently a typographical error, *ſecond* for *minute*, but when we admit ſuppoſitions for the ſake of illuſtration, there ought to be ſome conformity in the ſuppoſition to the fact it is intended to illuſtrate: then I would aſk, where's the man who can generate 32 feet of velocity in a maſs of 30 pounds in one ſecond? to be ſure he can let it fall a ſecond, but he cannot *carry* it 32 feet in a ſecond; but he ſays, "we

Meaſure of mechanic power or effect.

would expreſs the production or effect by 30 × 1920 or 5760 as the meaſure of the man's exertion during the minute." Sir, it is more than even one of Boulton and Watts' horſes can do.—Well may he ſay, "ſuch an exertion will completely exhauſt a man's ſtrength."

He then goes on to conſider "*more narrowly* what a man *really does*, when he performs what Mr. Smeaton allows to be the production of a meaſurable mechanical effect. Suppoſe a weight of 30 pounds hanging by a cord which paſſes over a pulley, and that a man taking this cord over his ſhoulder, turns his back to the pulley, and walks away from it, we know that a man of ordinary force will walk along raiſing this weight at the rate of about ſixty yards in a minute, or a yard in every ſecond, and that he can continue to do this for eight or ten hours from day to day, and that this is all he can do without fatigue. Here are 30 pounds raiſed uniformly 180 feet in a minute, and Mr. Smeaton would expreſs this by 30 × 180 or 5400, and would call this the meaſure of the mechanical effect, and alſo of the expenditure of power. This is very different from our meaſure 57600."—Yes, but I hope not the leſs concluſive on that account.

It is wholly incomprehenſible to me why thoſe men (who have certainly a right to controvert any propoſition which appears to them erroneous) ſhould take up the ſubject, aſſuming points which the doctrine advanced by Mr. Smeaton has nothing to do with. It is clear that all this animal exertion comes at laſt to the law of bodies falling in accelerated velocity, which Smeaton allows to be a diſtinct conſideration, as he ſays, "if the weight deſcends quickly, it is ſenſibly compounded with another law, viz. the law of acceleration by gravity." But how inconſiſtent is it to go about to elucidate the laws of bodies in motion by the action of a man or a horſe. What is the expenditure of animal power but a waſte of what has been uſually termed by anatomiſts nervous ſpirits, or perhaps an inceptive diſorganization of the conſtituent parts of the ligaments and muſcles? in ſhort, we may compare it to contractions, inflammation, and gangrene, but never to the momentum acquired by a body moving through a certain ſpace in a certain time. It is remarkable, that all this reaſoning is about that of which Mr. Smeaton has never ſaid a word, except in his illuſtration of the mechanic power neceſſary to give velocity to heavy bodies, where he ſuppoſes a man puſhing an iron ball on an extended plane, and this not to try his muſcular force, or to ſee what he can do without tiring, but merely in elucidation of the doctrine he ſets out with.

Meaſure of mechanic power or effect.

I have not time at preſent to purſue the ſubject to the extent I wiſh, and to enter on the ground of the miſtake of this great man: nor let it be imagined for a moment that I have availed myſelf of his everlaſting abſence, to call in queſtion what he has advanced in refutation of a ſuppoſed error. It would have remained as it was unto a diſtant period, had it not been, that I ſee Mr. Gregory advancing the ſame opinion, which no doubt he will defend or deſert.

I am, Dear Sir,

Your obedient Servant,

J. C. HORNBLOWER.

If Mr. Gregory ſhould read this, I ſhall be glad if he will ſet me right as to the identity of *animal exertion* and *mechanic power*.

VII.

An Inveſtigation of all the Changes of the variable Star in Sobieſki's Shield, from five Years Obſervations; exhibiting its proportional illuminated Parts, and its Irregularities of Rotation; with Conjectures reſpecting unenlightened heavenly Bodies. By EDWARD PIGOTT, *Eſq. Abridged from the Philoſophical Tranſactions for* 1805.

THE author begins his memoir with an inveſtigation of the periods of change in the variable ſtar in Sobieſki's ſhield, of which the right aſcenſion was 279° 9½′, and its declination ſouth 5° 56′, for end of June 1796. Its rotation on its axis in 1796 was eſtimated at 62¼ days, from a mean of ſix obſervations of its greateſt and leaſt brightneſs. In the preſent paper he gives about 26 ſimilar determinations, moſt of them the reſults of very accurate obſervations made in the year 1796, 1797, 1798, 1799, and 1801. From all theſe reſults it

The variable ſtar in Sobieſki's ſhield, revolves on its axis in 61½ days.

it was found that the diſagreements between the periods of change, as deduced from its full brightneſs, were much greater than thoſe deduced from its leaſt illumination. The former gave the mean period 63 days, and the latter 59¾; and the mean of theſe, namely 61½ days, agreeing with the former determination to ¼ day, is as near as could be expected in obſervations of this nature.

The places of full and leaſt brightneſs do not equally divide the ſtar's circumference; they divide it as 7 to 8.

The author in the next place proceeds to examine ſome other of the changes to which this ſtar is ſubject. By tabulated obſervations through the ſame ſeries of years, he finds that the time of decreaſe, from the middle of its full brightneſs to the middle of its leaſt, is on a mean of 34 days; and that the time of its increaſe, from the middle of its leaſt brightneſs to the middle of its full, is in like manner only 27 days. The ſum of theſe numbers amounting to the period 61, ſhews their probable exactneſs. Theſe compared and combined with the former determinations of 1796, give a mean of the whole 33 + and 29 − days. As it thus appears that the time of the decreaſe is longer than that of the increaſe, it follows of courſe, that the places of the full and leaſt brightneſs are not ſituated at the diſtance of half the circumference from each other; and the like circumſtance Mr. P. affirms is found to be the caſe with moſt, if not all the variable ſtars.

Other variable ſtars are ſimilarly affected.

The luminous parts are themſelves variable.

The next particulars to be reviewed were the durations of its brightneſs without any perceptible change, while at its maximum and minimum. Theſe determinations required a tolerable ſucceſſion of obſervations; where that is not the caſe he has omitted them in his tables. From theſe it is found in general, that when the degree of brightneſs at its maximum is leſs than uſual, and its minimum not much decreaſed, the changes take place but very ſlowly, and cannot be ſettled with much accuracy unleſs the obſervations have been made frequently and with great attention. He accordingly paſſes again over the ſeries of years, ſhewing the dates of its magnitudes when at its full brightneſs, and alſo when at its leaſt brightneſs, and he tabulates at the end of the firſt part of his paper, all the different changes that have been examined. The words in the firſt column or compartment deſcribe them; in the ſecond column the preſent reſults are exhibited; in the third are the reſults of the former obſervations; and in the laſt column is placed a mean of both computed proportionally, according to the number of obſervations of each.

TABLE VIII.

Table of the general affections of the variable ſtar.

	Days.	Days.	Days, on a mean.
Rotation on its axis - - -	61½	62¼	62−
Duration of brightneſs, at its maximum, without any perceptible change - - - -	8+	14	9½
Ditto, when it does not attain its uſual brightneſs - - -	20−	—	—
Duration of brightneſs at its minimum, without any perceptible change - - - -	9−	9	9
Ditto, when it does not decreaſe ſo much as uſual - - -	20−	—	—
Decreaſe in time, from the middle of its full brightneſs to the middle of its leaſt - - - -	34	28	33+
Increaſe in time, from the middle of its leaſt brightneſs to the middle of its full - - -	27+	35	29−
Extremes of its different degrees of brightneſs; with a mean of its uſual variations -	5+ 9 or 0	5+ 7.8	5. 6

The author having thus ſettled with conſiderable preciſion theſe eſſential variations of the ſtar, proceeds to examine ſome of its other phenomena, particularly one which is common to moſt of the changeable ſtars, and likewiſe in ſome degree to our ſun, namely, that the times of their periodical returns of brightneſs are in general irregular,—a circumſtance ſo intereſting as to engage our attention, and which induced Mr. P. to make the ſucceſſion of obſervations, in the hope of diſcovering its nature and cauſe. With this view he has proceeded to tabulate the ſeries of years, ſo as to aſcertain the apparent rotations in days from the obſerved middle times of its full brightneſs, and alſo from the obſerved middle times of its leaſt brightneſs, for ſingle periods. From theſe it appears, that the periodical returns of brightneſs are uncommonly fluctuating, and that the differences between the extremes are very conſiderable; to account for which he offers the follow-

The changes in the luminous parts occaſion the periods ſingly taken to differ from each other.

ing explanations, ſuggeſting previouſly a few plauſible conjectures, and ſome inferences ariſing from the obſervations themſelves *.

Aſſumed poſitions. 1. The ſtars are opake. 2. They have regular rotations. 3. Their luminous appearance is cauſed by an atmoſphere like that of the ſun. 4. The luminous parts are ſparingly diſperſed in the ſtar here treated of:

1ſt. That the body of the ſtars are dark and ſolid.

2d. Their real rotations on their axes are regular.

3d. That the ſurrounding medium is by times generating and abſorbing its luminous particles in a manner nearly ſimilar to what has been lately ſo ingeniouſly illuſtrated by the great inveſtigator of the heavens, Dr. Herſchel, with regard to the ſun's atmoſphere.

4th. That theſe luminous particles are but *ſparingly diſperſed* in the atmoſphere ſurrounding the variable ſtar of Sobieſki, appears from the ſtar being occaſionally diminiſhed to the 6.7 magnitude, and much leſs. July 4, 1799, it was of the 7th; September 15, 1798, and Auguſt 9, 1803, *of the 9th, if not inviſible.* (See Table VII †.) Does not this indicate a very ſmall portion of light on its *darkened hemiſphere?*

5. Probably ſmall patches:

5th. And may we not with much plauſibility conſider them as ſpots, ſomewhat circular, or of no great extent? for even on its *brighteſt hemiſphere* the *duration* of its full luſtre is, on a mean, only 9½ days of the 62, or about one-ſixth and ½ of its circumference. (See Table VIII. page 140.) The dimenſions therefore of the parts enlightened ſeem much circumſcribed, and can be tolerably eſtimated, and conſequently may be repreſented very ſmall, particularly if the *powerful effect of a little light* and *the length of time* a bright ſpot is remaining in view be taken into conſideration.

6. Changeable in their nature:

6th. And a further ground of preſumption that thoſe principal bright parts are but ſlight patches is, that they undergo *perpetual changes*, and alſo that ſuch changes are very viſible to us, for moſt probably they would be imperceptible, were not the bright parts contraſted by conſiderable intervals or diminutions of light.

7. And deducible from the phenomena.

7th, and laſt. We may obtain ſome idea of the *relative ſituation or intervals between* theſe bright parts, by the obſervations of the increaſe and decreaſe of brightneſs, as thereby the changes and times elapſed are pointed out. (See Table V. page 136; and Phil. Tranſ. for 1797.)

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* The reſt of the paper is given without abridgement, and the author himſelf ſpeaks in the firſt perſon.

† The author refers to his tables in the Tranſactions, of which the abridged reſult has been here given.

Experiment with a ſphere.

I have tried practically the effect of the above ſuppoſitions, by placing ſmall white ſpots on a dark ſphere, which being revolved round repreſented the various changes as nearly as could be expected: proceeding therefore with theſe and other conſiderations, I ſhall make ideal drawings of the ſtar with the ſmall illuminated parts in its atmoſphere, and apply to them ſome of the actual obſervations from both the preceding tables, having always in view that each period may, more or leſs, require a different diſpoſition of ſpots, in conſequence of their conſtant changeability.

1ſt View.

View of the ſtar at its greateſt brightneſs.

Plate XIII. Fig. 1, A B, the ſtar's polar axis, round which its rotation takes place in 62 days from C to D.

C D, its equator, the 360 degrees of which being revolved in 62 days, gives nearly 5¾ degrees for each day's motion; the brighteſt part or ſpot is repreſented as centrally facing us, and accordingly ſhewing the ſtar in its greateſt luſtre. Were this bright ſpot and the other parts to remain *unchangeable*, they would after having completed the revolution of 360 degrees, or 62 days, (the ſtar's rotation on its axis,) appear again as at firſt, and at every return continue to give exact periodical times, as was nearly the caſe in 1799, between Auguſt and October, (ſee Table IX. p. 142); but if the ſpot becomes obſcure, and another brightens up in a different place, this latter will make the ſtar appear at its next full ſplendour either ſooner or later than the real rotation according to its poſition, thus,

2d View.

Anomaly from another bright ſpot cauſing an apparently ſhort period.

Fig. 1. A full brightneſs having been ſhewn by the ſame ſpot, it afterwards loſes its light, and another as bright is produced 5 days motion (or 29 degrees) preceding it at E, ſee Fig. 2. This latter, when turned centrally to the earth, will appear 5 *days ſooner* than the former one, now obſcured, (here marked P.) and ſhow the ſtar at its full luſtre, making the rotation 57 days inſtead of 62, which was the caſe in 1796, the obſerved revolution between September 17 and November 13. (See Table IX.)

3d *View.*

Another cauſing a period apparently long.

Fig. 3. We will now apply a caſe of an interval of too great length, that of 72 days: the ſpot *m* alone having ſhewn us the ſtar in its full luſtre, its light diſappears during the revolution, and another brightens forth ten days (or 58 degrees) *following it* at H; when *m* returns to face us again in 62 days it being obliterated, the ſtar will appear obſcured, and not recover its ſplendour until the new brightened part H becomes central, which being *ten days later* than the poſition in which *m* was ſeen, makes the revolution 72 days inſtead of 62, as was obſerved between July 14 and September 24, 1801. (See Table IX.) In the above caſe the alterations took place while behind the ſtar, otherwiſe ſome irregularities would have been perceived, as will later be noticed. The ſame reaſoning with proper alterations will, I apprehend, account for the other revolutions, yet I ſhall ſoon again reſume the ſubject with regard to a *ſeries* of the greateſt irregularities; at preſent let us proceed to take a few views of the intervals of its *leaſt brightneſs*, which, contrary to my expectation, I find much more difficult to explain than thoſe of the full, although the reſults diſagree leſs among themſelves. The darkened face of the ſtar is here repreſented with a few ſmall changeable bright ſpots, placed in general at a proper diſtance, ſo as to keep up an uninterrupted increaſe and decreaſe of light with regard to us, and are alſo made to correſpond with ſeveral other obſervations.

4th *View.*

Greateſt period explained; between intervals of leaſt brightneſs.

Fig 4 is to explain the greateſt interval of 74 days, between July 4th, and September 16th 1799. (See Table X.) The darkened hemiſphere here exhibited in its *minimum* July 4th, with the following ſpots, *w* nearly gone off, next a ſmall one *l*, then another P of a ſimilar ſize, preceding the centre a day or two, (or a few degrees, (and laſtly a bright one at D, juſt appearing. During the rotation, D loſing its light and the P becoming *much brighter*, the ſtar at its next return in 62 days, when at its firſt poſition, muſt of courſe appear much brighter, (See fig. 5) but by the retiring of *l* and P continues to diminiſh in luſtre till the appearance of ſome large ſpot from the other hemiſphere; which taking place 12 days afterwards, will, (when this time is added to the 62 already revolved) make the revolution of 74 days, as required; for a view of a ſhort interval, for the preſent let that of 56 days be taken between Auguſt 21ſt and October 16th 1801. (See Table X.)

5th View.

Leaſt brightneſs.

The leaſt brightneſs or *minimum* is repreſented by fig. 6, when the bright ſpots *y* and *x* at each extremity of the equatorial diameter are mutually but juſt in ſight and a minute one, *r* alone on its ſurface preceding *y* by 6 days motion: *n n*, are other middling ſized ſpots near *x*, but preceding it; they cannot for the preſent be ſeen, being on the oppoſite or bright hemiſphere. The ſpot *x* during the ſtars revolution having loſt its light, and *r* being conſiderably increaſed, the next *minimum* will be between *n n* and *r*, (inſtead of *x* and *y*.) See fig. 7; and by the retiring of *n n* the *diminution* of the ſtar's light will continue to take place only until the re-appearance of *r*, at the place where *y* was, which being 6 days ſooner than the former poſition, (See fig. 6,) reduces the rotation to 56 days. All the foregoing views are from unconnected periods, where only the ultimate returns of each appearance have been attended to; but now, I ſhall examine a long interval with many intermediate changes, that between June 18th, and September 17th 1796, wherein are included the moſt intricate irregularities and viciſſitudes: theſe obſervations are already pointed at full length in the Philoſophical Tranſactions for 1797, and therefore can at any time be inſpected: indeed, I then little thought they would ever become of further uſe, but that of ſtating facts, to which, however, I have always been very partial, and particularly ſo, after having experienced the advantage of Maraldi's printed obſervations on the variable ſtar in Hydra, as it was partly by them that I aſcertained the periodical returns of brightneſs of that ſtar, and which flattered me the more, as Maraldi himſelf had been leſs ſucceſsful in the attempt; See Phil. Trans. for 1786. Yet in the preſent Paper I have omitted all ſuch details, being aware they might be thought too voluminous, but hope at ſome future time the Society will honour them with a place in their library.

The firſt ſketch, Plate XIV. repreſents, for June 13, 1796, the comparative ſize of the bright ſpots ſuppoſed to ſurround the ſtar, but here extended at full length; the next eight following are

Explanation by the figures of the periodical variations and irregularities of light in the ſtar in Sobieſki's ſhield.

are ſpherical views, on an enlarged ſcale, for each quarterly, rotation or leſs, ſhewing the principal changes, as expreſſed in the adjoining remarks, and correſponding with the obſervations; theſe being taken from my printed paper, as already mentioned, are marked in italics. It will be ſeen that the ſpots by which the changes are principally regulated, are placed at equal diſtances, yet intermediate ones might alſo frequently be inſerted without occaſioning any objection, but that of rendering the explanations more complex.

REMARKS ON PLATE III.

Fig. 2. "*June* 18*th*. *Full brightneſs Mag. bright* 5*th*," before or after which date the ſtar would appear leſs bright, by the ſpot E being removed from the centre, and one of the others out of view.

Fig. 3. "*July* 3*d*, 15 *days or* ¼ *rotation being elapſed ſince June* 18*th*, 5*th Mag, a little decreaſed*" by the removal of the brighteſt ſpot E, the *h* being much leſs.

Fig. 4. "*July* 19*th*, 16 *days or* ¼ *rotation* 5.6 *Mag*. *ſtill decreaſed*," N being much leſs than *h*, now gone off. *A ſlight minimum*."

Fig. 5. "*July* 27*th*, 8 *days af the rotation*, 5 *Mag*. *rather increaſed*" by the conſiderable increaſe of N ſince four days, with the addition of F, *a ſlight full brightneſs*.

Fig. 6. "*Aug*. 3*d*, 7 *days of rotation*, 5,6 *Mag*. *decreaſed* by the going off of N, the E, which is now reappearing, being reduced to much leſs than F.

Fig. 7. "*Aug*. 19*th*, 16 *days or* ¼ *rotation*, 5.6 *Mag*. *again decreaſed*," by the removal of F, by E being much leſs, and by the *h* alſo being conſiderably diminiſhed.

Fig. 8. "*Sept* 3*d*, 15 *days or* ¼ *rotation*, 6 *Mag*. *ſtill more decreaſed*," by the *h* being much leſs than E, which is now going off, and N ſcarcely reappearing, *another minimum*.

Fig. 9. "*Sept*. 17*th*, 14 *days or near* ¼ *rotation*, 5 *Mag*. *full brightneſs conſiderably increaſed*," by N having retained its increaſed brightneſs of July 27, and now facing us centrally.

1ſt, Thus are exhibited, the two ſhort intervals of its full brightneſs, one between June 18 and July 27, of 39 days, and the other between July 27 and Sept. 17, of 52 days. See Table IX.

2dly, The interval of 46 days between the two *minima* of July 19 and Sept. 3; See Table X.

Explanation by the figures of the periodical variations and irregularities of light in the ſtar in Sobieſki's ſhield.

3dly, The long decreaſe of 38 days between July 27 and Sept. 3, and

4thly, The rapid increaſes of 3 and 14 days between the 19 and 27th of July, and the 3d and 17th of September,

As alſo the other intermediate changes, yet I muſt again repeat, particularly as a few days error may occaſionally proceed from the obſervations, that by theſe ſketches it is not meant to give exact drawings of the ſize, diſtances or alterations of the ſpots, but merely to ſhew how the changes may take place, as, I believe, nothing of the kind has hitherto been offered to the public, either with or without corroborating obſervations; nor do I preſume to think, that the explanations are the only ones or beſt can that be imagined, the more ſo, as they ſolely refer (for greater ſimplicity) to the ſtar's equator, while poſſibly, were the ſpots placed in a northern or ſouthern latitude, or permanent ones near the poles, or were a proper inclination, given to the polar axis, they might be more ſatisfactory: however, the materials themſelves, the *obſervations* and *deductions* will I flatter myſelf ever be acceptable, and contribute to facilitate future conjectures, which from an allowable analogy may extend to ſimilar parts of the ſtarry ſyſtem, with regard to the probability of eſtabliſhing whether any of the moſt *irregular or particular* changes may not *return at fixt periods*, or after a certain number of rotations. I think we can entertain but ſlight hopes of it, owing to the *great fluctuation* of the luminous matter, as ſhewn by the *perpetual varying* of the *apparent* revolutions, magnitudes, &c. See Tab. IX. X. and VII. Still it is natural to ſuppoſe, that ſome parts of the atmoſphere of this ſtar may have a leſs tendency than others to become luminous, ſo as to promote at different times, ſimilar appearances; and indeed that is ſtrongly indicated by the *intervals* of the *minima* being far *more regular* than thoſe of the *full brightneſs*, which, with other reaſons induce us to ſuſpect even that one of its hemiſpheres is leſs favourably conſtituted or qualified, than the other for the generating of theſe particles, although they do occaſionally encroach on both ſides, as appears by the obſervations between June and Auguſt, See Phil. Trans. for 1797, or the eight ſketches of 1796, and likewiſe in 1797, ſee Tab. VII. when during *three months* it was only reduced to the

Changeable ſtars may become permanent;

and others diſappear.

There may be dark ſtars,

perhaps equal in number to the viſible ones.

5 or 6 Mag. by which the degree of brightneſs that ſurrounded it, muſt have been nearly equal: had the cauſes of varying its light then ceaſed, it would ever have continued to appear as an unchangeable ſtar of the 5 or 6 Mag. and ſuch is the caſe of ſeveral others that *formerly have been variables*, but for many years retain a ſteady brightneſs, as β Geminorum, δ Urſæ majoris, α Draconis, and perhaps that in the Swan's breaſt, while others, after *ſhewing their changes*, have *entirely diſappeared*, owing to a total abſorption of light, as the famous one in Caſſiopea, in Serpentarius of 1604, that near the Swan's head, and doubtleſs many more. Does not this induce us to preſume that there are alſo others, that have *never ſhewn* a glimpſe of brightneſs? Laſtly, *new variables* may become ſo at different periods, by an unuſual and partial increaſe or diminution of their bright parts, as not unlikely was the caſe of ο Ceti, Algol α Herculis, &c. for theſe ſtars being by times very conſpicuous, their changes, had they been always equally great, might have been eaſily noticed by the ancient aſtronomers, who obſerved only with the *naked eye*. A few lines above, I mentioned the probability that there exiſted *primary* inviſible bodies or *unenlightened ſtars* (if I may be allowed the expreſſion) that have ever remained in *eternal darkneſs*; how numerous theſe may be, can never be known. Would it then be too daring or viſionary to ſuppoſe their numbers equal to thoſe endowed with light? particularly when we take into contemplation the ample ſet of bodies viſible only by reflected rays, that appertain to our own ſyſtem, ſuch as the planets, aſteroides, comets, and ſatellites. Do not theſe, although but of a ſecondary nature, lead us to venture on the foregoing more enlarged conjecture; and moreover to ſuſpect, that the *enlightened ſtars* are thoſe that have already attained the higheſt degree of perfection? granting, therefore, ſuch multitudes do really exiſt, cluſters of them, by being collected together as in the milky-way, muſt intercept all more diſtant rays, and if free from any intervening lights, they would appear as *dark ſpaces* in the heavens, ſimilar to what has been obſerved in the Southern Hemiſphere. That ſo few of theſe obſcure places are perceived, may be attributed to their being obliterated by the preſence either of ſome ſcattered ſtars, or of other ſlight luminous appearances.

Our ſun, though variable, is ſtill very luminous;

I have thus fully inveſtigated the nature of this diſtant ſun, a ſingle one among many millions, and ſcarcely perceptible to the ſight, yet of no leſs importance than our own grand luminary. But ours is ſtill ſupplied abundantly with reſplendent particles, while Sobieſki's variable ſtar has them moſt ſparingly diſperſed over its ſphere: a ſcantineſs that apparently muſt occaſion to its ſurrounding planets, conſtant viciſſitudes of uncertain darkneſs, and repletion of light and heat. How far more enviable ſeems our ſituation; I mean that which we enjoy at preſent; there being ſtrong reaſons to believe, that the ſun's luminous appearance has been at times conſiderably diminiſhed; and I have little heſitation in conceiving that it may alſo be reduced at ſome future period to ſmall patches, and then the apparent irregularities of its periodical rotations, which at preſent are only perceived by the obſervations of trifling dark ſpots, would become evidently conſpicuous, particularly when ſeen at a diſtance as remote as the variable ſtars are from us. But ſuch conjectural flights of fancy cannot too ſoon be dropt. I therefore ſhall conclude with obſerving, that theſe inquiries on the alterations of light of the ſtars have been ſo little diſcuſſed, that it is to be hoped they will not be diſcontinued; and although I have already troubled the Society with many papers concerning ſuch changes, I nevertheleſs propoſe, ere long, having the honour of preſenting them with one more, moſt probably my laſt, on this ſubject.

though it may become reduced in future ages.

EDW. PIGOTT.

VIII.

Account of a Luminous Meteor. By a CONSTANT READER.

To Mr. NICHOLSON.

SIR,

HAVING found it frequently ſtated, that it might be uſeful, ſhould every one who has a fair opportunity of noting with reaſonable accuracy, the courſe and altitude of meteors, deſcribe their appearances as well as they are able; I ſend you the following account to make what uſe of you pleaſe.

Deſcription of the appearance, courſe, and duration of a luminous meteor.

Laſt night (Sunday the 21ſt) paſſing along the Strand, I ſtopped at the door of the Crown and Anchor, the vacant ſpace before it lately cauſed by the pulling down of houſes offering

Deſcription of the appearance, courſe, and duration of a luminous meteor.

offering a conſiderable view of the heavens, at that time ſplendid with ſtars; I was looking with attention towards the N. W. when ſuddenly a meteor from about 35° of height, ſhot from the W. by N. It was apparently about the ſize of a tennis ball, perhaps hardly ſo large, it was followed by a ſtream of light which ſeemed in ſpecks, the length of the train was about a degree, that is about twice the apparent diameter of the moon. Its courſe was from North of Weſt towards the North, paſſing about 10° below the of the Great Bear, which I judge was then about 45° above the horizon. Its motion was majeſtic, by no means rapid, I am ſure it was full ten ſeconds in motion, the light not ſo piercing as that of a ſtar of the firſt magnitude, but exceeded that of the ſecond, with which I had full opportunity of comparing it. It ran through 30° of the heavens, deſcribing an arch of great diameter, its path was convex above, and declining downwards. The extinction of it was at an altitude of about 25° having fallen certainly not more than 10°, I do not think ſo much. It very viſibly ſtopped before it was extinguiſhed. It burſt at laſt with very few ſparks, and its train and itſelf together diſappeared in a moment. I had perfect leiſure and ſpace to obſerve its whole courſe, it expired below the ſecond pointer, I inſtantly drew out my watch, and comparing it this morning with the clock of St. Paul's Cathedral, it was exactly at thirty-one minutes after eleven that I obſerved the end of the phenomenon.

I am, Sir,

Your moſt obedient humble Servant,

A CONSTANT READER.

Monday, July 22d, 1805.

IX.

Precipitation of Platina as a Covering or Defence to poliſhed Steel, and alſo to Braſs. In a Letter from Mr. J. STODART.

To Mr. NICHOLSON.

DEAR SIR,

Platina is taken from its ſolution by ether.

YOU kindly favoured me, by inſerting in the laſt Number of your excellent Journal, an account of a method I have uſed with ſucceſs, for gilding poliſhed ſteel with gold; perhaps it may be worth knowing, that a very ſimilar proceſs may be performed with platina. That metal, in a ſtate of ſolution, is taken up from the acid by agitation with ether, in the way that gold is, though certainly with leſs avidity. The ethereal ſolution of platina afforded by this proceſs, is, like that of gold, depoſited on the ſurface of poliſhed iron, or ſteel, forming a coat of defence from ruſt. It is perhaps a fact of equal importance, that the ſurface of poliſhed braſs is coated with platina by the ſame operation that ſteel is; namely, by plunging the braſs for an inſtant into the ethereal ſolution. As far as I know, theſe facts have not hitherto been noticed: on the contrary, authors highly reſpectable, have from ingenious and well conducted experiments, been led to concluſions very oppoſite to thoſe I have advanced. Dr. Lewis, to whoſe genius and induſtry the arts are much indebted, ſays, "gold is the only one of the known metals which the ether takes from acids; and hence this fluid affords a ready method of diſtinguiſhing gold, contained in acid ſolutions." The ſame author gives the following experiment. "Sulphuric ether was poured into a ſolution of platina, and into a compoſition of platina and gold. The vials were ſtopt and ſhaken, the ether received no colour from the ſolution of platina, but became inſtantly yellow from that of the platina and gold." The only way in which I can account for theſe reſults, ſo contrary to my experience, is by ſuppoſing that the platina with which Dr. Lewis made his experiments, was not ſo pure as that with which we are now furniſhed. What I uſed was part of an excellent malleable bar, its ſpecific gravity I do not exactly know. I am inclined to think it was quite pure. The ether was furniſhed by my friend Mr. Hume, whom I am again happy to thank for his kind and able aſſiſtance. The ethereal ſolution of platina is of a beautiful pale yellow colour, does not at all ſtain the hand, and is precipitated by volatile alcali. The precipitate I have not examined. It may be fulminating, and I have no reliſh for exploſions. The coat of platina on ſteel is of a dull white colour. I have no doubt of its proving quite as good a defence from ruſt, as the coat of gold. It is, however, by no means ſo beautiful; for which reaſon a preference will probably be given to the laſt named metal. I have uſed both the gold and platina in coating different

and coats iron or ſteel;

and alſo braſs.

Dr. Lewis's experiments gave a contrary effect;

probably becauſe his platina was impure.

The platina coating is leſs beautiful than gold.

different parts of the ſame inſtrument. The effect produced by the contraſt of colour is very beautiful. Whether any of theſe obſervations may be worth communicating through the medium of your moſt uſeful Journal, is a queſtion I beg leave to ſubmit entirely to your judgment. I have not tried any of the eſſential oils with ſolution of platina; further experiments will probably be made with theſe metallic ſolutions, by thoſe who have more time, and a better knowledge of theſe ſubjects. Such purſuits, when the reſults are frankly communicated, promiſe to benefit ſcience, and muſt ultimately prove uſeful to ſociety.

I remain with much reſpect,
Dear Sir, your obliged Servant,
J. STODART.

Strand, July 24, 1805.

X.

Experiments on Wootz. By Mr. David Mushett. *From the Philoſophical Tranſactions*, 1805.

(Concluded from Page 204.)

Forging No. 2.

Appearances on forging the cake of wootz, No. 2.

ONE half of this cake was heated to a ſcarlet ſhade, and put under the cutting chiſſel; it was at firſt ſtruck lightly, then reheated, and cut comparatively ſoft; but a ſmall crack had over-run the progreſs of the chiſſel. Its ſoftneſs in cutting was attributed to an evident want of ſolidity. The other half cake felt harder under the hammer, but proved afterwards ſpongy throughout the maſs. In the act of cutting, a looſe pulveriſed matter was diſengaged from ſome of the cells, poſſeſſed of a ſhining appearance.

The fractures obtained in conſequence of the diviſion of the half cakes, preſented a flattiſh cryſtallized appearance, more reſembling very white caſt iron, than ſteel capable of being extended under the hammer. One of the middle cuts was entirely cellular with cryſtallized interiors, and incapable of drawing; the correſponding cut of the other half cake was drawn into a ſtrait bar three quarters of an inch in breadth, and three-eighths thick, but was covered with cracks and flaws from end to end. The colour of the break was one ſhade lighter than No. 1, it tore leſs out, was equally yolky, and poſſeſſed on the whole an aſpect very unſavourable for good ſteel.

The other two outſide quarters were alſo drawn into ſhape, one under the tilt hammer, and the other by hand. Theſe were more ſolid in the fracture, poſſeſſed fewer ſurface-cracks, ſtood a higher degree of heat, tore out more, and exhibited a ſilky gloſſy grain, at leaſt two ſhades lighter in the colour than the centre pieces.

Forging 3d Cake.

Appearances on forging the cake of wootz, No. 3.

One half of this cake, firſt ſubjected to be cut, was found ſofter than any of the preceding, and exhibited no ſymptom of cracking. The other half was cut at three heats, but found looſe and hollow in the extreme. A conſiderable portion of the ſame brilliant powder, formerly noticed, was here again diſengaged. It was carefully taken up for examination, and found to be very fine ore of iron in a pulvereſcent ſtate, very obedient to the magnet, and without any doubt an unmetallized portion of that from which wootz is made.

This curious circumſtance led me to examine every pore and cell throughout the whole fragments. On the upper ſurface of two of them I found ſmall pits containing a portion of the ore, which had been ſlightly agglutinated in the fire, but ſtill highly magnetic. The upper ſurface of the preſent cake, cloſe by the gate or feeder, contained a large pit filled with a ſtratum of ſemi-fuſed ore, ſurmounted by a maſs of vitrified matter, which bore evident marks of containing calcareous earth.

Thoſe who have devoted ſufficient attention to the affinities of iron and earths for carbon, will be ſurpriſed to find that, on this particular ſubject, the rude fabricators of ſteel in Hindoſtan have got the ſtart of our more poliſhed countrymen in the manufacture of ſteel.

Two bars of wootz were formed from this cake, and theſe in point of quality inferior to any of thoſe formerly produced. The appearance of the metal was more varied, leſs homogeneous, and contained more diſtinct laminæ with ruſty ſurfaces, than either of the two former cakes.

It appeared highly probable, from the obſervations that occurred in forging, and in the examination of the cake, that the original proportion of mixture was ſuch as would have formed a quality of ſteel ſofter than No. 1 and 2; but as ſteel of ſuch ſoftneſs requires a greater heat to fuſe it, than when more fully ſaturated with carbonaceous matter, it is probable that the furnace had not been ſufficiently powerful to occaſion complete fuſion of the whole maſs, and generate a ſteel homogeneous in all its parts.

Forging 4th Cake.

Appearances on forging the cake of wootz, No. 4.

Both halves of this cake cut pleaſantly, and with a degree of tenacity and reſiſtance, mixed at the ſame time with ſoftneſs beyond what was experienced in any of the former cakes. Two quarters of this cake were drawn under the tilt hammer, and one by hand. The reſulting bars were nearly perfect. A ſlight ſcale was obſervable upon the bar, from that quarter which contained the figure. The fracture was ſolid, though not homogeneous as to quality and colour, and it appeared pretty evident, that a conſiderable portion of one ſide through the whole bar was in the ſtate of malleable iron, and of courſe not capable of being hardened. It was a ſubject of conſiderable regret, that the cake the moſt perfect and the moſt tenacious of the whole, in the proceſs of forging, ſhould get an imperfection which rendered it uſeleſs for the perfect purpoſes of ſteel.

Forging 5th Cake.

Appearances on forging the cake of wootz, No. 5.

The firſt half of this cake cut uncommonly ſoft for wootz, but by cracking before the chiſſel ſtill exhibited a want of proper tenacity. The next half cut equally ſoft, but with more tenacity. Two quarters of this cake drew readily out under the tilt hammer, and a third was drawn by hand at a bright red, ſometimes approaching to a faint white heat. None of the bars thus obtained were uniformly free from cracks and ſcale, although the fracture exhibited a fair break of a light blue colour, and the grain was diſtinctly marked, and free from yolks.

General Remarks.

Remarks. Wootz appears to be the product of a pecu-

The formation of wootz appears to me to be in conſequence of the fuſion of a peculiar ore, perhaps calcareous, or rendered highly ſo by mixture of calcareous earth along with a portion of carbonaceous matter. That this is performed in a clay or other veſſel or crucible, is equally preſumable, in which the ſeparated metal is allowed to cool; hence the cryſtallization that occupies the pits and cells found in and upon the under or rounded ſurface of the wootz cakes.

liar ore fuſed, and ſuffered to cool in a clay veſſel;

The want of homogenity, and of real ſolidity in almoſt every cake of wootz, appears to me to be a direct conſequence of the want of heat ſufficiently powerful to effect a perfect reduction; what ſtrengthens this ſuppoſition much is, that thoſe cakes that are the hardeſt, *i. e.* that contain the greateſt quantity of carbonaceous matter, and of courſe form the moſt fuſible ſteel, are always the moſt ſolid and homogenious. On the contrary, thoſe cakes, into which the cutting chiſſel moſt eaſily finds its way, are in general cellular, replete with laminæ, and abound in veins of malleable iron.

by an heat not ſufficient for good fuſion;

It is probable, had the native Hindoſtan the means of rendering his caſt ſteel as fluid as water, it would have occurred to him to have run it into moulds, and by this means have acquired an article uniform in its quality, and convenient for thoſe purpoſes to which it is applied.

which is the reaſon why it was not caſt in moulds.

The hammering, which is evident around the feeder and upon the upper ſurface in general, may thus be accounted for. When the cake is taken from the pot or crucible, the feeder will moſt probably be ſlightly elevated, and the top of the cake partially covered with ſmall maſſes of ore and ſteel iron, which the paucity of the heat had left either imperfectly ſeparated or unfuſed. Theſe moſt probably, to make the product more marketable, are cut off at a ſecond heating, and the whole ſurface hammered ſmooth.

I have obſerved the ſame facts and ſimilar appearances in operations of a like nature, and can account ſatisfactorily for it as follows.

The firſt portions of metal, that are ſeparated in experiments of this nature, contain the largeſt ſhare of the whole carbon introduced into the mixture. It follows of courſe, that an inferior degree of heat will maintain this portion of metal in a ſtate of fluidity, but that a much higher temperature is requiſite to reduce the particles of metal, thus for a ſeaſon robbed of their carbon, and bring them into contact with the portion firſt rendered fluid, to receive their proportion of the ſteely principle. Where the heat is languid, the deſcent of the laſt portions of iron is ſluggiſh, the maſs below begins to loſe its fluidity, while its diſpoſition for giving out carbon is reduced by the gradual addition of more iron. An accumulation takes place of metallic maſſes of various diameters, riſing up for half an inch or more into the glaſs that covers the metal; theſe are neatly welded and inſerted into each other, and diminiſh in diameter as they go up. The length, or even the exiſtence of this feeder or excreſcence, depends upon the heat in general, and upon its temperature at different periods of the ſame proceſs. If there has been ſufficient heat, the ſurface will be convex and uniformly cryſtalline; but if the heat has been urged, after the feeder has been formed and an affinity eſtabliſhed between it and the ſteelified maſs below, it will only partially diſappear in the latter, and the head or part of the upper end of the feeder will be found ſuſpended in the glaſs that covers the ſteel.

The ſame or ſimilar phenomena take place in ſeparating crude iron from its ores, when highly carbonated, and difficult, from an exceſs of carbon, of being fuſed.

The diviſion of the wootz cake by the manufacturers of Hindoſtan, I apprehend is merely to facilitate its ſubſequent application to the purpoſes of the artiſt; it may ſerve at the ſame time as a teſt of the quality of the ſteel.

Experiments to aſcertain the comparative meaſure of carbon in wootz, by the quantity of lead it reduces from flint glaſs.

To aſcertain, by direct experiment, whether wootz owed its hardneſs to an extra quantity of carbon, the following experiments were performed with various portions of wootz of common caſt ſteel, and of white crude iron, premiſing that in operations with iron and its ores, I have always found the comparative meaſure of carbon beſt aſcertained by the quantity of lead which was reduced from flint glaſs.

1ſt Cake.	Grains.
Fragments of wootz - - - - -	65
Pounded flint glaſs three times the weight -	195

This mixture was expoſed to a heat of 160° Wedgwood, and the wootz fuſed into a well cryſtallized ſpherule of ſteel. A thin cruſt of revived lead was found below the wootz, which weighed 9 grains, or $\frac{138}{1000}$ the weight of wootz.

2d *Cake*

2d *Cake.*

	Grains.
Fragments of wootz - - - - - -	80
Flint glaſs, ſame proportion as above - -	240

The fuſion of the mixture in this experiment was productive of a maſs of lead weighing 10 grains, equal to ⅛th the weight of the wootz.

Experiment to aſcertain the comparative meaſure of carbon on wootz, by the quantity of lead it reduces from flint glaſs.

3d *Cake.*

Fragments of wootz - - - - -	75
Flint glaſs - - - - - - -	225

The maſs of lead precipitated beneath the ſteel in this experiment, amounted to 9 grains, or $\frac{12}{100}$ the weight of the wootz employed.

4th *Cake.*

Fragments of wootz - - - - - -	93
Flint glaſs - - - - - - - -	279

Lead obtained, precipitated from the glaſs by means of the carbon of the wootz 14½ grains, equal to $\frac{156}{1000}$ the weight of the wootz.

5th *Cake.*

Fragments of wootz - - - - - -	69
Flint glaſs - - - - - - -	207

The lead revived in this experiment amounted to 7 grains, which is equal to $\frac{102}{1000}$ the weight of the wootz.

6th. *Caſt Steel formed with $\frac{1}{30}$th part of its Weight of Carbon.*

Fragments - - - - - - - -	90
Cryſtal glaſs - - - - - -	270

Lead revived 8½ grains equal to $\frac{94}{1000}$ the weight of the ſteel introduced.

7th. *White caſt Iron dropt while Fluid into Water.*

Fragments - - - - - - - -	103
Cryſtal glaſs - - - - - -	309

The fuſion of this precipitated 23½ grains of lead which is equal to $\frac{228}{1000}$ the weight of the caſt iron.

Recapitulation of these Experiments.

Recapitulation of the experiments.

1ſt cake of wootz revived of lead	,139
2d ditto	,125
3d ditto	,120
4th ditto	,156
5th ditto	,102
Steel containing $\frac{1}{60}$ of its weight of carbon	,094
Caſt iron	,228

Wootz contains more carbon than ſteel does, and leſs than caſt iron.

It would appear to reſult from theſe experiments, that wootz contains a greater proportion of carbonaceous matter, than the common qualities of caſt ſteel in this country, and that ſome particular cakes approach conſiderably to the nature of caſt iron. This circumſtance, added to the imperfect fuſion which generally occurs in the formation of wootz, appear to me to be quite ſufficient to account for its refractory nature, and unhomogeneous texture.

Its ore is probably very excellent.

Notwithſtanding the many imperfections with which wootz is loaded, it certainly poſſeſſes the radical principles of good ſteel, and impreſſes us with a high opinion of the ore from which it is formed.

The poſſeſſion of this ore for the fabrication of ſteel and bar iron, might to this country be an object of the higheſt importance. At preſent it is a ſubject of regret, that ſuch a ſource of wealth cannot be annexed to its capital and talent. Were ſuch an event practicable, then our Eaſt India Company might, in their own dominions, ſupply their ſtores with a valuable article, and at a much inferior price to any they ſend from this country.

XI.

A Memoir on the Webs of Spiders. By C. L. CADET.*

On the natural and medical hiſtory of ſpiders.

SPIDERS have often excited the curioſity of naturaliſts and the attention of phyſicians. The former have ſucceſsfully ſtudied the habits and conduct of theſe inſects; and notwithſtanding the repugnance they naturally inſpire, theſe accounts have become intereſting, from the induſtry with which they extend their webs for ſeizing their prey, and from obſervations on the multiplicity and arrangement of their eyes, which are geometrically diſpoſed on a motionleſs head, in a manner conformable to their neceſſities. Their combats, the ſingularity of their amours, their ſenſibility for muſic, and their patience, all conſtitute ſubjects of wonder in the hiſtory of ſpiders. Phyſicians have examined whether their bite be really venomous, as is generally thought; and they have found only two ſpecies productive of danger, namely, the tarantula and avicularia of Cayenne. Swanmerdam, Roſſi, and Baglivi have left us little to wiſh for in this matter, as the effects of their bite and the remedies are both known.

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* Abridged from the Journal de Phyſique, LVIII. 463.

Spiders' webs a popular remedy for ſlight wounds.

The webs of ſpiders are conſidered by the common people as a remedy for wounds; country people often apply them on cuts or ſlight wounds, and apparently with ſucceſs. This property was not of ſufficient importance to induce chemiſts to analyſe the material; but as there has alſo been attributed to them a febrifuge virtue, ſuperior in ſome circumſtances to the bark, I have thought them entitled to a more particular examination. The following extract is taken from the *Journal d'Economie Rurale*, for Germinal, in the year XII.

Narrative of fevers cured by ſpiders internally taken.

" We have ſeen upwards of thirty years ago, a good prior, the curate of Batheren in Franche Comté, cure all the fevers of his pariſh, and of the neighbouring villages, by pills of a ſtrange compoſition. He went into his barn and formed ſmall pills with ſpiders' webs, by rolling them between his hands in the ſtate he found them. He adminiſtered this remedy to his patients in white wine, and very ſeldom failed to cure. M. Marie de St. Urſin being chief phyſician of the Hotel de Dieu, of Chartres, treated a very obſtinate fever in that hoſpital. He had employed bitters, the bark, and all the remedies of medical art without ſucceſs, when one of the female attendants offered to undertake the caſe with a certainty of cure. When ſhe was interrogated concerning her remedy, ſhe refuſed to mention it. M. de St. Urſin therefore continued to attend his patient for ſome days; after which, having a good opinion of the attendant, he determined to put his patient under her care. There was no return of the fever after the firſt doſe of the remedy. The phyſician ſuppoſed that the imagination of the patient, his confidence in a new remedy, and particularly the ſecrecy, might have ſuſpended the attack; and he waited, but to no purpoſe, for its return. The attendant encouraged by her ſucceſs, conſented to mention the remedy, which proved to be the ſame as that of the curate of Batheren."

Suppoſed to be effected by gelatine.

The editor of the Journal here quoted, being ſtruck with the new experiments of Seguin upon gelatine applied to the treatment of intermitting fevers, ſuſpects that ſpiders' webs may contain a principle reſembling animal jelly. The experiments of Cadet, while they overthrow this ſuppoſition, appear to him entitled to the attention of medical men.

Analyſis of ſpiders' webs.

Experiment 1. Spiders' webs triturated in the cold with quick-lime, emit a ſlight ammoniacal ſmell. 2. Cold water by digeſtion on the webs, becomes of a red-brown colour; is ſlightly precipitated by infuſion of nut-galls; is precipitated by acids; and this precipitate is again diſſolved when the acids are ſaturated with ammonia. 3. Spiders' webs cleaned as much as poſſible from duſt and foreign matters, were boiled in diſtilled water. The decoction ſmelled like champignons, and lathered by agitation. The undiſſolved matter was boiled in additional waters, until it gave out nothing more. All theſe waters being put together and evaporated, let fall their contents in ſucceſſive pellicles; and at length, by gentle evaporation, a ſolid extract was had, nearly equal to half the weight of the ſpiders' webs. 4. The reſidue not diſſolved in boiling water, was digeſted in alcohol. It gave a very deep orange-coloured tincture, which did not lather. Water being added, threw down a grey flaky precipitate, of a brown colour when dry, and little more than one hundred and ſeventieth part of the original webs. On hot coals it ſwelled up, ſmoaked, and took fire; and from its habitudes in theſe reſpects, and with the alkalies, it reſembled a reſin. The diluted alcoholic ſolution being then evaporated, afforded a reſidue ſlightly deliqueſcent, of a taſte at firſt ſweetiſh, and afterwards bitter, and in quantity nearly three times that of the reſinous precipitate. 5. The inſoluble reſidue after this treatment with water and alcohol, burned without ſwelling up, and emitted a ſmall quantity of white fumes having the ſmell of burned wood. Neither the oxigenated muriatic, nor the ſulphureous acids, diſcoloured it. It was ſoluble with efferveſcence in muriatic acid, which took up two-thirds and

left

Analyſis of ſpiders' webs.

left a black paſte. Ammonia ſeparated a brown matter in ſmall quantity from the clear ſolution; and this matter, when calcined, did not loſe its colour. It was almoſt totally ſoluble in muriatic acid, and this ſolution gave a black precipitate with nut-galls, and a blue with alkaline pruſſiate. The fluid to which the ammonia had been added, gave a grey precipitate by pot-aſh. This retained its colour when ignited, and was again ſoluble in muriatic acid with effervеſcence. 6. Cauſtic pot-aſh poured on the reſidue of ſpiders' webs previouſly treated with water and alcohol, diſengages a little ammonia, and partly diſſolves the matter. An acid throws down from this ſolution a black pulvurelent taſteleſs precipitate, which ſlightly puffs up by heat, and leaves by deſiccation a brittle and apparently reſinous matter. Its quantity is about one-twelfth of the exhauſted matter made uſe of. It is partly ſoluble in volatile oils.

7. The aqueous extract of No. 3 being digeſted with alcohol, gave out one-ſeventh part. This alcoholic extract was brown, conſiderably deliqueſcent, and of a ſharp taſte. It ſwelled conſiderably on the coals, and at a certain period it burned rapidly, as if a nitrate were preſent. It effervеſced briſkly with ſulphuric acid, giving out a white vapour of a muriatic ſmell. Potaſh and lime diſengaged from this extract a ſtrong ammoniacal ſmell, and the vapours were very ſenſible on the approach of muriatic acid. The extract having been incinerated, appeared by ſeveral experiments to contain muriate of lime and a ſulphate. What remained of the aqueous extract after treatment with alcohol, was leſs deep in colour than before, had a purverelent appearance, and ſlightly pungent taſte. On hot coals it did not ſwell up, but left a very abundant precipitate. Strong ſulphuric acid poured on this extract produced no ſenſible ſmell, and there was no production of ammonia when it was triturated with quick-lime.

8. Spiders' webs ſubjected to deſtructive diſtillation, gave firſt water ſlightly coloured, but becoming deeper as the proceſs went on; and afterwards a black thick oil, with carbonated hydrogen and carbonic acid. A very ſenſible ſmell of ammonia was developed, and a reſidual coal was left, amounting to half the matter employed. The coal after incineration left two-thirds of its weight, half of which was taken up by muriatic acid, and the remainder ſeemed to be ſilex and coally

matter,

Analyſis of ſpiders' webs.

matter. The muriatic ſolution, during evaporation, depoſited ſulphate of lime. When ſpiders' webs were incinerated in an open veſſel, the aſhes were found to contain ſulphate of lime, muriate of ſoda, and carbonate of ſoda. Muriatic acid applied to the reſidue took up more ſulphate of lime; and when this ſolution was treated with ammonia and afterwards with pot-aſh, it gave oxide of iron, a little alumine, and ſome lime. The undiſſolved part was ſilex.

9. Spiders' webs were almoſt totally diſſolved in nitric acid amounting to ſix times their weight; carbonic acid and nitrous gas being diſengaged. The ſolution when evaporated let fall cryſtals of ſulphate of lime, and by continuing the evaporation, the yellow, bitter, deliqueſcent matter, which Welter calls *amer*, was afforded.

Component parts.

Hence the author concludes that ſpiders' webs are compoſed of, 1. A brown extract ſoluble in water, and not changeable in the air; 2. A reſinous extract ſoluble in alcohol, and very deliqueſcent; 3. A ſmall quantity of alumine; 4. Sulphate of lime; 5. Carbonate of ſoda; 6. Muriate of ſoda; 7. Carbonate of lime; 8. Iron; 9. Silex. The author thinks that the earths and earthy ſalts may be derived from the local ſituation of theſe inſects, and that it is probable that the webs of garden ſpiders may not afford them. The two conſtant products to which he demands particular attention, are thoſe obtained from the aqueous and alcoholic ſolutions. He thinks it deſirable to try their medical powers ſeparately. He ſuppoſes the reſinous matter to be the ſame ſubſtance as under other circumſtances forms the ſpiders' ſilk, and the wax which Mr. Accum has elſewhere mentioned as one of their products.

XII.

Information on the Mines and Manufactures of the Eaſt Indies, and other Subjects. By J. MACHLACHLAN, *Eſq. of Calcutta.*[*]

SIR,

Account of ſome receipts for dying.

SHOULD you think the encloſed receipts for dying the beautiful reds of the Coromandel coaſt can be of any uſe to the dyers of the united Britiſh kingdom, be pleaſed to lay them before the Society for the Encouragement of Arts, &c. that they may be publiſhed in the volume of their Tranſactions; if not, I truſt you will excuſe my troubling you with them. They were ſent to me from Madras by a ſcientific friend, who had the ſeveral operations, detailed in them, performed in his own preſence. I forwarded a copy of them, and a ſmall quantity of the ingredients mentioned in them, to a friend at home, ſeveral years ago; but he dying about or ſoon after the time of their arrival, I never learned what became of them. It ſtrikes me, however, that there is a conſiderable coincidence between the thread proceſs and that which I have ſeen recommended by Mr. Henry, of Mancheſter, for dying the Adrianople or Turkey red.

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[*] Soc. Arts, 1804; for which the ſilver medal was given.

Immenſe quantities of talc found in the hills in Bahar.

I am not certain whether it is known at home, that many of the hills in Bahar, and other parts of India, contain immenſe quantities of mica, talc, or Muſcovy glaſs. The natives of this country and China make very ſplendid lanterns, ſhades, and ornaments of it, tinged of various fanciful colours; and it is alſo uſed by them in medicine. When burned or calcined, it is, I am told, conſidered as a ſpecific in obſtinate coughs and conſumptions. When powdered, it ſerves to ſilver the Indian paper, &c. uſed in letter-writing; and, in fact, it is applied to numberleſs purpoſes. The bazar price of that of the beſt quality, ſplit into ſheets of about two lines thick, is ſix rupees the maund of 84 lb. avoirdupois. If it could be applied to any uſeful purpoſe at home, it might go in part ballaſt of ſhips, and at a trifling expenſe. I encloſe a ſmall ſpecimen of it, and am,

Sir,

Your very obedient ſervant,

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

N. B. The chaya, or red dye root of the Coaſt, is, I believe, known at home: as alſo the caſhaw leaves, which are uſed as an aſtringent.

CHARLES TAYLOR, Eſq.

Directions

Directions for dying a bright Red, four Yards of ¼ broad Cotton Cloth.

Inſtructions for dying cottons bright red by the Indian method.

1ſt. The cloth is to be well waſhed and dried, for the purpoſe of clearing it of lime and congee, or ſtarch, generally uſed in India for bleaching and dreſſing cloths; then put into an earthen veſſel, containing twelve ounces of chaya or red dye root, with a gallon of water, and allow it to boil a ſhort time over the fire.

2d. The cloth being taken out, waſhed in clean water, and dried in the ſun, is again put into a pot with one ounce of myrabolans, or galls coarſely powdered, and a gallon of clear water, and allowed to boil to one half: when cool, add to the mixture a quarter of a pint of buffalo's milk. The cloth being fully ſoaked in this, take it out, and dry it in the ſun.

3d. Waſh the cloth again in clear cold water, and dry it in the ſun; then immerſe it into a gallon of water, a quarter of a pint of buffalo's milk, and a quarter of an ounce of the powdered galls. Soak well in this mixture, and dry in the ſun. The cloth, at this ſtage of the proceſs, feeling tough and hard, is to be rolled up and beetled till it becomes ſoft.

4th. Infuſe into ſix quarts of cold water, ſix ounces of red wood ſhavings, and allow it to remain ſo two days. On the third day boil it down to two-thirds the quantity, when the liquor will appear of a good bright red colour. To every quart of this, before it cools, add a quarter of an ounce of powdered alum; ſoak in it your cloth twice over, drying it between each time in the ſhade.

5th. After three days waſh in clean water, and half dry in the ſun; then immerſe the cloth into five gallons of water, at about the temperature of 120 degrees of Fahrenheit, adding 50 ounces of powdered chaya, and allowing the whole to boil for three hours; take the pot off the fire, but let the cloth remain in it until the liquor is perfectly cool; then wring it gently, and hang it up in the ſun to dry.

6th. Mix intimately together, by hand, about a pint meaſure of freſh ſheep's dung, with a gallon of cold water, in which ſoak the cloth thoroughly, and immediately take it out, and dry it in the ſun.

7th. Waſh the cloth well in clean water, and ſpread it out in the ſun on a ſand-bank (which in India is univerſally preferred to a graſs-plat) for ſix hours, ſprinkling it from time to time, as it dries, with clean water, for the purpoſe of finiſhing and perfecting the colour, which will be of a very fine bright red.

Instructions for dying cottons bright red by the Indian method.

J. MACHLACHLAN.

Calcutta, Oct. 4, 1803.

Charles Taylor, Eſq.

Directions for dying of a beautiful red, eight ounces of Cotton Thread.

1ſt. Put one gallon and a half, by meaſure, of ſap-wood aſhes, into an earthen pot, with three gallons of water, and allow the mixture to remain twenty-four hours to perfect it for uſe.

2. Put the following articles into an earthen pot, viz. Three-quarters of a pint of Gingelly oil; one pint, by meaſure, of ſheep's dung, intimately mixed by hand in water; two pints of the above ley.—After mixing theſe ingredients well, pour the mixture gradually upon the thread into another veſſel, wetting it only as the thread, by being ſqueezed and rolled about by the hand, imbibes it, continuing to do ſo until the whole is completely ſoaked up, and allow the thread to remain in this ſtate until next day.

3d. Take it up, and put it in the ſun to dry; then take a pint and a half of aſh-ley, in which ſqueeze and roll the thread well and allow it to remain till next day.

4th. Squeeze and roll it in a like quantity of aſh-ley, and put it in the ſun to dry; when dry, ſqueeze and roll it again in the ley, and allow it to remain till next day.

5th. Let the ſame proceſs be repeated three or four times, and intermit till next day.

6th. Ley the thread once, as the day before, and, when well dried in the ſun, prepare the following liquor: One gill of Gingelly oil; one pint and a half of aſh-ley.—In this ſqueeze and roll the thread well, and leave it ſo till next day.

7th. Repeat the proceſs of yeſterday, and dry the thread in the ſun.

Inſtructions for dying cottons bright red by the Indian method.

8th. The ſame proceſs to be repeated.

9th. Firſt repeat the aſh-ley proceſs three or four times, as under the operations 3, 4, and 5, and then prepare the following mixture: On pint of ſheep-dung water; one gill of Gingelly oil; one pint and a half of aſh-ley.—In this ſqueeze and roll the thread well, and dry it in the ſun.

10th. Repeat the ſame proceſs.

11th. Do. Do.

12th. Do. Do.

13th. Do. Do.

14th. Do. Do.

15th. Waſh the thread in clean water, and ſqueeze and roll it in a cloth until almoſt dry; then put it into a veſſel containing a gill of powdered chaya root, one pint by meaſure of caſhan leaves, and ten pints of clear water; in this liquor ſqueeze and roll it about well, and allow it to remain ſo till next day.

16th. Wring the thread, and dry it in the ſun, and repeat again the whole of the 15th proceſs. leaving the thread to ſteep.

17th. Wring it well, dry it in the ſun, and repeat the ſame proceſs as the day before.

18th. Do. Do.

19th. Do. Do.

20. Wring and dry it in the ſun, and with the like quantity of chaya root in ten pints of water, boil the thread for three hours, and allow it to remain in the infuſion until cold.

21ſt. Waſh the thread well in clear water, dry it in the ſun, and the whole proceſs is complete.

J. MACLACHLAN.

Calcutta, Oct. 4, 1803.

SCIENTIFIC NEWS.

Imperial Academy of Sciences at Peterſburgh.

Priſe of the Ruſſian Imperial Academy.

THE Vice Admiral Tchitchagoff, Miniſter of the Marine department, has forwarded to the Academy a queſtion on the reſiſtance of fluids, and its application to naval architecture, for the ſolution of which that department will beſtow a reward of 1000 ducats of Holland, or 462*l.* 10*s.*

The academy being deſirous of ſeconding the patriotic views of the marine department, decided on the publication of a program in the following terms.

Prize propoſed by the department of the Marine, on the 1ſt of *July*, 1804.

The ſubject. To improve the theories of the reſiſtance of fluids;

It is propoſed, that of the two theories of the reſiſtance of fluids propoſed and applied to naval architecture by Don G. Juan, in his *Examen Maritime*, and by M. Romme, in his *Art de la Marine*, one or the other of them, for example, that of Don Juan ſhould be corrected and improved to ſuch a degree, as to afford reſults that ſhall differ from the reſults of experiment, by ſo ſmall a quantity, as may be practically neglected without ſenſible error:—Or otherwiſe, if theſe theories cannot be corrected, it is propoſed, that a new theory ſhould be eſtabliſhed and applied to naval architecture, which ſhall lead to concluſions of the ſame degree of accuracy;—Or otherwiſe, laſtly, if it ſhould be impoſſible to eſtabliſh ſuch a theory, it is propoſed, that from experiments at leaſt, there ſhould be deduced a formula reſembling thoſe which have been given by Meſſrs. Boſſut and Prony; and ſuch that it ſhall be not only more conformable to experiments than thoſe formulas, but that it ſhall lead as nearly as poſſible to the concluſions drawn from experiments, even when the formule ſhall be applied to naval architecture.

or to eſtabliſh a new theory;

or to deduce a formula from experiment.

One thouſand ducats, or 462l. 10s.

For the ſatisfactory ſolution of this problem the department of the marine has appointed a prize of 1000 Ducats of Holland, and has fixed a term of two years to be reckoned from the date of this program. After the expiration of that term, no memoirs addreſſed to the academy will be received on this ſubject, the time appointed being ſufficient for thoſe new experiments which the ſolutions in queſtion render indiſpenſably neceſſary. The memoirs forwarded to the academy muſt be written in a diſtinct legible character, either in the French Engliſh or Ruſſian language.

(Signed in the original) PAUL TCHITCHAGOFF.

Terms and conditions for receiving memoirs.

In the invitation accompanying this program, the Academy requires men of ſcience who intend to make application for this prize, to addreſs their memoirs to the perpetual ſecretary of that body, before the 1ſt of July, 1806, and that the writer ſhould clear the poſt charges as far as the regulations of their reſpective countries will allow. The cuſtomary mode of marking the memoirs with a device or motto, and ſending at the ſame time a ſealed letter, having the ſame device, and containing the name and reſidence of the author, is alſo to be adopted in the preſent inſtance. The memoirs will be examined before the expiration of the term of concurrence, by the Department of the Marine and by the Academy; the latter of whom will publiſh the judgment they ſhall adopt, and the Department of the Marine will crown by the payment of the prize, the labours of that author who ſhall have ſatisfied the conditions of the program.

Prize concerning Light, propoſed by the Imperial Academy of Sciences for the Year 1806.

Prize concerning light.

The uſual theories that light is projected matter; or mere undulation of a fluid;—

—or it is a chemical principle.

Five hundred roubles, £.112 10 0

After an introduction, in which a conciſe ſtatement is given of two theories reſpecting the nature of light, the one aſcribed to Newton, which ſuppoſes light to conſiſt in the emanations of matter from luminous objects, and the other aſcribed to Euler, which deduces the effects from vibrations of a peculiar elaſtic fluid,—they proceed to ſtate rather more fully a chemical hypotheſis of Lavoiſier, who not only conſiders light as cauſed by a peculiar matter, but alſo that this matter is ſubject to the elective attractions, ſo as by its combinations and diſengagements to produce an extenſive ſeries of phenomena, which are thus accounted for. It is principally with a view to develope this laſt hypotheſis that they have propoſed a prize of 500 roubles (£112 10 0.)

The queſtion.

"For the moſt inſtructive ſeries of new experiments on light, conſidered as matter; on the properties which may with juſtice be aſcribed to it; on the affinities which it ſhall appear to have with other bodies, whether organic or inorganic, and on the modifications and phenomena which are manifeſted in thoſe ſubſtances by virtue of the combinations in which the matter of light may have entered along with them."

Remarks by the academy.

After propoſing this queſtion, the Academy proceeds to explain, by obſerving that without entering into any hiſtoric diſcuſſion, or the objections which have been oppoſed to this hypotheſis, nor the reſearches already made with a view to develope traces of chemical action between light and bodies in the different modifications of natural phenomena,—the enquiries here propoſed may not be unuſefully extended to the galvanic fire, of which the dazzling brilliancy when large piles are made to act upon coally matters, in ſome reſpect imitates the ſolar light. The Academy has choſen to enunciate the ſubject of their prize in a general way, in order that philoſophers may not be in any reſpect impeded as to the points of view from which they may be diſpoſed to contemplate and to treat ſo difficult a ſubject, which has ſcarcely yet been entered upon; though ſo eminently worthy of attention from the cultivators of natural ſcience.

Conditions and terms.

The memoirs are to be written either in Ruſſian, French, Engliſh, German, or Latin, and forwarded to the perpetual ſecretary of the Academy, ſealed up, with device and indicatory billet, as mentioned with regard to the former prize. No memoirs will be received after the 30th of April, 1806, incluſive, and the author of that memoir which in the judgment of the Academy ſhall have merited the prize, ſhall be proclaimed in the public meeting of the following month of July. The ſucceſsful memoir becomes the property of the Academy, and muſt not be printed without their formal permiſſion. The other treatiſes will be delivered to the reſpective authors, on application to the ſecretary, either perſonally or by procuration.

Voyages of Meſſrs. HUMBOLDT *and* BONPLAND.

Voyages of Humboldt and Bonpland.

Meſſrs. Levrault, Schoell and Company circulated at the beginning of the preſent year a proſpectus of the voyage of Meſſrs. Humboldt and Bonpland, the publication of which is committed to them: "the travellers, they obſerve, have in general re-written all their obſervations, whatever might have been the object, in the narrative of their voyage. Mr. Humboldt has thought it proper to follow another courſe, and to treat

Humboldt's travels.

treat ſeparately all thoſe objects which conſiderably differ in their nature. He has determined accordingly to publiſh in detached collections, all that more particularly belongs to Aſtronomy, Geology, Botany, Zoology, &c. and his voyage, properly ſo called, will embrace all that relates to General Phyſics, the Origin of Nations, their Manners, their Intellectual Culture, Antiquities, Commerce, and Political Economy. Upon this part of his obſervations and the hiſtory of his voyage, he will not at preſent publiſh more than a narrative, under the title of *Relation abrégé*, &c. or an abridged Relation of a Voyage to the Tropics, performed in the interior of the new continent during the years 1799, 1800, 1801, 1802, and 1803.

It is agreed by Meſſrs. Humboldt and Bonpland, who are connected by the moſt intimate ſentiments of friendſhip, and have ſhared together in all the fatigues and dangers of this voyage, that the whole of their publications ſhall be in their joint names. The preface to each work will announce to which of them the ſeveral parts are reſpectively to be aſcribed. The liſt of works ſpeedily to appear are as follows:

1. The abridged Relation of the Voyage, in quarto; promiſed in July, 1805.

2. A Collection of Aſtronomical Obſervations and Admeaſurements made on the new continent; promiſed in 1805.

3. An Eſſay on the Geography of Plants, or a Philoſophical Sketch of the Equinoctial Regions; founded on obſervations made from the 18th degree of ſouth latitude, in the years before mentioned, with one large plate, coloured; promiſed in June, 1805.

4. Equinoctial Plants, collected in Mexico and the Iſle of Cuba, in the Provinces of Caracas, Cumena and Barcelona, on the Andes of New Grenada, Quito and Peru, and on the banks of Rio Negro, Oroonoko and the River of Amazons, with plates engraved by *Sellier*, in folio; the firſt number to appear in April, 1805.

5. A Collection of Obſervations of Zoology and comparative Anatomy, made in a Voyage to the Tropics; with plates engraved by *Bouquet*, coloured or not, at the option of the purchaſer. The firſt number to appear in May.

N.B. All theſe works will bear the general title of *Voyage de M. M. Alexandre de Humboldt et Aimé Bonpland*, and will form a collection of the ſame ſize and type, except the Equinoctial Plants, which are larger. Subſequent notices of the price and publication are to appear in the journals.

Fiſh ejected from Volcanoes.

Volcanic eruption of fiſhes.

AMONG the great number of facts which Humboldt has collected in his voyage, the following lately communicated to the National Inſtitute is very curious. Several volcanoes of the Cordilleras of the Andes occaſionally throw out eruptions of mud mixed with large volumes of freſh water, and what is moſt remarkable, an infinite number of fiſhes. The volcano of Imbaburo, among others, threw out at one time ſo great a number near the town of Ibarra, that their putrefaction occaſioned diſorders. This phenomenon, aſtoniſhing as it appears, is not even extraordinary, but, on the contrary, of conſiderable frequency, ſo that the facts are authentically preſerved in the public regiſters, along with thoſe of earthquakes. It is more particularly ſingular that theſe fiſh are not at all injured, though their ſtructure is very ſoft. They do not even appear to have been expoſed to a high temperature; for the Indians aſſert that they ſometimes arrive at the foot of the mountain ſtill living. Theſe animals are ſometimes thrown out of the mouths of the crater and ſometimes through lateral clefts; but always at the height of 12 or 1300 toiſes or fathoms above the ſurrounding plains. Humboldt thinks they are produced in lakes ſituated at that height within the crater; and it is a confirmation of this opinion that the ſame ſpecies are found in the brooks which run at the foot of the mountains. It is the only ſpecies which ſubſiſts at the height of 1400 toiſes. It is a new ſpecies to naturaliſts. Humboldt made a drawing of it on the ſpot, and gave it the name of *Pimelodrus Cyclopum* or *thrown by the Cyclops*. It will be found in the firſt number of his Zoology.

Water formed by Mechanical Preſſure.

Combuſtion of oxygen and hydrogen by mechanical preſſure.

IN a ſitting of the French National Inſtitute at the commencement of the preſent year, M. Biot read a note on the formation of water by mere compreſſion. The experiment of forming water out of its component parts oxygen and hydrogen by burning thoſe gaſes by the electric ſpark is well known. M. Biot has ſucceeded in determining this combination independently of electricity, by rapidly compreſſing a mixture of the two gaſes included in the ſyringe of an air-gun. The compreſſion which forces the particles of gas together, cauſes them to give out a ſufficient quantity of heat to ſet them on fire. Some caution is requiſite in repeating this experiment, which is not without danger. Out of three times that M. Biot made it, there were two in which the braſs cap of the pump and the barrel itſelf, which was iron, were broken by the exploſion.

Malleability of Zinc.

Zinc is malleable while heated between 210° and 300°.

A VERY curious and uſeful diſcovery has been made by Meſſrs. Charles Hobſon and Charles Sylveſter, both of Sheffield, that zinc is in fact a malleable metal. The laminability of this metal to a certain conſiderable degree, has long been known; but it was not ſuſpected that it is capable of being forged and drawn into wire. They have found that at a temperature between 210° and 300° of Fahrenheit zinc yields to the hammer, and alſo that it may be wire-drawn or laminated by keeping it at this temperature during the mechanical operation. An oven or a hollow metallic veſſel kept at a due heat may be uſed for the pieces, in the ſame manner as the Smith's forge is uſed for iron and ſteel. It appears that the zinc, after having been thus annealed and wrought, continues ſoft, flexible and extenſible, and does not return to its former partial brittleneſs, but may be bended and applied to the uſes for which zinc has hitherto been thought unfit, ſuch as the fabrication of veſſels, the ſheathing of ſhips, and numerous other important applications. I have ſeen a chaſed or ſtamped figure raiſed at one ſtroke in thin zinc; which is, I think, as much elevated as it could have been in copper.*

Palladium.

Palladium.

BY a letter from Meſſrs. Knight, of Foſter-lane, I am informed that the new metal, palladium, may be purchaſed at their warehouſe.

Erratum corrected.

MR. GREGORY begs leave to correct a miſtake which occurs in his paper on horſe-powers, occaſioned by his inadvertently copying from ſome of his former obſervations on that ſubject the fraction $\frac{2}{4}$ inſtead of $\frac{2}{3}$ for the approximate value of the exponent n: this makes a change in the reſults of the computation at page 148; for $(9-3)^{\frac{2}{3}} : (9-4.4)^{\frac{2}{3}} :: 130 : 80{\cdot}58$, or nearly $80\frac{1}{2}$ lb. inſtead of $71\frac{1}{2}$, as there given. He alſo points out a preſs error at p. 153, where the ſymbols of multiplication in each of the theorems ſhould have been ſigns of addition.

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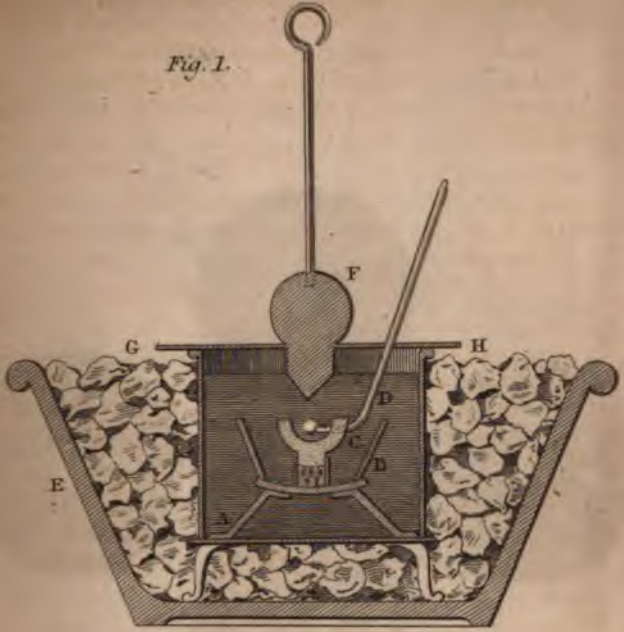
* The inventors have obtained a patent.

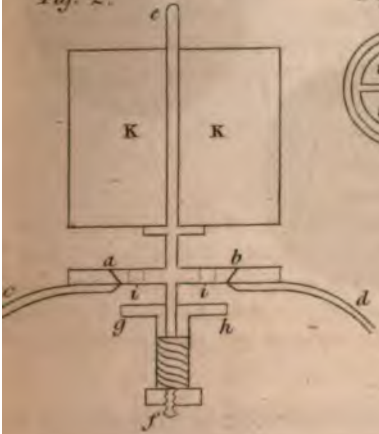
New Experiments by Count Rumford to prove the maximum of Density in Water above 32° fahr.

Fig. 1.



Chevalier Edelcrantz's Valves for Steam Engines.

Fig. 2.



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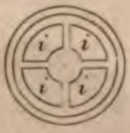
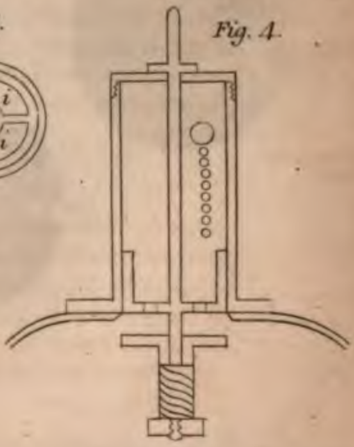


Fig. 4.



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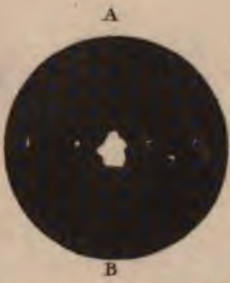
Variable Star in Sobieski's Shield.

Fig. 1.



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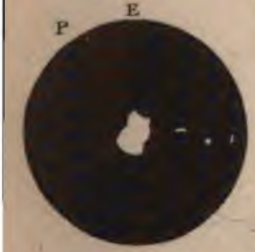

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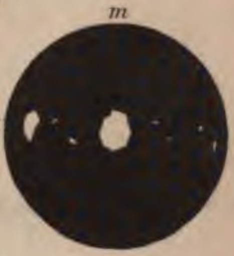


Fig. 4.

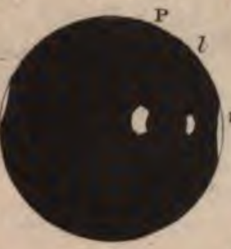


Fig. 5.

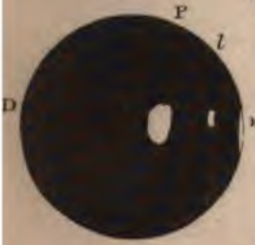


Fig. 6.

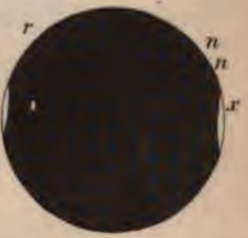
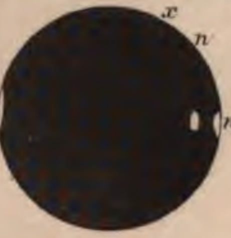


Fig. 7.



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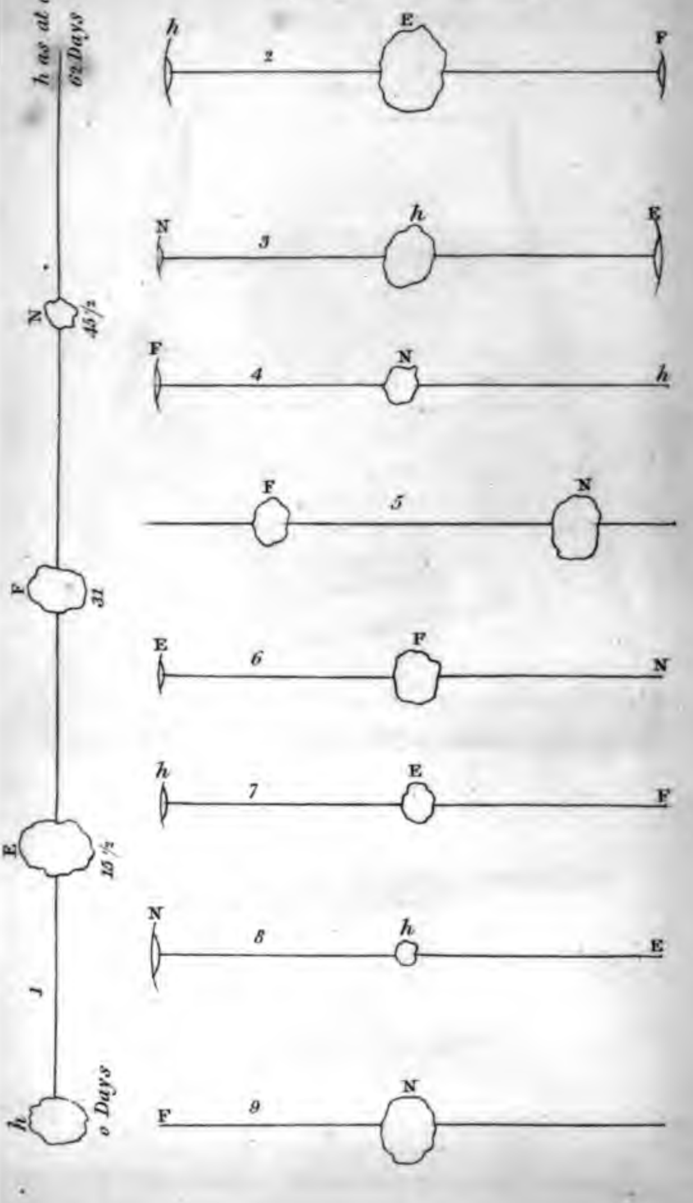
Spherical Views.



Mutlow sc. Russell C^t

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Mr. Harriſon's Preſs for Botanical Specimens.

Fig. 1.

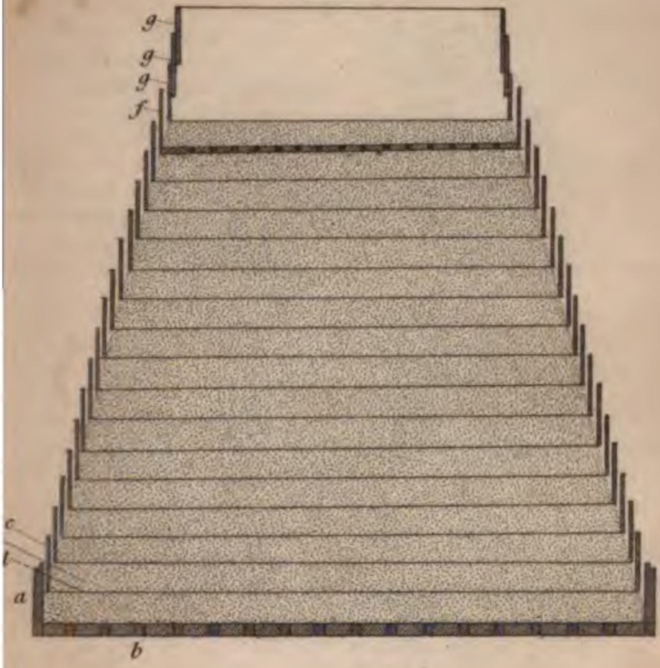


Fig. 2.

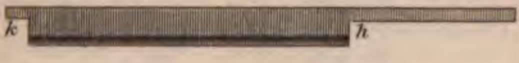
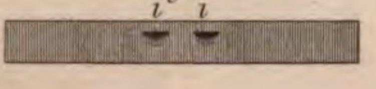


Fig. 3



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INDEX.

A.

ABERNETHY, Mr. 245
Abroma auguſta, a promiſing ſubſtitute for hemp, 45[illegible]
Accum, Mr.; reply to, on nitrous acid, 109
Acid, fluoric, in the topaz, 58
—— nitric, on the rectification of, 134
—— nitrous, on the production of, 109
Aeroſtatic voyage from Peterſburgh, moſt remarkable facts and obſervations in, 52, 55
Agues, popular remedy for, 291
Albumen, obſervations and experiments on, 245
Alumine and water, a ſtone conſiſting principally of, 153
Amicus, eaſy method of making the very combuſtible oxide of phoſphorus, 136
Animal fluids, primary, on the definite characters of, 244—254
Animate matter may be connected with inanimate, 197
Arrow palm, valuable for its fibres, wine, and ſago, 33
Atmoſphere, obſervations and doubts concerning its electricity, 17

B.

Babington, Dr. 154, 158
Balloon, log for ſhewing the courſe of a, 53
Banks, Sir Joſ. 221
Barnes, Dr. 107
Baruel, apparatus for making the gaſiform oxide of carbon invented by, 116
Barytes, mode of preparing muriate of, 1[illegible]61.—Method of decompoſing ſulphate of, 161
Battering-ram, uſeful application the, 184
Bavarian method of evaporating ſaline waters, 47, 49
Berthollet, 129
Berthollet's elements of the art of dying, new edition, 63
Berthoud, 1, 3, 4, 5
Bichat, X. 64
Biot, water formed by him by mechanical preſſure, 303
Black, Dr. 85
Blanconi, 128
Blaſting, efficacy of Mr. Jeſſop's mode of 241
Bode, Mr. 57.—New edition of his Celeſtial Atlas, 99.—His tables of the geocentric places of the new planets, 100, 102
Bonjour, 134
Bonnard, on the Bavarian method of evaporating ſaline waters, 47—49
Boring tube generally uſed in America deſcribed, 202
Boſſut, 299
Boſtock, Dr. J. his experiments on the analyſis of Goulard's extract, 75—79.—His obſervations and experiments on the definite characters of the primary animal fluids, 244—254
Boſwell, Mr. his anſwer to an Old Correſpondent, 21.—Inveſtigation of the properties of his lines drawn in a circle, 29—31.—Reply to him from an Old Correſpondent, 112.—His rejoinder, 224.
Botanical ſubjects, deſcription of a preſs for, 236—240
Boulton and Watt, Meſſrs. 96, 146, 270
Bournon, de, 104

INDEX.

Bow ſtring flax, ſtrongeſt of all vegetable fibres, 33
Boyle, Mr. his argument for diſcloſing the proceſſes of manufactories, 105
Bralle, his proceſſes for watering hemp in two hours time, 86—92
Brandé, W. Eſq. his conciſe view of the theory of reſpiration, 79—86
Braſs, method of coating with platina, 282
Brochant, 14
Brochi, 103
Brodie, Capt. Joſ.; his method of connecting iron bars and coating them with lead to form ſolid pillars, 108
Brugnatelli, on the non-exiſtence of the charged pile, 144
Burckhardt, 57
Burrows, Capt. 34

C.

Cadet, C. L. on the webs of ſpiders, 290—294
Carliſle, Ant. Eſq.; on muſcular motion, 189—201, 255—264
Carpathian mountains, mineralogical and phyſical tour in the, 60
Carteau, 60
Chenevix, R. Eſq.; on the action of platina and mercury on each other, 162, 182
Churchman, J. Eſq.; geographical and topographical improvements by, 202—206
Circle, inveſtigation of the properties of certain lines drawn in, 29—31
C. L. to Mr. Walker, 64
Cleifs, 47
Clement, 71, 170, 171
Clennell, Mr. J. on diſcloſing the proceſſes of manufactories, 105
Coal, light afforded by the gas from, 65, 73
Colours, improved mill for levigating, 119—Oil, advantageous mode of keeping, 123
Columniferæ of Linneus, utility of theſe plants, 37
Compenſation curb for watches, 19
Conſtant Reader's account of a luminous meteor, 281
Copper, on the purification of, 107, 108—Action of phoſphorus on ſolution of, 140—Method of ſeparating from ſilver, 142
Cordage, experiments on the ſtrength of various kinds, 40
Corindon found in Italy, 103—Other European ſpecimens not of the ſpecies, 104
Correſpondents, acknowledgments to, 64, 224
Cotton cloth and thread, Indian method of dying red, 296, 297
Cruikſhank, Mr. 66, 68, 69—His table of the conſtitution of carburetted hidrogen gaſes, erroneous, 70, 71
Cuthbertſon, Mr.; his remarks on Mr. Wilſon, and M. Henry's experiments on the electricity of metals, 56—Mr. Wilſon's anſwer, 110

D.

Dalton, Mr. 68, 70
Davey, Mr. 167
Davy, Prof. 81, 82, 83, 167—His analytical experiments on Devonſhire mineral, 153—158
Death, certain criterion of, 256
Deiman, Meſſrs. 67
Delametherie, J. C. on organized beings, [illegible]
Derham, 128
Deſaguliers, Dr. his eſtimate of the power of a horſe, 96, 145
Deſcartes, 266
Deſcotils, 58, 59—New metal found in platina by, 163
Deſormes, 71, 171
Deyeux, his deſcription of a new apparatus for making [illegible]ſiform [illegible] carbon, 116

Diaſpore,

INDEX.

Diaſpore, 158
Dodd, Mr. G. his improved, gun-lock, 28
Douglas, Mr. T. 36
Drieſſen, 133
Duhamel, 38—On the refining of lead in the large way, 206—215
Duméril, A. M. Conſtat, his elementary treatiſe on natural hiſtory, 62
Duncan, Dr. jun. 75
Dundonald, Lord, 69, 74
Dutertre's ſcapement an old German invention, 5

E.

Eaſt Indies, information on the mines and manufactures of the, 294—298
Edelcrantz, Sir A. N. his ſafety valve containing a vacuum valve in the ſame holes, 243
Electricity, atmoſpheric, obſervations and doubts concerning, 17—Of metals, remarks on, 56, 110—Very ſenſible inſtrument for weak degrees of, 136
Emmerling, 14
Erman, Mr. on atmoſpheric electricity, 17—19
Europeans, two found at the Marqueſas, who had forgotten their language, 60

F.

Fiſh ejected from volcanoes, 303
Fluids, on the deſcent of heat in, 225—235
——— primary animal, on the definite characters of, 244—254
Foſſil, new, 153—158
Fourcroy, 59, 75, 84, 163, 164, 181
Fulhame, Mrs. 138, 139

G.

Galvanic pile incapable of receiving a [illegible]mitted charge, [illegible]4—Rank of minerals, 169

Gaſes, combuſtible experiments on, as applicable to the purpoſes of giving light, 65—74
Gate for fields improved, 23[illegible]7
Gay, 129
Gehlen and Roſe, failure of their attempts to produce palladium, 61, 164
Gelatine, experiments and obſervations on, 250
Geographical and topographical improvements, 202—206
Goettling, his advantageous mode of preparing muriate of barytes, 133—Method of ſeparating copper from ſilver, 142—Mode of decompoſing ſulphate of barytes and preparing the muriate, 161
Gold, new and cheap method of purifying, 125—Action of phoſphorus on ſolution of, 139
Goodrich, Mr. his ſcapement not new, 4
Gough, Mr. J. 237—His inveſtigation of the properties of certain lines drawn in a circle, 29—31
Goulard's extract, experiments on the analyſis of, 75—79
Gregor, Rev. W. 158
Gregory, Mr. O. his treatiſe on mechanics announced, 144—Remarks on horſe powers, 145—153—Obſervations on his remarks, 265—271—Miſtake in his remarks corrected, 304
Gum, reſembles mucus, 25[illegible]
Gun-lock, deſcription of [illegible] improved one, 28
Guyton, 163

H.

Hales, Dr. 81, 82
Harding, the diſcoverer of the planet Juno, 57
Hardy, Mr. W. his accurate method of banking the balance of a time-keeper, 114
Harriſon, Mr. T. his preſs for botanical ſubjects, 23[illegible]—240—His experiments on Mr. Jeſſop's mode of blaſting, 241

b 2 Haſſenfratz,

INDEX.

Haſſenfratz on the propagation of ſound, 127—132

Hatchett, C. Eſq. 108, 154, 167, 251, 254—His analyſis of magnetical pyrites, 1—6[illegible]

Hauy, Abbe, 14, 104—Remarks on his experiments on the electricity of metals, 57

Heat, Dr. Irvine's experiment on the relative capacities of ice and water for, 50—Experiments ſhewing it to be conveyed downward in fluids by currents in ſome caſes, 225—235—In others by the veſſel, 235

Hemp, experiments on the culture of in India, 32—Proceſſes for watering in two hours time, at all ſeaſons, 86—92

Henry, Mr. W. 107, 295—His experiments on the gaſes obtained by the deſtructive diſtillation of wood, peat, pit-coal, oil, wax, &c. 65—74

Herſchel, Dr. 99, 274

Honey, two kinds of, 61

Hook, Dr. 5

Hopſon and Sylveſter, Meſſrs. malleability of zinc diſcovered by, 304

Hornblower, Mr. J. C. on the meaſure of force by horſe powers, 95—99—On the meaſure of mechanic power, 264—271

Horſe powers, on the meaſure of force by, 95—99, 145—153, 264—271

Howard, Mr. 167

Humboldt and Bonpland, voyages of announced, 301

Hume, Mr. 215, 283

Hunter, Dr. W. 43

——— Mr. G. 85, 196, 199

Hybernating animals, peculiarities of, 200, 255

Hydrargillite, 153—158

I.

India, on the culture of hemp in, 32—47

Iron, combined with ſulphur, naturally or artificially may be rendered magnetical, 6—Unleſs the ſulphur exceeds 46 per cent. 8—Phoſphuret of, magnetical, 9—Different ſub-carburets of, 16—Method of connecting bars and coating them with lead, to form ſolid pillars, 108 Beſt mode of aſcertaining the quantity of carbon contained in, 288

Irvine, Dr. letter on his fundamental experiment on the relative capacities for heat of ice and water, 50—52

J.

Jeſſop, W. Eſq. efficacy of his mode of blaſting, 241

Johnſon, Dr. 105

J. P.'s queſtion reſpecting the purification of copper, 107

Juan, Don G. 299

Juno, the new planet, note on, 57

K.

Klaproth, 61, 168, 169—His diſcovery of fluoric acid in the topaz, 58

Knights, Meſſrs. have palladium for ſale, 304

Knowles, Sir Charles, 38

Kruſenſtern, his voyage round the world, 60

L.

Lagrange, 82

Lalande's propoſal of a new ſcale for the thermometer, 61

Lampadius, Prof. his diſcovery of ſulphur-alcohol, 170

Lamps, theory of, 72

Langelot, Dr. Joel, 121

Laplace, 128

Laugier, 58

Lavoiſier, 79, 81, 82

Lead, Goulard's extract the proper acetate of, ſaccharum ſaturni, a ſuperacetate, 78—Cryſtallized muriate of, 135—Action of phoſphorus on ſolution of, 139—Method of refining in the large way, 206—215

Leibnitz,

INDEX.

Leibnitz, 266

Lelievre, 105

Lenſes, aberrations of light paſſing thro', 159

Lewis, Dr. 283

Light, experiments on procuring from combuſtible gaſes, 65—On the aberrations of when paſſing through lenſes, 159

Log for ſhowing the courſe of a balloon, 53

Lower, 83

Lowitz, 52

Ludlam, Mr. his wooden rod pendulum, 2

Luminous meteor, account of one, 281

M.

Machlachlan, J. Eſq. on the mines and manufactories of the Eaſt Indies, &c. 294—298

Magnet, different kinds of native, 15

Magnetic needle, its dip leſs at great heights in the air, 54

Magnetiſm of ſulphuret of iron, 6—Of carburet and phoſphuret of iron, 9

Mairan, 131

Manganeſe, action of phoſphorus on ſolution of, 141

Manufactories, diſcloſure of their proceſſes a general advantage, 105

Maps, improvement in, 203

Maraldi, 277

Marechaux, an extremely ſenſible micro-electrometer by, 136—138

Marſden, 33, 37

Maſſy, Mr. his pendulum for regulating the ſtriking part of clocks not new, 4

Mechanics, treatiſe on, announced, 144

Menzies, Dr. 81

Mercury, *ſee* Quickſilver

Metals, experiments on the electricity of, 56, 110

———, action of phoſphorus on ſolutions of, 138

Micro-electrometer, an extremely ſenſible, 136

Mill for levigating colours, improved, 119—Philoſophical, 121, *note.*—For dry-grinding, 12[illegible]—Mechanical apparatus for meaſuring the reaction in, 149

Mineralogical and phyſical tour in the Carpathian mountains, 60

Molard, 86, 92

More, S. Eſq. his experiments on the re-action againſt horſes, 97, 146, *note*—His eſtimate of horſe powers, 98

Mucus, experiments and obſervations on, 251—Reſembles vegetable gum, 254

Murdock, Mr. his proceſs for illuminating by gas from coal, 65, 73

Muriate of barytes, mode of preparing, 133, 161

——— lead, cryſtallized, 135

——— tin, recent, the moſt delicate teſt of mercury, 179

Murray, Mr. M. his deſcription of a portable ſteam-engine, 93

——— Dr. 75

Muſcles, ſtructure and qualities of, 190

Muſcular motion, Croonian lecture on, 189—20[illegible]55—264

Muſhet, Mr. Dav. 16—His experiments on wootz, 221—224, 284—290

Muſſin Puſhkin, Count, 177, 178

N.

Nerves of voluntary and involuntary motion, 194—Definite in their termination, 195—Their power conſtantly active, 264

Newton, Sir I. 159, 160

O.

Old Correſpondent, Mr. Boſwell's anſwer to, 21—His reply to Mr. Boſwell, 112

Oxide of carbon, gaſiform, apparatus for making, 116

P.

Palladium, unſucceſsful attempts to form, 61, 163—Experiments on the formation of, 162—182—For ſale at Meſſrs. Knights, 30[illegible].

Parker, Mr. T. N. 27

Pelletier,

INDEX.

Pelletier, 9, 108
Pendulum, Mr. Troughton's, 1—Wooden rod, 2—Improved, [illegible]
Penpington, Mr. 4
Perrole[illegible]5
Phillips, Mr. R. 104
Phoſphorus, eaſy method of making the very combuſtible oxide of, 136—Action of on metallic ſolutions, 138—141
Pigott, E. Eſq. his inveſtigation of the changes of the ſtar in Sobieſki's ſhield, 271—281
Pini, Prof. his letter on corindon found in Italy, 103
Planet Juno, [illegible]te on, 57—Juno, Ceres, and Pallas, geocentric places of, 100—102
Plants, methods of preſerving, 236, 240
Platina, crude, examination of by Meſſrs. Tennant and Woollaſton, 59—New metals found in, 59, 163—And mercury, action of on each other, 162—182—How fuſed by Mr. C[illegible]evix, 166—Method of coating poliſhed ſteel and braſs with, 282
Preſs for botanical ſubjects deſcribed, 236—240
Prieſtley, Dr. 66, 79, 81, 82, 83
Prize queſtions, 59, 299, 300
P[illegible]y, 299
Prouſt, 10, 75, 164—His experiments on pyrites, 12—Diſcovery of two kinds of honey, 61
Py[illegible]s, comparative analyſis of different kinds of, 6—17
Pyrometer, new, 141

Q.

Quickſilver, action of phoſphorus on ſolution of, 139—And platina, action of on each other, 162—182—Recent muriate of tin the moſt delicate teſt of, 179

R.

Rawlinſon, Mr. James, his improved mill for levigating paint[illegible] colours, 119
Reaumur, 38
Reid, Mr. T. on the conſtruction of time-keeping machines, 1—5
Reſpiration, concise view of the theory of, 79—86—Muſcles concerned in, of a mixed nature, 262
Richter, failure of his attempts to produce palladium, 62, 167
Ritter, his miſtake reſpecting the charged pile, 144—On the galvanic rank of minerals, 169
Robertſon and Sacharoff, moſt remarkable facts and obſervations in their aeroſtatic voyage from Peterſburgh, 52—55
Robiſon, Prof. his meaſure of f[illegible], 151, 265
Romme, 299
Ropes weakened by overtwiſting, 38—And by tarring, 39, 42.—Perhaps ſtrengthened by tanning, 42
Roſe and Gehlen, failure of their attempts to produce palladium, 61, 164
Roxburgh, Dr. W.; on the culture and comparative ſtrength of hemp and other vegetable fibres, 32—47
——— W. jun. 42
Roy, Julien le, 4
—— Peter le, 5
Rumford, Count, on the temperature of water when its denſity is a maximum, 225—235
Ruſſian voyage round the world, 60

S

Sacharoff and Robertſon's aeroſtatic voyage, 52—55
Safety valve, containing a vacuum valve in the ſame hole, 243
Sage, 138
Sago, the pith of the arrow palm, 33
Saline waters, Bavarian method of evaporating. 47—49
Sauſſeur, Theod. de, 158
Sauveur M. his eſtimate of a horſe-power, 146
Sawyer, Mr. 167
Scapement, crank, invented by Soumille, 4—Bad, ib.—Duplex, 5—Detached, ib.

Schmidt,

INDEX.

Schmidt, Mr. J. G. his new pyrometer, 141

Schnaubert, on the action of phoſphorus on metallic ſolutions, 138—141

Scott, Mr. James, his deſcription of a compenſation curb, 19—21

Seguin, 292

Seppings, Mr. R. his method of obviating the neceſſity of lifting ſhips, 182—188

Ships, method of obviating the neceſſity of lifting, 182—188—New fid for the upper maſts of, 186

Silver, new and cheap method of purifying, 125—Action of phoſphorus on ſolution of, 139—Method of ſeparating copper from, 142

Simmering of water, cauſe of, 216—220

Sinclair, Mr. 32

Smeaton, Mr. his eſtimate of a horſe-power, 96—His meaſure of forces, 151, 265

Smith, Mr. 104

Soumille, Abbe, 4

Sound, memoir on the propagation of, 127—132

Spiders' webs, applied to ſlight wounds, 291—A cure for agues, ib.—Analyſis of, 292

Stacio, 60

Star, variable, in Sobieſki's ſhield, inveſtigation of the changes in, 271—281

Stars, remarks on their conſtitution, 274, 280

Steam engine, deſcription of a portable one, 93

Steel, method of gilding upon, 215—From India, 221—224, 284—290—Method of coating with platina, 282

Steinacher, on the rectification of nitric acid, 134

Stocker, Mr. 154

Stodart, Mr. James, his method of gilding upon ſteel, 215—Of covering ſteel or braſs with platina, 282

Stone, ſound propagated to a great diſtance through, 129

St. Urſin, M. de, 291

Sulphate of barytes, method of decompoſing, 161

Sulphur-alcohol o[illegible]ampadius, 170

Sun, or hemp of the Bengaleſe, 34, 43—46

Sylveſter and Hopſon, Meſſrs. malleability of zinc diſcovered by, 304

Sympathy, phyſical, occaſion of 195

T.

Talc found in vaſt quantities in India, 295—For what purpoſes uſed there, ib.

Tan, perhaps ſtrengthens vegetable as well as animal fibres, 38, 42

Tar, weakens ropes, 39, [illegible]

Taylor, Mr. C. 121

Tchitchagoff, Vice-admiral, 299

Tennant, Mr. his diſcovery of two new metals in crude platina, 59

Thermometer, Lalande's propoſal for a new ſcale of the, 61

Thenard, 77

Thiout, inventor of the detached ſcapement, 5

Thoelden, Mr. A. F. his letter on the geocentric places of the new planets, 99

Thomſon, And. Eſq. his new and cheap method of purifying gold ſilver, 125—127

——— Dr. 75, 79, 80, 82, 84, 85, 251

Time-keeping machines, on the conſtruction of, 1—5—Compenſation curb for, 19—Accurate method of banking the balance of, 114

Tin, action of phoſphorus on ſolution of, 140—Recent muriate of, the moſt delicate teſt of mercury, 179

Topaz, fluoric acid diſcovered in, 58

Topographical delineations, improvements in, 203

Trevithick, Mr. 95

Tromm[illegible]orff, 133, 168, 169

Troughton, Mr. the ſteel wires of his pendulum too light, 1

V.

Vacuum and ſafety valve combined, 243

Vaſcularity of animals limited, 193

Vauquelin,

INDEX.

Vauquelin, on the topaz, 58—On platina, 59, 163, 1[illegible]—On the diaſpore, 158
Velley, T. Eſq. 236
Volcanoes, fiſh ejected from, 303
Volta, 144
Volta, on the galvanic pile, 144
Voyage round the world, 60
Voyages of Humboldt and Bonpland announced, 301

W.

Waiſtell, Mr. C. his improved gate for fields, 23—27
Walker, Mr. Ezekiel, on the aberrations of light paſſing through lenſes, 159
Watches, compenſation curb for, 19—Accurate method of banking the balance of, 114
Water, on the noiſe emitted by, before boiling, 216—220—On the maximum denſity of, 225—235—Formed by mechanical preſſure, 303
Water-hammer deſcribed, 217
Watt, Mr. his eſtimate of the power of a horſe, 96
Wavell, Dr. 153
Wavellite, 153—158
Wells, Capt. 186
Welter, 134
Werner, 14
W. F. S. his reply to Mr. Accum on nitrous acid, 109
Whatley, Mr. T. 237
Windenmann, 14
Wilſon, Mr. W. remarks on his experiments on the electricity of metals, 56—His anſwer, 110
Withering, Dr. 236
W. N. on the ſimmering of water, 216—220
Wollaſton, Dr. his diſcovery of two new metals in crude platina, 59—And of palladium in it, 182
Wootz, experiments on, 221—224, 284—290
Woulſe, 116

Z.

Zinc, a malleable metal, 304.

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Of

PREFACE.

Of the Engravings, the Subjects are, 1. Apparatus for raising Water, by Mr. Close. 2. Furnace for bending Wood. 3, 4. Blowing Engine by the Fall of Water at Poullaouen. 5, 6. Apparatus for analysing Vegetable Soils, by H. Davy, Esq. 7. Diagrams to explain the Doctrine of Heat, by Count Rumford, V. P. R. S. 8. Magnified Representation of Insects found on Corn, by G. Cumberland, Esq. 9. An improved Steam Engine, by Mr. Clegg. 10. Simple Register Thermometers. 11. The Framing employed to raise the Roof of Clapham Church. 12. An improved Sheep Fold, by T. Plowman, Esq. 13. Applicative Compass, by Capt. Starck, R. N. 14. Section of a Drain, by J. C. Curwen, Esq. 15. Diagrams to illustrate a Series of Propositions respecting a Division of the Circle, by Mr. John Gough. 16. Schemes exhibiting the Positions of the new Planet Juno, by Dr. Herschel. 17. Mr. Pape's Improvement of Rye Harbour. 18. A new Air Pump, by Mr. Wright of America. 19. New Valve and Parts of a condensing Syringe for the Gases, by Mr. Cuthbertson, with which Mr. Northmore's Experiments were made. 20. A very simple and improved Graphometer, by Mr. Bancks.

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TABLE OF CONTENTS

TO THIS TWELFTH VOLUME.

SEPTEMBER, 1805.

GRAVINGS of the following Objects: 1. New Apparatus for raiſing iter above its Level by the Syphon, by Mr. Cloſe; 2. Furnace for bending od; 3, 4. Two Plates of the Machine for producing a Blaſt by a Shower Water paſſing down a perpendicular Pipe.

ter from Davies Giddy, Eſq. M.P. deſcribing a ſingular Fact of the invi- e Emiſſion of Steam and Smoke together from the Chimney of a Furnace; ugh either of them, if ſeparately emitted, is viſible as uſual. Page 1

n a Meteoric Stone that fell in the Neighbourhood of Sigena, in Arragon, 773, by Profeſſor Prouſt. - - - - - - 2

Farther Remarks on Mechanic Power, in Reply to Mr. J. C. Horn- wer. In a Letter from Mr. O. Gregory, Royal Mil. Academy. 7

eſcription and Effects of an Apparatus for raiſing Water by Means of condenſed in its Deſcent through an inverted Syphon. By Mr. William ſe. From the Inventor. - - - - - - 16

marks on Count Rumford's Experiments relating to the Maximum Den- of Water. In a Letter from Mr. John Dalton. - - 28

n the Art of bending Wood. By J. H. Haſſenfratz. - - 30

Experiments made in the great, in a reverberating Furnace on Caſt Iron, firming the eſtabliſhed Theory reſpecting the Difference between caſt and leable Iron. By G. A Lampadius, Prof. of Chemiſtry and Metallurgy reyberg. - - - - - - - - 34

Remarks on the burſting of two Muſquet Barrels by a Charge of Gun- der confined by Sand. W.N. - - - - 40

eport of a Method of meaſuring the initial Velocity of Projectiles dif- rged from Fire-arms, both horizontally and with different Elevations, le to the Phyſical and Mathematical Claſs of the National Inſtitute by Mr. ny, Dec. 11, 1803. Abridged from the Original. - - 41

ct concerning the inviſible Emiſſion of Steam into the Air. W.N. 47

xperiments made with the Water blowing Machines of the Iron Works of llaouen; by Citizens Beaunier and Gallois, Mine-Engineers. - 48

A Method of rendering the long and ſhort Vibrations of a Balance, erned by a ſpiral Spring, preciſely equal in Duration. By Mr. Charles ung. In a Letter from the Inventor. - - - 56

ific News, 58.—Compoſition of Muriatic Acid, ib.—Literary and Philo- hical Society of Newcaſtle upon Tyne, 60.—New Proceſs for ſteeping np, 61.—Medical Theatre, St. Bartholomew's Hoſpital, ib.—Medical itution, 62.—Properties of blued Steel not generally known, 63.—Pre- ation of ſucculent Plants, 64.

OCTOBER

OCTOBER 1805.

Engravings of the following Objects: 1, 2. Apparatus for Analyſing Vegetable Soils, by H. Davy, Eſq. Profeſſor of Chemiſtry at the Royal Inſtitution; 3. Diagrams to explain the Doctrine of Heat, communicated by Count Rumford.

I. Experimental Inveſtigations concerning Heat. By Benjamin Count of Rumford, V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c. Received from the Author. - - - - Page 65

II. On pure Nickel, diſcovered to be one of the noble Metals, and on its Preparation and Properties. By J. B. Richter. - - - - 75

III. On the Analyſis of Soils, as connected with their Improvement. By Humphrey Davy, Eſq. F. R. S. Profeſſor of Chemiſtry to the Board of Agriculture and to the Royal Inſtitution. - - - 81

IV. Diſcovery of a new Vegetable Subſtance, by Mr. Roſe. - - 97

V. New Galvanic Diſcoveries by Mr. Ritter, extracted from a Letter from Mr. Chriſt. Bernoulli. - - - - - - - 99

VI. Improvement in applying the Points in Electrical Machines. By Mr. G. J. Singer. - - - - - - - - 103

VII. Queſtion whether Light as a Body may not have its Temperature raiſed or lowered, and produce the Effects aſcribed to reflected Heat. By J. P. 104

VIII. Experiments on a Mineral formerly called falſe Tungſtein, now Cerite, in which a new Metal has been found. - - - 105

IX. Abſtract of a Memoir, entitled Conſiderations on Colours, and ſeveral of their ſingular Appearances; read at the Claſs of Mathematical and Phyſical Sciences of the National Inſtitute, March, 1805, by C. A. Prieur. 112

X. Report made by the Phyſical and Mathematical Claſs of the Inſtitute, in Anſwer to the Queſtion, whether thoſe Manufactories, from which a diſagreeable Smell ariſes, may prove injurious to Health. Read in the Sitting of January, 1805, by Meſſrs. Guyton-Morveau and Chaptal. 122

XI. Facts relative to the torpid State of the North American Alligator. By Benjamin Smith Barton, M. D. - - - - - 131

XII. Obſervations and Experiments on the conducting Power of Fluids. By T. S. Traill, M. D. From the Author. - - - 135

XIII. Indian Account of a remarkably ſtrong and ferocious Beaſt, which (they ſay) exiſted in the northern Parts of the State of New York about two hundred Years ago. Collected and communicated by Mr. John Heckewelder. 139

Scientific News, &c. 142.—Aſtronomical Prize, ib.—New Muſical Inſtrument, ib.—Saverien, 143.—Pure and beautiful Ceruſe, ib.—Chromate of Lead and of Silver, 144.—Putrefaction prevented, ib.—Leverian Muſeum, ib.

NOVEMBER

NOVEMBER 1805.

Engravings of the following Objects: 1. Magnified Repreſentations of Inſects found on Corn, by G. Cumberland, Eſq. 2. An improved Steam Engine, by Mr. Clegg; 3. Simple Regiſter Thermometers; 4. The Framing employed to raiſe the Roof of Clapham Church; 5. An improved and œconomical Sheep Fold, by T. Plowman, Eſq. 6. Applicative Compaſs, by Capt. Starck, R. N. 7. Section of a Drain, by J. C. Curwen, Eſq.

I. Facts and Obſervations relating to the Blight, and other Diſeaſes of Corn. In a Letter from G. Cumberland, Eſq. - - - - Page 145

II. Experimental Inveſtigations concerning Heat. By Benjamin Count of Rumford, V. P. R. S. &c. - - - - - - 154

III. Obſervations on blaſting Rocks; with an Account of an Improvement, whereby the Danger of accidental Exploſion is in a great Meaſure obviated. By Mr. William Cloſe. From the Author. - - - 171

IV. Deſcription of a portable Steam-engine, invented by Mr. Samuel Clegg, David-Street, Mancheſter. Communicated by Mr. Dalton, Lecturer at the Royal Inſtitution, &c. - - - - - - 174

V. Letter from Mr. J. C. Hornblower, deſcribing the framed Work by which the Roof of Clapham Church was raiſed to its original Situation, without diſturbing the Interior of the Building, &c. - - - 176

VI. Experiments on draining Land. By John Chriſtian Curwen, Eſq. M. P. of Workington-Hall, in Cumberland, with an Engraving. - 177

VII. Remarks on a Letter of Mr. Dalton, concerning the Maximum Denſity of Water; with an Account of two Experiments of Dr. Hope, tending to ſhew that it takes Place at a Temperature above the freezing Point. In a Letter from T. I. B. - - - - - - - 180

VIII. Obſervations and Conjectures relative to the ſuppoſed Welch Indians in the weſtern Parts of North America. Republiſhed from the "Kentucky Palladium," with additional Remarks and Conjectures, by the Editor of the Philadelphia Medical and Phyſical Journal. - - - 181

IX. Account of an improved Sheep-Fold, contrived and conſtructed by Thomas Plowman, Eſq. of Broom in Norfolk, and communicated by him to the Society for the Encouragement of Arts. - - - - - 192

X. Anecdotes of an American Crow. By William Bartram. - 194

XI. An Account of the Seiches of the Lake of Geneva. By M. Vaucher. 198

XII. Experiments to aſcertain the beſt Colour for marking the Heads of Pieces of Cotton or Linen in the rough, which ſhall be capable of reſiſting the Operations of Bleaching, as well as the moſt complicated Proceſſes of Calico Printing, without ſpreading beyond the Limits of the Impreſſion. By Mr. Hauffmann. - - - - - - - - 206

XIII. Note on the Formation of Water by mere Compreſſion, with Reflections on the Nature of the Electric Spark. By M. Biot. - - 212

XIV. Account of Thermometers for regiſtering the higheſt and loweſt Temperatures in the Abſence of the Obſerver. By F. A. - - 215

XV. Abſtract of a Memoir on Milk. By M. Thenard. - - 218

Scientific News, 220.—Temperature of the Sea, ib.—Spent Oil of the Curriers, ib.—Note reſpecting the Decompoſition of Sulphate of Lead by the Muriatic Acid. By M. Deſcotils, 221.—Extract of a Letter from Naples, dated Auguſt 13, 222.—Applicative Compaſs for taking Bearings on a Chart. By N. D. Starck, of the Royal Navy, 224.

DECEMBER

DECEMBER, 1805.

Engravings of the following Objects: 1. Diagrams to illuſtrate a Series
poſitions reſpecting a Diviſion of the Circle, by Mr John Gough; 2. I
exhibiting the Poſition of the new Planet Juno, by Dr. Herſchell;
. Pape's Improvement of Rye Harbour.

I. On the Diviſion of an Arch of a Circle into two ſuch Parts, that thei
or Coſines, or Verſed-Sines, ſhall have a given Relation. In a Lett
John Gough, Eſq. - - - - - - - P

II. Concerning the State in which the true Sap of Trees is depoſited
. Winter. By Thomas Andrew Knight, Eſq.

III. On the Deliqueſcence and Efflореſcence of Salts. By C. L. Cadet.

IV. Account of the ſimple and eaſy Means by which the Harbour
was reſtored, and made navigable for Ships of conſiderable Burthe
the Rev. Daniel Pape. - - - - -

V. New Experiments on the Reſpiration of Atmoſpheric Air, principal
Regard to the Abſorption of Azote, and on the Reſpiration of the
Oxide of Azote. By Profeſſor Pfaff, of Kiel. - -

VI. Experiments on Gum Arabic and Gum Adracanth. By M. Vauquel

VII. Method of obtaining Cobalt pure. By M. Tromſdorf. -

VIII. A new Method of extracting Sugar from Beet-Root. By M. Acha

IX. On Nickeline (Niccolanum), a Metal in many Reſpects reſembling
lately diſcovered by Dr. J. B. Richter. - - -

X. Letter from G. Cumberland, Eſq. on a Project for extended Roads
Principle of the inclined Plane. - - - -

XI. On the State of Provincial Societies for Scientific and Literary In
ment. By a Correſpondent. - - - -

XII. Notice of certain Inſtances of waſteful Negligence in ſome Fiſherie
North. By an Enquirer. - - - -

XIII. On Bile. By M. Thenard. - - -

XIV. Quotation from Sir George Staunton's Embaſſy, containing a I
tion of Fire Works unknown in Europe. Propoſed by a Correſ
with a View to obtain an Explanation of the Means by which they w
duced. - - - - - -

XV. On the Carbonate of Potaſh. By M. Steinacher. - -

XVI. Method of preparing a luminous Bottle, which long preſerves its Eff

XVII. Analyſis of the Magneſian Earth of Baudiſſero in Canavais (in
partment of the Loire), known by the Name of Porcelain Earth,
therto conſidered as a Clay. By M. Giobert. - -

XVIII. Experiments for aſcertaining how far Teleſcopes will enable us t
mine very ſmall Angles, and to diſtinguiſh the real from the ſpuri
ameters of celeſtial and terreſtrial Objects: with an Application of
ſult of theſe Experiments to a Series of Obſervations on the Natu
Magnitude of Mr. Harding's lately diſcovered Star. By William H
L. L. D. F. R. S. Abridged from the Phil. Tranſ. 1805. -

XIX. Account of ſome new Improvements in Steam-Engines. By Mr.
Woolf. - - - - - -

Scientific News, 296.—Geometry, ib.—Aſtronomy, 297.—Geography
Electricity, ib.—Aeroſtation, 298.—Univerſity of Charkow, ib.—
Garden, &c. at Copenhagen, 299.

SUPPLEM

CONTENTS.

SUPPLEMENT.

s of the following Objects: 1. A new Air-Pump, by Mr. Wright
rica; 2. New Valve and Parts of a condenſing Syringe for the Gaſes,
Cuthbertſon, with which Mr. Northmore's Experiments were made;
ry ſimple and improved Graphometer, by Mr. Banks.

cription of an Air-Pump upon a new Conſtruction. By Eliſur
Communicated by Benj. Silliman, Eſq. Profeſſor of Natural Phi-
&c. in Yale College, Newhaven, America. - Page 303

rning the State in which the true Sap of Trees is depoſited during
By Thomas Andrew Knight, Eſq. - - 30[illegible]

ılar Method of forming Walls and Roofs of Rural Buildings in In-
Communicated by M. Legoux de Flaix, Officer of Engineers. 313

unt of ſome new Improvements in Steam-Engines. By Mr. Arthur
- - - - - - 316

Magneſian Earth of Baudiſſero. By M. Giobert. - 320

Communication on an artificial Tan prepared from Coal, chared
reſinous Subſtances, &c. Abridged from the Original of Charles
, Eſq. F. R. S. - - - - 327

noir on the Diſcovery of a factitious Puzzolana, preſented to the
National Inſtitute by M. Dodun, Engineer in Chief of Bridges and
ys in France. - - - - 331

periments and Obſervations upon the Contraction of Water by Heat
Temperatures. By Thomas Charles Hope, M. D. F. R. S. Ed.
r of Chemiſtry in the Univerſity of Edinburgh. From the Edinburgh
hical Tranſactions for 1804. - - - 339

vations on Turf, from the German Rathbeger fur alle Stænd. By
Collenbuſch. - - - - - 364

ments on the remarkable Effects which take place in the Gaſes, by
in their Habitudes, or elective Attractions, when mechanically com-
By Thomas Northmore, Eſq. In a Letter from the Author. 368

unt of a Graphometer for meaſuring the Angles of Cryſtals In a
rom Mr. Robert Bancks, No 411, Strand. - 374

new Books, 376.—Philoſophical Tranſactions of the Royal Society
on for 1805, Part II. ib.—Tranſactions of the Royal Society of
gh, (being the Continuation of Part II. together with Part III. of
Volume), &c. 377

ADVERTISEMENT.

THE Patrons and Friends of the Philoſophical Journal are reſpectfully informed, that this work will in future conſiſt of ſix ſheets of matter inſtead of five in each number, together with a ſupplementary number to each volume; and that the plates will be ſo managed as to contain a larger number of ſubjects executed in the beſt ſtile. Theſe improvements, though attended with additional expence, are ſuch as the Editor has thought it his duty to adopt; in order that the very extenſive communications with which the Journal has been honored, might not prevent him from giving all the foreign diſcoveries and ſuch other general intelligence as the nature of the plan demands. It will eaſily be ſeen that the additional copy will by this means amount to one half more than the former quantity; and it is unneceſſary to point out the great advantages which muſt reſult from ſuch an addition. The Editor takes this occaſion to repeat his acknowledgments for the encouragement which has been given to his exertions, particularly within the laſt twelve months, in which the ſale has nearly doubled. The quantity of original matter continues to increaſe, and amounts to more than half of the whole work. Great part of the remainder conſiſts in foreign articles never before publiſhed in this country, together with ſome extracts and abridgments from our beſt academical tranſactions. The whole publication may therefore be conſidered as original, ſince it is never made up by extracts from the periodical works of this country, which, on the contrary, very frequently copy from its contents.*

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* It has been neceſſary to reprint a conſiderable part of the former volumes, in order to make complete ſets, which may now be had, or any ſingle numbers or volumes, from the commencement.

A
JOURNAL
OF
ATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SEPTEMBER, 1805.

ARTICLE I.

ter from Davies Giddy, *Eſq. M. P. deſcribing a ſingular 'act of the inviſible Emiſſion of Steam and Smoke together from he Chimney of a Furnace; though either of them, if ſeparately mitted, is viſible as uſual.*

To Mr. NICHOLSON.

SIR, *Cliſton, Auguſt* 6, 1805.

RAVELLING, and a variety of occupations, have hi- rto prevented me from ſending you an account of the cir- iſtances obſerved by myſelf and others, during the work- of an engine on Mr. Trevithick's conſtruction, at Merthyn well in South Wales, and which I had the pleaſure of ting to you, ſome time ſince, in Soho Square. I now ſmit a ſtatement of the facts, avoiding all comments or mpts at explanation.

Peculiar facts obſerved in working one of Trevithick's ſteam engines.

Ir. Trevithick having adapted his ſteam engine to the poſe of moving waggons, contrived every acceſſary part ight as he poſſibly could, and as little inconvenient to per- who might aſſiſt, or witneſs an experiment. The flue conveying off the ſmoke, and affording a draft, was made

The ſteam engine was adapted to move a carriage, and every part made light and ſimple.

The ſteam was (after work) thrown into the chimney many feet from the fire.

Neither ſmoke nor ſteam were viſible.

When the ſmoke was ſhut off the ſteam was viſible, and when the ſteam was ſhut off the ſmoke was viſible.

The draft up the chimney was increaſed by the admiſſion of the ſteam.

of rolled iron; and the ſteam, which wholly eſcapes from theſe machines uncondenſed, was conducted into the ſame tube, about a foot above its inſertion into the boiler; therefore many feet from the fire, and beyond the regiſter. When the engine began to move, it was ſoon remarked that neither ſteam nor ſmoke were ſeen to iſſue from the flue: and when freſh coal was added, nothing more than a faint white cloud became apparent, and that only for a ſhort time; nor were drops or miſt viſible any where. It was propoſed, that the regiſter ſhould be ſlowly cloſed; and as this was done, a condenſation of ſteam manifeſted itſelf at a ſmall diſtance from the chimney, and finally appeared in the ſame quantity, as if it had proceeded immediately from the boiler. The experiment was then reverſed. The ſteam was gradually confined to the boiler; when ſmoke became more and more viſible, till it equalled in quantity and appearance that commonly produced by a ſimilar fire: and theſe trials were alternated a great number of times, with unvarying ſucceſs. Laſtly, it became a matter of ſpeculation, whether or in what degree the draft was affected by the admiſſion of ſteam into the flue. To aſcertain this, every one preſent looked as attentively as poſſible into the fire-place; while the engine moved at the rate of a few ſtrokes in a minute; and all agreed in declaring, that the fire brightened each time the ſteam obtained admiſſion into the chimney, as the engine made its ſtroke.

I am, Sir,

Your very faithful humble ſervant,

DAVIES GIDDY.

II.

On a Meteoric Stone that fell in the Neighbourhood of Sigena, in Arragon, in 1773, *by Profeſſor* PROUST[*].

Stones have fallen from the atmoſphere in various ages,

THE author begins his paper by ſome previous hiſtorical facts. No one now queſtions, that ſtones have fallen from the atmoſphere in different parts of the world. The ancients mention it as having occurred at various times, and later ages have recorded the time and authenticated circumſtances of ſeveral ſuch incidents. In our own days, ſtones or mineral bodies termed meteoric, have been collected in the Eaſt Indies, America, Scotland, England, France, Italy, Hungary, and laſtly in Spain: and that nothing may be wanting in future to convince thoſe who refuſe their aſſent to the united teſtimony of all ages and all countries, nature appears to have purpoſely ordered a repetition of this ſurpriſing phenomenon: no longer ago than the 26th of April, 1803, a ſhower of theſe ſtones covered a ſpace of ground two miles long and above a mile broad, near l'Aigle in Normandy. The French Inſtitute immediately nominated a commiſſioner, to examine into the fact on the ſpot, to take the depoſitions of witneſſes, compare them with the circumſtances, and bring ſome of the ſtones to Paris.

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[*] Abridged from the Journal de Phyſique, for March 1805.

and in all parts of the world.

Shower of ſtones in 1803.

As the firſt thing to be done with a new mineralogical ſubſtance, is, to analyſe it, the Preſident of the Royal Society of London, and ſeveral other gentlemen who had ſuch ſtones in their collections, put them into the hands of Mr. Howard, a Member of the Society, that he might ſubject them to chemical examination. He found to his great ſurpriſe, that all theſe ſtones, from the remoteſt quarters of the globe, contained the ſame principles, differing only in proportion; and, what was ſtill more ſtriking, that they all contained iron combined with nickel, a compound to be met with among none of the minerals in any part of the globe with which we are acquainted. Vauquelin has ſince confirmed by repeated experiments, the accuracy of Mr. Howard's obſervations. All men of ſcience have hence been led to conclude, that theſe ſtones muſt have a common origin; but whence they originate is the queſtion. Do they belong to that earth on which they fall? are they formed in the atmoſphere itſelf? or have they been projected from lunar volcanoes? On theſe points men's ſentiments are divided; and the arguments have been collected by Dr. Izarn, in his *Lithologie Atmoſphérique.*

Analyſis of many of theſe bodies by Mr. Howard,

who found them all ſimilar in their compoſition.

They have therefore a common origin,

but whence?

One of theſe ſtones has been in the royal collection at Madrid ever ſince 1773. This the miniſter has allowed Mr. P. to analyſe, leaving the principal part of it ſtill in the collection for the ſatisfaction of the curious. The following letter was ſent with it to Don Manuel de Roda, Miniſter of State, by the captain-general of Saragoſſa.

One in the collection at Madrid.

"In

Account of its fall.

"In November laſt an extraordinary occurrence, ſaid to have happened on the Seventeenth of that Month in a ploughed field at Sena, a village in the diſtrict of Sigena, was the ſubject of converſation in this city.

"The ſky being perfectly ſerene, three reports reſembling thoſe of cannon were heard, and followed by the fall of a ſtone weighing nine pounds and one ounce, at a little diſtance from two labouring men. One of them went up to it, but the ſtrong ſmell it emitted ſtopped him a moment.

"Recovering from his ſurpriſe he went nearer, raiſed it up with his ſpade, and waited till it was ſufficiently cold for him to carry it to the village, where he delivered it to the prieſt.

"From inquiries made immediately afterwards on the ſpot, and among the people in the neighbourhood, it appears, that the noiſe in the air and fall of the ſtone were not accompanied with any ſtorm, or with lightning."

Another meteoric ſhower of ſtones that fell at Roa in Spain, in 1438,

To bring into one view all that is yet known of ſtones falling in Spain, the author ſubjoins a letter of the Bachelor Cibdadreal, on thoſe that fell in the village of Roa, near Burgos, in 1438.

In the view of the King of Spain.

"While the King Don John and his court were hawking near the village of Roa, the ſun was concealed behind white clouds, and bodies reſembling gray and blackiſh ſtones were ſeen to fall from the air, of ſuch bulk as to occaſion the greateſt ſurpriſe.

The ground was covered with them.

"After this phenomenon had continued for an hour the ſun re-appeared, and the falconers immediately rode to the place, which was not above a mile diſtant. They brought back information to the king, that the ground was ſo completely covered with ſtones of all ſizes as not to be viſible.

"The king would have gone thither, but his courtiers prevented him, obſerving, that a place choſen by Heaven for the theatre of its operations might not be free from danger, and that he had better ſend ſome of his attendants. Gomez Bravo, the captain of his guards, undertook the office. He brought four of the ſtones to Roa, whither the king had already retired. They were of conſiderable ſize: ſome were round, and as large as a mortar, others like pillows and *half-fanegue* meaſures (ſuch as contain about 45lbs weight of corn,) but what was moſt aſtoniſhing, was their exceſſive lightneſs, ſince

These ſtones were very large,

extremely light,

and of a tender texture.

the largeſt did not weigh half a pound. They were of ſuch a tender texture, that they reſembled the foam of the ſea condenſed more than any thing. You might ſtrike on them with your hand without fear of bruiſe, or pain, or the ſlighteſt mark. The king has ordered ſome to be ſent you, &c."

It ſeems from this deſcription, that theſe ſtones muſt have been very different from thoſe of the preſent day.

The ſtone of Sigena deſcribed.

The ſtone of Sigena, when delivered to Mr. Prouſt, weighed ſix pounds ten ounces. With it was a piece of three or four ounces, the only one remaining of thoſe that had been broken from it by curious perſons. It was interſperſed with ſpots of ruſt, both externally and internally, owing probably to its having been immerſed in water to try the effect of that fluid on it. From theſe however, ſome inſtructive inferences may be drawn reſpecting the native place of theſe ſtones.

Interſperſed with ruſty ſpots.

Its figure.

Its ſhape is an irregular oval, ſeven or eight inches long, four or five broad, and four in its greateſt thickneſs. One ſide is flattiſh, a little depreſſed in the middle, and very round on the edges: the other is an obtuſe triedral pyramid with unequal ſides, greatly rounded at the ſummit and on the edges*. It appears to have had the black vitreous cruſt common to ſtones of this kind, though from its fragility the greater part has fallen off in paſſing through many hands and receiving occaſional blows, ſo that none remains except in the hollow of the baſe, and a little on the faces of the pyramid.

It had a vitreous cruſt.

This muſt have been produced by violent momentary heat ſince its formation.

On examining this cruſt it is eaſy to ſee, that it muſt have been the effect of heat ſubſequent to the formation of the ſtone, and unqueſtionably very powerful though momentary; ſince the metallic and ſulphureous particles immediately beneath the cruſt had not time to change colour, or even loſe their luſtre.

It is porous,

not very hard,

It has all the poroſity of an aggregated maſs of ſandy particles without any cement, ſo that the breath will eaſily paſs through a piece held between the teeth. It will not ſtrike fire with ſteel, and the ſame may be ſaid of the pyrites it contains.

of a bluiſh gray.

Its colour is a uniform bluiſh gray, like that of a black ſubſtance enlightened by a white: it is the hue of an earthy compound tinged by iron oxided at a *minimum*.

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* Does this deſcription agree with what is ſaid above, that ſeveral pieces have been broken from it? Apparently above a quarter of the ſtone, on comparing its original and preſent weight. J. C.

The

It is a ſandy maſs interſperſed with metallic and ſulphureous particles.
Its granules are cryſtalline.

The ſtone itſelf is a ſandy maſs, formed of rounded oval grains, the largeſt of which are ſcarcely bigger than hempſeed, among which are interſperſed metallic and ſulphureous particles with all their primitive luſtre, and particularly with that light tint of kupfernickel obſerved in the other ſtones. On examining the earthy grains by the microſcope, we perceive, that, far from having been faſhioned by the movement of water, they are globules rough with cryſtalline or reflecting points, ſo that they can by no means be confounded with ſand.

Heat deepened its colour, and oxided the metal.

A piece of about two inches being expoſed to a red heat in a crucible for half a quarter of an hour was much changed: the ſandy globules became of a darker gray, and the metallic particles, diveſted of their luſtre, were viſibly oxided.

Greater heat fuſed it.

About two ounces were heated for half an hour in a forge fire, which converted the ſtone into a ſemivitreous maſs, blackiſh, and ſlightly porous. It did not appear to have effervеſced much previous to fuſion, and was interſperſed with globules of iron, which had not time to deſcend, though upwards of a hundred grains of a regulus were collected at the bottom.

It contained much magnetic iron,

The iron attractable by the magnet was not uniformly mixed in the ſtone, as from ſome parts 22 in the 100 were extracted, from others not more than 17.

combined with nickel.

This iron was combined with nickel in the proportion of about 3 per cent. No nickel was diſcoverable in any other part of the ſtone.

After this alloy was ſeparated by the magnet, the remainder of the ſtone was found by analyſis to conſiſt of,

Conſtituent parts of the remainder.

Iron ſulphurated at a *minimum* -	12
Black oxide of iron - - -	5
Silex - - - -	66
Magneſia - - - -	20
Lime and magneſia in quantities too ſmall to be appreciated - -	
	103

New hypotheſis. Their origin probably in the polar regions,

On conſidering the rapid alteration of theſe ſtones by moiſture, for a fragment kept twelve hours under water was taken out covered with ſpots of ruſt, which diſtinguiſhed the grains of alloy from the ſulphureous particles with which they were before confounded;—it is obvious, according to the author, that they cannot ſubſiſt in any of the habitable parts of the globe. But from the eternal cold of the polar regions, where water remains for ever a ſolid maſs, and iron cannot ruſt, he thinks we may reaſonably look to theſe regions as the native place of ſuch bodies. In this he inſiſts there is nothing impoſſible, or even improbable. And why ſhould thoſe meteors, he demands, of which we know neither the origin, the combuſtibles that afford them aliment, the impulſe by which they are moved, nor the nature of the lines they deſcribe in their courſe, be leſs capable of tearing them from ſome part of the globe, than of forming them, contrary to all phyſical probability, from elements which the atmoſphere can neither create nor hold in ſolution?

whence they are conveyed to other parts by meteors.

III.

Farther Remarks on Mechanic Power, in Reply to Mr. J. C. Hornblower. In a Letter from Mr. O. GREGORY, *Royal Mil. Academy.*

To Mr. NICHOLSON.

SIR,

I AM ſorry to be under the neceſſity of troubling you with a few obſervations for inſertion in your Journal, in conſequence of being called upon by Mr. Hornblower, as though it were to defend ſome newfangled doctrine, when the poſitions in my former letter, which that gentleman thinks proper to cenſure, are in perfect conformity with the principles aſſumed or demonſtrated by every correct writer on mechanical philoſophy ſince it has been placed upon its proper baſis in the *Principia* of Newton. The ſubject I am now invited to diſcuſs, has ſo frequently been exhibited in the cleareſt light by various authors, both in England and on the Continent, that I ſhould not think myſelf juſtified in occupying many of your pages by an elaborate diſſertation; out of regard, however, to ſo reſpectable a correſpondent as Mr. H. I cannot help entering a little into the diſcuſſion, though I am, I confeſs, quite unable to aſcertain whether his laſt letter is meant to oppoſe my former remarks, and thoſe of Profeſſor Robiſon with ſerious arguments, or is merely intended as a *Jeu d'eſprit*.

Prefatory remarks.

It will not, I hope, be expected that I ſhould point out in what inſtances Mr. H's remarks appear to me completely irrelative to the ſubject in hand; or thoſe in which he ſeems to have miſunderſtood the arguments of the late leared Profeſſor: ſuch a procedure would only lead into farther diſcuſſion, while I feel ſolicitous to avoid it, from a conſcioufneſs that it would be very unintereſting to moſt of your readers. I ſhall ſtrive to confine myſelf, therefore, to ſuch of Mr. H's enquiries as bear upon the point in diſpute, and for the ſake of condenſing my labour, ſhall begin with that in his poſtſcript.

Are animal exertion and mechanic power identical?

Firſt then, I will endeavour to "ſet Mr. H. right as to the identity of *animal exertion* and *mechanic power*:" and to this end it will be requiſite to anſwer the queſtion,—what is mechanic power? excluding, for the preſent, that acceptation of the term in which it is underſtood to denote one of the ſix ſimple machines. Now, it is pretty obvious, that the terms power, force, &c. when uſed in mechanical ſcience are purely metaphorical; for, as Profeſſor Dugald Stewart remarks, (*Elements of the Philoſophy of the Human Mind*, p. 202.) "All the languages which have hitherto exiſted in the world, have derived their origin from *popular uſe*; and their application to philoſophical purpoſes was altogether out of the view of thoſe men who firſt employed them."

Riſe and application of the terms, force, power, &c.

Language commenced amongſt ſimple men, who had little, if any acquaintance with what is now called ſcience: and in the gradual progreſs of moſt nations, from the ſavage to the ſhepherd ſtate, thence to the agricultural, and farther to the commercial ſtate, it would be very long before they would think of attaching any other meaning to the terms in the different languages, correſponding to power, force, action, reſiſtance, repulſion, &c. than thoſe which were manifeſtly referable to the different kinds of human, or of animal exertion: in ſubſequent times when ſcientific men began to claſſify, arrange, and ſyſtematize the phenomena which they obſerved in the congreſs, motion, and mutual operation of bodies, they found it much eaſier to denote the circumſtances they would deſcribe or treat of, by a figurative application of old terms, to which ſome analogous notions would neceſſarily be attached by every perſon, than to invent new ones, which would be attended by no ideas independant of an arbitrary definition. Nor, in this application, was there any danger of important error, for the things to which the terms were appropriated would exhibit ſuch ſpecific differences as would almoſt entirely preclude the chance of confounding one with another; and leave no room to fear, that when the terms were applied to inanimate beings, they would be concluded to exert ſtrength, or to poſſeſs power, as animals did; any more than we ſhould now fear being miſunderſtood when we ſpeak of the force of arguments, the attractions of benevolence, the faſcinations of beauty, or the repulſive tendency of envy. Thus, from contemplating the proceſs of this gradual refinement, (a refinement produced not by barren ſpeculators, but by the neceſſary demands occaſioned by the progreſs of civilization,) we ſee that the words power and force, primarily uſed to denote animal energy, are now, by a natural extenſion grounded upon an obvious analogy, employed to expreſs *efficiency* in general. It will hence be eaſy to aſſign the proper philoſophical acceptation of theſe terms, when uſed in the ſcience of mechanics. *Force or power, in a mechanical ſenſe, is that, whatever it be which cauſes a change in the ſtate of a body, whether that ſtate be reſt or motion.* This definition does not require our entering into any metaphyſical diſquiſitions relative to the nature of cauſes, or the connection of cauſe and effect: that every event is brought about by ſome cauſe, that is, by ſome agency, or ſomething which precedes in the order of occurrence, is a truth which I think none will be diſpoſed to deny; but what is the agency, or where it actually reſides, we can ſeldom know, except perhaps in the caſe of our own voluntary actions. It is not then the buſineſs of the mechaniſt, ſtrictly ſpeaking, to enquire into the *modus operandi*; we learn from univerſal experience, that the muſcular energy of animals, the operation of gravity, electricity, impact, preſſure, &c. are ſources of motion, or of modifications of motion; and hence, without pretending to know any thing of the eſſence of either of theſe, we do not heſitate to call them mechanical forces; becauſe it is incontrovertible that bodies expoſed to the free action of either, are put into motion, or have the ſtate of their motion changed. Forces therefore, being only known to us by their effects, can only be meaſured by the effects they produce in like circumſtances, whether thoſe effects be creating, accelerating, retarding, deflecting, or preventing motions: and it is by comparing theſe effects,

Definition of the terms force and power.

Forces only known to us by their effects, thence meaſureable.

or by referring them to ſome common meaſure of ready appreciation, that mechanics is made one of the mathematical ſciences *.

Animal efforts are a *ſpecies* of mechanic power.

Theſe obſervations will enable us to ſet Mr. H. "right as to the identity of *animal exertion* and *mechanic power*." Animal efforts are juſtly conſidered, both by the Mathematician and the practical Engineer, as conſtituting *one ſpecies* of mechanic power; when theſe efforts give to bodies equal momenta, or give to equal bodies equal velocities, it is truly ſaid the animals exert equal forces; and we ſay that animal power is greater or leſs, as it is capable of imparting to bodies greater or leſs momenta, or as it is capable of ſtopping bodies moving with greater or leſs momenta: and the language of ſcientific men is analogous to this when they ſpeak of any forces whatever.

Reply to Mr. H's remarks on a former ſtatement.

It is now time to proceed to Mr. Hornblower's animadverſions upon the inſtance I adduced (and many others might be adduced) to ſhew that Mr. Smeaton's meaſure of mechanical power and effect is not univerſally applicable. I aſſerted,

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* That Mr. Hornblower may not reſt upon the mere authority of any theoretic man, I beg to throw into this note an extract from the *Mechanique Philoſophique* of M. Prony, an Engineer, who unites with a profound acquaintance with the theory, an extenſive practice, and whoſe example in this reſpect I ſhould ſincerely rejoice to ſee more frequently imitated in this country.

"La nature de cette cauſe de mouvement, nommée *force* ou *puiſſance*; nous eſt tout-à-fait inconnue: l'homme appelle *force* la faculté organique qu'il a de ſe mouvoir, de s'arrêter, de produire ou de faire ceſſer le mouvement des corps qui l'environnent; et ſans ſavoir en quoi conſiſte cette faculté, il a ſuppoſé qu'il exiſtait quelque choſe de ſemblable dans les agens phyſiques qui ſont ou qu'il croit être, ſur le globe terreſtre et dans l'univers, les cauſes du mouvement de différens corps. Mais nous n'avons en mecanique, aucun beſoin de connaître la nature de la *force* ou *puiſſance* qui eſt repréſentée, meſurée et introduite, dans le calcul, uniquement par les *effets* qu'elle produit. Ces effets ſe réduiſent toujours à des vêteſſes que les puiſſances ou tendent à donner, ou ont effectivement données à de certaines maſſes." "Parmi les diverſes puiſſances que la nature nous offre, il en eſt une très remarquable dont il convient de prendre les effets pour terme de comparaiſon de ceux des autres puiſſances; c'eſt la *peſanteur terreſtre* a la ſurface de la terre," &c. p. 20.

that

Reply to Mr. H's remarks on a former ſtatement.

that a horſe ſtanding ſtill and ſuſtaining a weight which hung by a cord over a fixed pulley, would, after a due interval of time, be completely fatigued, although neither the animal nor the weight moved, and that, of conſequence, there was a power expended, of which Mr. Smeaton's rule did not furniſh an adequate meaſure. Mr. H. as though he underſtood me to affirm, that fatigue was the *only* indication of mechanical power expended, inſtead of limiting it to animal efforts as the connection evidently required, exclaims, " it is really difficult to be grave on this occaſion:" p. 268. and argues in a kind of exulting ſtrain which ſavours a little, I am afraid, of the ſpirit alluded to in the French proverb, *Chanter le triomphe avant la victoire!* Let us, ſays this gentleman, have a "*poſt* inſtead of the horſe, and ſurely that will not tire, and what will be the conſequence then? why then there will be no power expended, and no effect produced." Mr. H. then, it would ſeem, has forgotten that the poſt is retained in *its* ſituation by a force which in this caſe oppoſes that of gravity acting upon the ſuſpended weight. The cord running over the pulley and ſuſtaining the weight, being faſtened to the poſt, would move it, were it not that the coheſive force of the earth in which the poſt is fixed, changes the ſtate into which the poſt would be brought by the action of gravity upon the weight, and is ſufficient to retain the whole at reſt. If the poſt were ſet in looſe ſand, or in a quagmire, the weight would draw it away, and then I ſuppoſe, even according to Mr. H's notion, there would be a power expended, and an effect produced. So likewiſe, in Mr. H's other example, of the hat hung upon the pin, the force of gravity is balanced by the coheſive force of the wood or other matter, into which the pin is fixed. But it would be egregious trifling to dwell much longer upon ſuch inſtances as theſe. Mr. H. conceives, if I have not completely miſunderſtood his meaning, that there is no " mechanical power" that is not " made up of a maſs of matter moving with a determinate velocity;" and as ſuch an opinion muſt either ariſe from neglecting to diſcriminate between cauſe and effect, or from a virtual denial of the whole doctrines of ſtatics, (in which powers are excited without any motion being produced,) I ſhall hope to be excuſed though I waſte no time on a refutation of any ſuch poſition.

Indeed,

Familiar illuſtration.

Indeed, much of Mr. H's reaſoning, not only with reſpect to the poſt and the hat-peg, but throughout his paper, ſeems to reſt upon a tacit admiſſion (not a direct avowal, it is true,) of the erroneous notion, that forces exerted by animated beings, and thoſe operating through the intervention of inanimate things, are totally diſtinct, and cannot be ſubſtituted the one for the other, or have a fair compariſon inſtituted between them. Whereas, on the contrary, not only the theory but the practice of mechanics, proceeds upon the principle, that thoſe forces are equal in degree, however different in their origin, or various in their mode of operation, which produce equal effects. Thus, for a familiar example, in the boring of a piece of ordnance; the borer may either be brought up to its proper poſition in the gun by the action of a man on the handles of a wheel, connected with the borer by rack and pinion work, or by the action of a weight attached to the farther end of a lever proceeding from the axle of the ſame wheel: and Mr. H. might as well deny the poſſibility of the work performed being the ſame in both theſe caſes, as deny that a weight is kept from falling by an equal force, when prevented either by an animal, or by a fixed inanimate object; or deny that there is an expenditure of mechanic power when a man counteracts the operation of gravity upon his arm, when extended horizontally. While ſpeaking of the propoſition which includes any ſuch denial, we may ſafely apply to it Mr. H's own language;—" A more erroneous propoſition was never introduced into the theory or practice of mechanics."

Miſtake of Profeſſor Robiſon.

Mr. Hornblower has taken the trouble of extracting ſeveral paſſages from the Article MACHINERY, *Sup. Ency. Britan.* and among them has taken that which exhibits Profeſſor Robiſon's meaſure of the exertion of a man, who walks at the rate of 60 feet per minute, and raiſes a weight of 30 pounds. The meaſure 57600, which this gentleman thinks enormouſly too large, is, in fact, too ſmall, in ſo far as it does not include that part of the exertion required by the man to move himſelf. It was this omiſſion of the learned Profeſſor that induced me to lay down the general ſtatement at p. 152 of your 43d Number, though I thought it might be deemed invidious if I ſpecified my motives in that place. But when Mr. H. had commenced the labour of extracting, it would ſurely

Robiſon on Smeaton's meaſure.

ſurely have been but candid to produce a paſſage which ſtrikes with great force againſt the univerſality of Mr. Smeaton's meaſure, at the ſame time that it admits the utility of this meaſure to engineers in many caſes. This paſſage is as follows:—"When a weight of five pounds is employed to drag up a weight of three pounds, by means of a thread over a pulley, it deſcends with a motion uniformly accelerated, four feet in the firſt ſecond. Mr. Smeaton would call this an expenditure of a mechanical power 20. The weight three pounds is raiſed four feet. Mr. Smeaton would call this a mechanical effect 12. Therefore the effect produced is not adequate to the power expended. But the fact is, that the preſſure, ſtrain, or mechanical power, really exerted in this experiment, is neither five nor three pounds; the five pound weight would have fallen 16 feet, but it falls only four. A force has therefore acted on it ſufficient to make it deſcribe 12 feet in a ſecond, with a uniformly accelerated motion, for it has counteracted ſo much of its weight. The thread was ſtrained with a force equal to 3¾ pounds, or ¾ of 5 pounds. In like manner, the three pound weight would have fallen 16 feet; but it was raiſed four feet. Here was a change preciſely equal to the other. A force of 3¾ pounds acting on a maſs whoſe matter is only three, will in a ſecond, cauſe it to deſcribe 20 feet with a uniformly accelerated motion. Now 5 × 12 and 3 × 20, give the ſame product 60. And thus we ſee, that *the quantity of motion extinguiſhed or produced, and not the product of the weight and height, is the true unequivocal meaſure of mechanical power really expended, or the mechanical effect really produced; and that theſe two are always equal and oppoſite.* At the ſame time, Mr. Smeaton's theorem merits the attention of engineers; becauſe it generally meaſures the opportunities that we have for procuring the exertion of power. In *ſome* ſenſe, Mr. Smeaton may ſay, that the quantity of water multiplied by the height from which it deſcends in working our machines, is the meaſure of the power expended; becauſe we muſt raiſe this quantity to the dam again, in order to have the ſame uſe of it. It is expended, but not employed, for the water at leaving the wheel is ſtill able to do ſomething."

Meſſrs. Hornblower and Smeaton both concede the point in debate.

In oppoſition to all this, Mr. H. I ſuppoſe, would ſay that this is not a caſe in point, becauſe, "if the weight deſcends quickly it is ſenſibly compounded with another law, viz. the acceleration of gravity." Or, adopting other language of Mr. Smeaton, he might reſtrict his meaſures to "the height through which a body *ſlowly* and *equally* deſcended, or to which it was raiſed." But if, inſtead of the body's aſcending or deſcending ſlowly and equally, it moved rapidly and irregularly; or, if the motion was reciprocating, the velocity increaſing from quieſcence to a certain magnitude, and diminiſhing to quieſcence again; or, if we refer to the retarded riſe and accelerated fall of heavy ſtampers; in ſuch caſes if Smeaton's meaſure be applicable, I wiſh to ſee its manner of application explained; and if it be not univerſally applicable, a point which is, in reality conceded both by Mr. H. and Mr. Smeaton, there is then as to this head no ground of difference between us, and Mr. H's laſt letter becomes in a great meaſure a ſuperfluous labour; for, admitting the want of univerſality in the rule, is admitting all that I affirmed. Had not the meaſure been often very injudiciouſly exhibited as *univerſal*, a thing which Mr. Smeaton himſelf certainly never intended, I ſhould not have at all referred to it in my former paper.

In correct language *weight* and *heavineſs* ought to be diſtinguiſhed.

It may be deemed a ſlight deviation from the immediate object of this letter, but I truſt a juſtifiable one, if I briefly notice the ſurprize expreſſed by Mr. H. on account of Profeſſor Robiſon's diſtinguiſhing between weight and heavineſs. That the three terms *gravity*, *weight*, and *heavineſs*, admit of a palpable and obvious diſtinction, is, in my opinion, indubitable: and till this time, I imagined it was univerſally reckoned one great excellence of an accurate philoſophical diſquiſition, that it compriſed a careful diſcrimination of the various acceptations of theſe and other terms, which were commonly reputed ſynonimous. There may, undoubtedly, be occaſions in which a cautious ſelection from words of nearly ſimilar import may be diſpenſed with: but there are many more, particularly when handling philoſophical topics, where this careful choice cannot be ſafely neglected. And an attention to this point appears the more neceſſary, when it is recollected, that greater part of the controverſies which have been agitated by men of ſcience, have been rather verbal, than relative to things in themſelves. To contend for the uſe of many terms to expreſs one idea, inſtead of ſeeking for adequate ſeparate expreſſions to denote every idea the mind can form, is to ſacrifice preciſion and accuracy at the ſhrine of an ill-judged ſuperfluity. The common reſemblance between words eſteemed ſynonimous, does not comprehend the aggregate ſignification, but ſome iſulated particular attendant upon all, in ſome ſuch manner as may be traced in individuals of the ſame ſpecies: there is generally one, if not more qualities, on which a manifeſt diſtinction depends; and the determination of ſuch qualities is highly deſerving the notice, not only of the linguiſt, but of all who aim at philoſophical preciſion. I have not leiſure to look attentively over twenty-five cloſely printed quarto pages, in order to find how Profeſſor Robiſon diſtinguiſhes heavineſs from weight. But the labour is unneceſſary; for the diſtinction has often been made; and I will take the liberty of delineating it in the words of an author who is in no danger of having his ſentiments warped to ſquare with the tenets of any ſpeculative mechanical ſyſtem: I now advert to Dr. Truſler, who in his work on ſynonimous words ſpeaks thus:—

Truſler's remarks on heavineſs and weight.

" *Heavineſs, weight.*—In the figurative ſenſe the difference of theſe words is ſo extremely great, as needs no pointing out; in the literal indeed, they are often confounded: conſidered then in this laſt ſenſe, *heavineſs* is that quality in a body which we feel, and diſtinguiſh by itſelf: *weight* is the meaſure and degree of that quality, which we cannot aſcertain but by compariſon.—We ſay abſolutely, and in an undetermined ſenſe, that a thing is *heavy;* but relatively, and in a manner determined, that it is of ſuch a *weight*, for example, of two, three, or four pounds.—A thouſand circumſtances prove the *heavineſs* of the air; and the mercury determines its exact *weight.*" Vol. I. p. 133.

My letter has attained a much greater magnitude than was at firſt intended, and I will now conclude it. The remarks I have been tempted to offer, are founded upon the moſt correct interpretation I could put upon Mr. Hornblower's language; and if I have any where miſunderſtood his meaning, I ſhall be pleaſed to ſee that miſunderſtanding candidly removed. I entertain great reſpect for that gentleman's talents as a practical engineer; though I cannot but think him completely wrong in moſt of thoſe remarks which have occaſioned this communication. I have replied to ſuch of his ſtrictures as bore any relation to myſelf, I believe, without acrimony: but I have a deeply rooted averſion to every thing that wears the garb of controverſy, and ardently hope the diſcuſſion on my part will be permitted to terminate here.

I am, Sir,
Your's, with much reſpect,
OLINTHUS GREGORY.

Royal Mil. Academy,
Woolwich, Aug. 9, 1805.

IV.

Deſcription and Effects of an Apparatus for raiſing Water by Means of Air condenſed in its Deſcent through an inverted Syphon. By Mr. WILLIAM CLOSE. *From the Inventor.*

To Mr. NICHOLSON.

SIR, *Dalton, July* 27, 1805.

Reference to the author's ſyphon engine.

IN one of my letters, ſome time ago, I briefly noticed an experiment I had made, to determine the practical value of the hydraulic machine, or inverted ſyphon, repreſented and deſcribed in the firſt volume of the preſent ſeries of your Journal *, obſerving, that, at ſome future period, I might probably tranſmit to you a more particular account. Having ſince repeated the experiment, I now ſend you a letter upon the ſubject, for I am of opinion that a machine operating upon the principle, when conſtructed in the manner herein deſcribed, will anſwer very well, in certain ſituations, to raiſe water for domeſtic purpoſes; and although it may not be competent to perform half as much work as a bucket engine by a forcing pump, yet it may be kept continually employed, and be ſubject to very little wear, as its operation will almoſt be performed without friction.

Deſcription of another apparatus;

The inverted ſyphon when applied to raiſe water in the manner deſcribed in this letter, has its higher orifice placed in a ſituation to receive both air and water at the ſame time. The air being conveyed by the velocity of the aqueous column to the loweſt part of the ſyphon, and collected in a veſſel, is employed as the medium for conveying preſſure to raiſe water in another part of the apparatus.

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* See Philoſ. Journal, Vol. I. p. 30, Pl. IV.

In May 1803, I determined to find by experiment, under what degree of preſſure it would be moſt advantageous to collect the condenſed air, and likewiſe the proportion then exiſting between the two fluids moving in the ſyphon. The apparatus conſtructed for this purpoſe, is repreſented in *Plate I. Fig.* 1. It required only a ſmall ſupply of water, but condenſed the air ſufficiently to be employed in the actual conſtruction of a machine upon the principle.

being an inverted ſyphon, which carries down air and condenſes it;

A round vertical pipe A B, half an inch in diameter, and 22 feet 5 inches in length, had its higher end placed in the ciſtern A, and its lower connected to a ſmall oblong veſſel C, which had an inverted glaſs bottle cemented upon a projecting cylinder on its upper ſide. From the other end of the veſſel aſcended another vertical pipe D E, half an inch in diameter, and 18 feet 3 inches in length, and terminated in a crook, 4 feet 2 inches below the higheſt part of the pipe A B.

The whole apparatus being filled with water, the ciſtern having a conſtant ſupply ſufficient to keep the ſurface of the fluid juſt above the orifice of the pipe A B, when the orifice of emiſſion at E was opened, the water flowing through A B, carried bubbles of air into the veſſel C, which aſcending, diſplaced the water in the bottle, and afterwards that contained in the veſſel C, above the lower ends of the pipes A B and D E. At the firſt efflux, and after the deſcent of every material portion of air, the jet at E was projected ſeveral inches from the adjutage, but its curve decreaſed during the deſcent of more air; for the bubbles did not riſe inceſſantly into the bottle, but after ſhort intervals of reſt, diſlodging two or three ounces of water each time, with a guggling noiſe, which was very audible to the perſon regulating the ſupply of the ciſtern. After the water in the veſſel C was depreſſed to a level with the ends of the pipes, the denſe air carried down A B, aſcended through D E, and cauſed frequent interruptions in the jet; for, expanding under a light preſſure, it expelled the water in the higheſt part of the pipe with violence, and then the efflux ceaſed for ſome time after.

and this air is afterwards uſed for raiſing water.

The condenſed air, however, could any time be let out, by a ſmall pipe which was placed within the bottle, and opened on the outſide of the veſſel C.

The pipe A B had a joint above the bottom of the ciſtern, to facilitate the trial of mouth-pieces of various forms, to find by which the apparatus would ſip the moſt air: and it appeared that no form, or poſition, conduced more to this effect, than when the pipe was crooked at top to receive the water in a horizontal current, and the higher ſide of its orifice was not more than two lines below the ſurface of the water in the ciſtern. It alſo appeared, that no leſs quantity of air was collected, when the diameter of the orifice of emiſſion was reduced to four lines, than when it was half an inch.

Experiments to ſhew how much air can be carried down with he water.

After ſeveral experiments to determine the quantity of water requiſite to ſupply the expenditure from the ciſtern, and keep the ſurface of the fluid accurately at the height beſt adapted to the operation of the apparatus; ſeveral trials were made to aſcertain the quantity of air a given quantity of water would convey into the bottle in a given time. The reſults of ſeveral trials on the 21ſt of May 1803, were as follow:

1. The fall being 50 inches, and the orifice of emiſſion four lines in diameter, the inverted bottle above C, holding ten ounce meaſures of water, was filled with air, under the preſſure of a column 18 feet high, by 14 pints of water flowing out at the orifice of emiſſion at E, in 145 ſeconds. 2. By 13 pints, in 133 ſeconds. 3. By 12½ pints, in 125 ſeconds. By 12 pints. 5. By 11 pits, in 95 ſeconds. 6. By 14 pints, in 114 ſeconds. 7. By 12 pints, in 102 ſeconds. 8. By 12 pints, in 108 ſeconds.

9. The orifice of emiſſion being half an inch in diameter, the bottle was filled by 12 pints. 10. By 13 pints, in 133 ſeconds. 11. By 12¼ pints.

12, 13. The fall being 44 inches, the orifice of emiſſion four lines in diameter, 11 pints filled the bottle in 95 ſeconds; and 14 pints, in 120 ſeconds.

With an height of 18 feet and fall 50 inches, 20 parts of water carried down one of condenſed air.

The difference in the time, and the quantity of effluent water required to fill the bottle with air, in theſe trials, was probably occaſioned by a portion of the air being ſometimes contained in the higher, and at other times in the lower part of the pipe A B, at the commencement of the effuſion: or, perhaps, in part, by the water in the ciſtern not being always of the ſame height; for the ciſtern did not overflow, but was ſupplied with great care, ſometimes by a pump, and ſometimes by letting water out of a veſſel; always keeping the ſupply from agitating the contents of the ciſtern as much as poſſible. Had the bottle been larger, there had probably been more uniformity in the reſults of the trials. In eſtimation, I think, however, we ſhall not overrate the operation of this machine, by taking 13 pints for the mean quantity of effluent water emitted while the bottle was filling with air; and then deducting the quantity expelled from the bottle, it will appear that 20 parts of water carried one of air down the pipe A B: and as one ounce meaſure of condenſed air at leaſt was collected in 14 ſeconds, ſo 16 pints would collect every hour.

Trial with a greater length of ſyphon:

Some few days after theſe experiments, the pipe A B was lengthened to 24 feet 7 inches, and D E to 20 feet; but upon trial, the air was carried into the bottle ſo much ſlower than before, that a ſuſpicion aroſe that ſome part of the apparatus was not air-tight; and on this ſuppoſition the pipes were taken down.

Leſs air was carried down.

In February 1804, the pipes, &c. were examined, and ſet up again with conſiderable care. A B was 24 feet 7 inches long; D E, 21 feet one inch; conſequently the difference for the fall was 3 feet 6 inches. With this apparatus, when the diameter of the higher orifice of the pipe D E was four lines, it appeared by four trials (Feb. 25, 1804), that the bottle loſt only one ounce of water per minute.

When the pipe D E was ſhortened to 19 feet 7 inches, and had its higher orifice five feet below the ſurface of the water in the ciſtern, four ounce meaſures of condenſed air deſcended into the bottle, during the emiſſion of 16 pints of water, through the orifice at E, when half an inch in diameter.

The diminution in the collection of air, in theſe laſt experiments, was much more conſiderable than was expected to happen, either from the abſorption of the water, or the increaſed condenſation of the air, which might be occaſioned by ſo ſmall an addition being made to the apparatus. The jet at E was projected more ſteadily in theſe laſt, than in the preceding trials; and the condenſed air, inſtead of riſing into the bottle in large detached bubbles, aſcended in a continual ſtream, like the evolution of gas from the bottom of an effervefcing mixture. From the minute diviſion of the air, it is not improbable, that a ſmall portion might be carried along with the current of water under the bottle, and aſcend through the pipe D E; but this was not determined. If there was no defect in the apparatus, it appears, that it will not be ſo advantageous, in the conſtruction of a working machine upon this principle, to employ a condenſing column ſo heavy as 24 feet, as one that is lighter.

Other trials.

Feb. 28, 1804. The pipe A B being ſhortened to 22 feet 5 inches, and D E to 18 feet 3 inches, the orifice of the adjutage at E being four lines in diameter, the bottle was emptied by 13, 12½, 14, 13, 13, 12, and 14 pints of effluent water, in ſeven ſucceſſive trials, as in thoſe of May, 1803.

March 9. With a fall 3 feet 9 inches, A B being 15 feet one inch, half a pint of air was collected during the diſcharge of 12½ pints of water. Again, the orifice of emiſſion being four lines in diameter; the fall 4 feet 2 inches; A B 15 feet 8 inches; D E 11 feet 6 inches; nine ounce meaſures of air were conveyed into the bottle, in one minute, during the diſcharge of 10 pints of water, in five ſucceſſive trials: and when the diameter of the higher orifice of the pipe D E was half an inch, the ſame quantity of air was carried down in 50 ſeconds, by nine pints of effluent water, including that diſplaced from the bottle. When A B was 15 feet 8 inches; D E 13 feet 2 inches; the fall 2 feet 6 inches; nine ounce meaſures of air were collected in the bottle, by the diſcharge of 16 pints, in 90 ſeconds; in 105 ſeconds, by 18 pints; and again, by 18 pints, in 90 ſeconds.

March 16. The fall being 2 feet; A B 8 feet 5 inches; D E 6 feet 5 inches; and the diameter of its higher orifice half an inch; ten ounce meaſures of air were collected in one minute; again in 64 ſeconds, when the effluent water meaſured 10 pints; and again in one minute, when 10 pints.

Having now ſhewn what power a machine operating upon this principle may be expected to poſſeſs, I proceed to ſhew how its principle may be applied to practice.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

Fig. 2, *Plate I.* exhibits a machine for raiſing water above the ciſtern.

R repreſents a ciſtern ſupplied by a ſpring, where there are four or five feet fall for the water.

W W, a well or pit ſituated below the bed or lower level of the ſtreamlet; its depth varying from 6 to 20 feet, according to the elevation to which water is to be raiſed above the ciſtern, and the number of progreſſive columns by which it is to aſcend.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

A A, a pipe leading from the ciſtern R, to a bell-ſhaped veſſel B, fixed a little above the bottom of the well, with its mouth downwards. The top of the pipe is crooked, as repreſented at A, *Fig.* 5, and there is a joint below, which allows the crooked part to be detached from the reſt. The lower end of this pipe is alſo crooked, and turns under the ſide of the veſſel B.

C C, a pipe fixed into the top of the veſſel B, and carried a little above the ciſtern, where two ſmaller pipes, E and G, are connected with it by a ſtop-cock.

E, a ſmall pipe leading to F, a veſſel or chamber, placed in the ciſtern R.

G, another ſmall pipe leading to H, a veſſel or chamber, ſomewhat leſs than F, placed in a higher ſituation. This pipe has a turn, a foot above the top of the veſſel H.

I, a pipe leading from the ciſtern R to the veſſel F.

K K, a pipe deſcending a foot or more below the veſſel F, and then aſcending to the veſſel H.

L, a pipe connected to K, a foot below H, and then carried to the conduit or ciſtern which receives the raiſed water.

The pipes I, K, L, have each a valve opening upwards.

The conſtruction of the cock is repreſented in *Fig.* 3. The conical barrel has four holes, C, E, G, O, and the turning part or key, has a notch, or hollow, on each ſide in that part which moves oppoſite thoſe holes, ſo that the pipes C and E, or C and G, may be connected by a quarter of a turn. When the communication opens between C and E, the external air has acceſs to the inſide of the pipe G, and to the chamber H above, through the opening O; and when C is joined to G, the air is admitted into the chamber F, through the opening O, and the pipe E.

In the narroweſt part of the veſſel B, a buoy or float is fixed upon the elevated end of a crooked lever, moving upon a horizontal axis, ſupported by pieces attached to the ſide of the veſſel. Inſtead, however, of a common hollow buoy, it will be preferable to uſe a body ſpecifically heavier than water, and by

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

by adding weight to the other end of the lever, to make ſuch adjuſtment, that while both are immerſed in water, the body within the veſſel B, ſhall be a few ounces lighter; and, on the contrary, when it alone is above the water, it ſhall be ſo much heavier than its counterpoiſe, which is covered. A ſmall cylindrical veſſel 2½ inches in diameter, and the ſame in depth, filled with water and cloſed, will probably be ſize ſufficient for ſuch a float; and the proper counterpoiſe may be very readily and eaſily found, by having the lever fixed on its axis in a veſſel of water, repeatedly drawing off and replacing the fluid in that part of the veſſel which contains the float; and increaſing or diminiſhing the weight, until the proper adjuſtment is obtained. But to proceed with our deſcription.

S repreſents a ſmall ſyphon ſuſpended by a lever, with one branch in the inſide, and the other on the outſide of the ciſtern R. The outſide branch being re-curved in the manner repreſented in *Fig.* 5, it is evident, that when the inſtrument is filled, it will draw water out of the ciſtern whenever the orifice of the re-curved branch is depreſſed below the level of the water in the ciſtern; and that its operation will be ſuſpended by raiſing the ſame a very little above that level. From the contrary end of the lever, a chain or wire deſcends, and is connected to the lever which carries the float; and by this connection, the ſyphon is ſuſpended with the orifice of its re-curved branch above the ſurface of the water in the ciſtern, while the float occupies the higheſt part of the veſſel B; for the weight of the ſyphon and that of the included column muſt be ſo nearly counterpoiſed by the chain and an additional weight, that it cannot depreſs the float, though it muſt poſſeſs ſufficient weight to deſcend when allowed by the deſcent of the float.

M repreſents two cuneiform buckets, connected at their baſes by a tranſverſe partition, and fixed upon a horizontal axis, as is more clearly exhibited by the ſection, *Fig.* 4. When the bottoms of theſe are placed in an oblique direction, making an angle with the horizon of 25 or 30 degrees, as repreſented in the drawing, (*Fig.* 2.) and a ſmall ſtream of water falls from the ſyphon, the higher bucket will receive the water, and falling in conſequence of its load, will raiſe the other bucket, which will now receive the water, and by falling will raiſe the firſt, whoſe contents were emptied in its deſcent. A ſmall arm or lever is fixed to the axis of this apparatus, and connected by a piece of ſtrong wire to another ſhorter lever fixed upon the ſmaller end of the turning part of the cock C, and by this means the cock moves with the alternate motion of the buckets M, when ſupplied with water.

Deſcription of the machine as conſtructed for raiſing water, by air condenſe in an inverted ſyphon.

When the ſpace from the top of the veſſel B, to the ſurface of the well is equal to 18 feet, the top of the chamber H, above the bottom of F, and the perpendicular height of the pipe L, above the bottom of the chamber K, may be each 18 feet.

The valves and every other part of the apparatus being in proper order for work; the well being filled with water; and the reſervoir R conſtantly ſupplied, ſo as to overflow in one part ſomewhat lower than the reſt of the brim, while the higher orifice of the pipe A A, is about two lines below the ſurface of the water, and takes in its full quantity; the manner of bringing this machine into action, and its operation afterwards, may be underſtood by attending to the following directions, and ſtatement of particulars:

Open the pipe E which leads to the chamber F, by turning the cock C, and water will deſcend into F from the ciſtern R, by the pipe I: When the chamber is full, place the two connected buckets M, in a horizontal poſition, and the cock C, if properly conſtructed and connected with theſe buckets, will cut off all communication between the pipe C C and the pipes E G. The air carried down the pipe A A, by the column of water which deſcends and keeps the well conſtantly overflowing, will aſcend into, and gradually expel a quantity of water from the pipe C C, and afterwards that contained in the higher part of the veſſel B alſo. The float receiving an acceſſion of weight by being out of water, will deſcend and let down the re-curved ſyphon S, which will pour water upon the buckets below. At this period depreſs that bucket which by deſcending opens a communication between the pipes C and E, and no farther attendance will be requiſite.

The preſſure of the column in the well, above 18 feet high, being thrown upon the water in the chamber F, by the intervention of the condenſed air in the pipe C, the valve in the pipe I will be ſhut, and water will aſcend from the chamber F through the pipe K K into H; the ſyphon S will alſo aſcend into its place, and ceaſe to draw water from the ciſtern.

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

ciſtern. An equilibrium being ſoon eſtabliſhed between the acting column in the well, and the re-acting one included in the pipe K K and the chambers F, H, by the expulſion of the air from B, the water will afterwards aſcend much ſlower into H than at firſt, an equal depreſſion of the fluid being produced in the pipe C and the chamber F, by the collecting of the condenſed air.

In more or leſs time, according to the capacities of the chambers, ſo much water will be expelled from the lower as will fill the higher, the air having been expelled from this laſt, through the pipe G, whoſe outlet is at O in the cock C E G O, *Fig.* 3. Water will then begin to aſcend into that part of the pipe G which riſes from the top of the veſſel, and the acting column being lengthened in proportion to the increaſing height of that which it counterpoiſes, the condenſed air will depreſs the water in the veſſel B below the float, which deſcending, will lower the re-curved ſyphon, and water will fall into the elevated bucket M, which ſoon afterwards in conſequence of its load will deſcend, and by moving the cock above, will open a communication between the pipes C and G, and between the inſide of the chamber F and the external air, when the condenſed air will ruſh out, and this veſſel refill with water from the ciſtern.

The force of the acting column being now thrown upon the contents of the higher chamber H, the valve in the pipe K will cloſe, and water will aſcend through the pipe L, into the ciſtern appropriated for its reception.

At the firſt opening of the communication between B and H, the re-curved ſyphon S will pour water into the bucket laſt elevated; but before the load is ſufficient to move the apparatus, the ſyphon, if properly adapted to its purpoſe, will be drawn up again, in conſequence of the condenſed air being expelled faſter from the veſſel B, than it deſcends by the pipe A A: for the water will always riſe with the greateſt rapidity after the turning of the cock, becauſe of the difference ſubſiſting between the acting and re-acting columns, and the air previouſly ſtored up in the veſſel B. The ſupply of condenſed air, however, being inadequate to ſupport the difference, an equilibrium ſoon takes place, by the water aſcending into the loweſt part of the pipe C.

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Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

The ſupply of condenſed air will now continue to force water out of the chamber H, and to depreſs the fluid in the pipe C, in an equal degree, until all the water in the chamber is expelled. The water will then ſink in the higheſt part of the pipe K, and the acting and re-acting columns proportionally lengthening, air will collect in the veſſel B; the float and re-curved ſyphon will deſcend; and before the depreſſion of water in the pipe K reaches the lower end of the pipe L; before the condenſed air can eſcape by expelling the water contained in this pipe, the elevated bucket M will fall in conſequence of water poured into it by the ſyphon, and the communication between C and E being opened, the condenſed air will ruſh out of H, through the open pipe G, whoſe outlet is at O in the cock C E G O, *Fig.* 3; the valve in L will ſupport the water above it, and the force of the acting column being again thrown upon the water in the chamber F, the fluid will begin to aſcend into K; the re-curved ſyphon will riſe and continue in its place as before, until the water begins to fill that part of the pipe G which riſes from the top of the chamber H, or, if there cannot be a ſufficient quantity raiſed from the lower chamber to fill the higher, until it begins to be depreſſed into that part of the pipe K, connected with the bottom of the chamber F; the ſyphon will then be let down by the float, will pour water into the bucket laſt elevated, and thus again open the communication between the air-holder B C, and the higher chamber, from which the water will be expelled; and in this manner the alternations will proceed, the machine continually raiſing water from one or other of its chambers.

By additional pipes and chambers, ſimilar to G H L, a ſmaller quantity of water may be raiſed ſtill higher, the loweſt additional chamber being fixed upon the top of the pipe L, and the pipe for ſupplying it with condenſed air, connected to the pipe E. A pipe for ſupplying a chamber ſtill higher muſt be attached to the higheſt part of the pipe G; and it is evident, that if E leads to two chambers and G only to one, the machine will regulate its operations ſo as to loſe no time. It will be requiſite, however, to fill the additional chambers with water before the machine is ſet to work.

Where the ſupply of water is variable, the machine may be adapted by having ſeveral pipes ſimilar to A A, but ſome wider

Deſcription of the machine as conſtructed for raiſing water, by air condenſed in an inverted ſyphon.

wider and others ſmaller, and by ſetting ſuch of theſe to work as are requiſite, for whatever may be the increaſe or diminution of power, the turning of the cock will be duly regulated by the float.

The water which ſupplies the pipe I, and aſcends in the apparatus, ſhould be cleared, by filtration, from impurities and ſubſtances that would obſtruct the cloſure of the valves. The ciſtern ſhould have a moveable piece at the place where the water overflows, to accommodate the ſurface of the fluid to the ends of the pipes, that the full quantity of air may deſcend with each column, and that the maximum of effect may be obtained from the ſupply.

To determine how well ſuch a combination as that I have deſcribed would anſwer the purpoſe, I had a model conſtructed upon the plan exhibited in *Plate I. Fig.* 2; but having no convenience for an overflowing well or ciſtern, I was obliged to modify ſome parts, in a manner tending to diminiſh its power.

The water from the ciſtern R, falls into a capacious veſſel, from whence, when the machine is at work, a hand-pump continually raiſes it again into a veſſel above, which ſupplies the ciſtern R through a pipe nearly half an inch in diameter, under a conſtant preſſure of 3½ inches charge. The ſupply keeps the ciſtern continually overflowing, and the ſurface of the water is calm and always at the ſame height.

The pipe A A is 8 feet 3 inches in length, and half an inch in diameter. Its lower end is inſerted into the veſſel B, which is cloſed at bottom, and conſtructed of ſuch a form as to include the lever which carries the float. Above that end of the lever bearing the counterpoiſing weight, a vertical pipe 6 feet 3 inches, is connected to the top of the veſſel; and through this pipe, which is no wider than A, the water aſcends and flows to the pump: A chain, conſiſting of pieces of wire four or five inches in length, looped together by the ends, alſo paſſes through it, and connects the float-lever to that which moves the re-curved ſyphon S.

The bottoms of the buckets M are both together 14 inches long and 6 broad. The baſe partition is 4 inches high. Each bucket has an end parallel with the baſe, one inch deep, provided with a hole to let out the water when depreſſed. The cock moves with leſs than a pint of water in the elevated bucket.

The float in the veſſel B, is a ſmall cylindrical copper veſſel, one inch in height and two inches in diameter. It was filled with water and cloſed before it was fixed in its place. Though equal in bulk only to one ounce and three quarters of water, yet it is quite ſufficient to move the ſyphon which would work a larger machine.

Deſcription of the machine as conſtructed for raiſing water, by air condenſe in an inverted ſyphon.

The pipe C is half an inch in diameter: E, K, G, L, are ſmaller. The valves are leather.

This machine, when in good order, raiſes water nearly 12 feet above the ciſtern, at the rate of 20½ pints per hour, and performs all its operations as well as can be deſired. When firſt ſet to work, the cock is ſo placed as to cloſe the top of the pipe C, until the condenſed air begins to collect in the veſſel B, and then the communication is opened to the chamber F. If the communication was open at firſt, the water would be expelled from F into the ciſtern, while the preſſure was inſufficient to cloſe the valve in the pipe I.

The chambers F H being ſmall, the ſyphon moves frequently; but in a working machine theſe veſſels ſhould not only be broad and ſhallow, but capacious, that the wear of machinery may be reduced to its utmoſt extent.

To determine what quantity of water flows through the apparatus, I fixed a ſpout upon the top of the aſcending water pipe; but in doing this I entangled the float in the veſſel B, that it could not be made to work the ſyphon. The effluent water, in this unemployed ſtate of the machine, including half a pint diſplaced from the veſſel B, amounted to 8½ pints. The ſuperfluous water from the ciſtern R meaſured 8½ pints alſo. If the machine had been working the waſte water would have been leſs, as part would have been drawn off by the ſyphon.

From ſeveral trials, this model appears to raiſe water above the rate that might be eſtimated by the experiment of March 6, previouſly related.

In actual practice, I think the allowance for waſte and working the buckets, of one third or perhaps only of one fourth of the ſupply, will be ſufficient; then ſuppoſing the apparatus ſo adapted to the ſupply, as 28 or 30 gallons from the ciſtern will raiſe one gallon 18 feet, ſo 84 or 90 gallons will raiſe one gallon 44 feet, by three aſcending columns.

The bucket-engine at Irton-Hall, in Cumberland [*], is ſaid to raiſe one gallon of water 60 feet high by 36 gallons ſupply; hence, if the waſte water be included, it appears that our machine will not be competent to perform half as much work by the ſame ſupply, and its peculiar advantages muſt depend upon its durability when conſtantly employed.

I am, Sir,
Your's reſpectfully,
WILLIAM CLOSE.

V.

Remarks on COUNT RUMFORD'S *Experiments relating to the Maximum Denſity of Water. In a Letter from* Mr. JOHN DALTON.

To Mr. NICHOLSON:

SIR,

Count Rumford's experiments on the max. denſity of water conſidered.

IN your laſt Number, page 225, is an intereſting article on the queſtion, At what point of temperature water is of greateſt denſity? From the introductory paragraph I was led to expect, that all the material objections to the current doctrine were conſidered and obviated, and that new and convincing arguments in its ſupport would be adduced. In the former of theſe expectations I was altogether diſappointed; and though the new experiments are ingenious and well worth attention, they are not quite ſo demonſtrative to me as they appear to be to Count Rumford. Perhaps we may both be too ſtrongly biaſſed towards preconceived theories: however this may be, it ſeems proper that when new facts are brought forward, we ought to reconcile them to the theory eſpouſed.

Mr. Dalton's exper. in this Journal on the ſame ſubject.

At page 93 of Vol. X. of this Journal, I have ſtated a number of facts and experiments which appear to me irreconcileable with the notion of water being denſeſt at 40°. I believe it is denſeſt at 32°, or the freezing point; and it is my preſent intention to ſhew how, on my hypotheſis, I explain Count R.'s reſults.

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[*] See Philoſ. Journal, Vol. II. p. 60.

Water

Count Rumford's experiments explained, on Mr. Dalton's hypotheſis.

Water has very little change of dimenſions by heat or cold near its max. denſity.

Water expands by heat from ſome point (whatever it may prove to be) by a law which is nearly that of the ſquare of the temperature from the ſaid point, as is evident from Sir Charles Blagden's table. Conſequently the force of aſcent, which water acquires by temperature, is at firſt very ſmall, but increaſes to a very conſiderable amount before ebullition. The coheſion of the particles of water is a conſtant force; there will therefore be a point of equilibrium between theſe two forces; that is, a point at which the increaſed temperature will be but juſt ſufficient to counteract the tenacity, in which caſe no internal motion can enſue. Whether a diminution in denſity in water to the amount of *one hundredth* or *one thouſandth*, or *one ten thouſandth* or more, is the point alluded to, is to be determined only by experiment. I apprehend that water of 40° is about one *ten thouſandth* part lighter than water of 32°; but that this force of aſcent is but juſt ſufficient to counteract the tenacity, and conſequently no motion takes place; in ſuch caſe the diffuſion of heat through water is the ſame as through a ſolid body. Whenever the difference in denſity exceeds that juſt mentioned, internal motion is the conſequence, and that greater in proportion to the difference of denſity, which we know may amount to $\frac{1}{24}$ of the whole.

This change at 40° is not ſufficient to produce a current;

—and therefore the heat at firſt paſſed downwards as if thro' a ſolid; but in higher temperatures there was a current, which prevented the deſcent.

Count R.'s experiments therefore will be explained by obſerving that the thermometer acquired heat by the proper conducting power of water, as if it had been metal, or any other ſolid body; the temperature acquired was greater in the 2d experiment than in the 1ſt, becauſe the heat of the ball was greater; but in the 3d experiment the heat of the ball was ſuch as to produce a current upwards that almoſt precluded the deſcent of heat, by carrying away the heated particles as ſoon as formed.

Difficulty reſpecting the two thermometers.

The circumſtances of the two thermometers by the ſide of the ball and the cup, in the two firſt experiments not acquiring any temperature, is certainly remarkable, and not eaſy to be explained, even upon Count R.'s principle; for, the ſuppoſed deſcending current of warm water ſhould have filled the cup and overflowed, ſo as to affect the collateral thermometer.

Important experiment propoſed. Let the water be at 40° and the ſolid at 32°.

One moſt important experiment Count R. has omitted, and which it is particularly deſirable that he, or ſome one in poſſeſſion of a ſimilar apparatus, would perform, eſpecially as it would go further than any other to eſtabliſh the doctrine of currents in water, when the temperature varies from 32 to 40°. This is to repeat the 1ſt experiment, with the difference that the maſs of water ſhould be at the temperature 40°, and the ball at 32°; in which caſe the thermometer in the cup would not be at all affected upon Count R.'s principle; but if the explanation I have attempted above be accurate, no material difference in the reſults of the two experiments would be obſerved.

I am your's,

J. DALTON.

Mancheſter, Auguſt 17, 1805.

VI.

On the Art of bending Wood. By J. H. Hassenfratz.*

Either live or dead wood may be bended.

THE operation of bending may be performed either on live or dead wood, the proceſſes differing however for theſe two ſtates.

I. *On the bending of Live Wood.*

Live wood may be

Live wood has a natural elaſticity †, which varies according to its ſpecies, ſize, and age. The larger and older the wood, the leſs elaſticity it poſſeſſes.

—bended for ſhip-building, or for felloes of wheels, &c.

This operation is performed on wood when growing, either to ſtraighten it, to give it a figure ſuitable to the ornamental purpoſe for which it is deſigned, or to ſhape it to the uſe for which the timber is intended when cut. Thus trees may be bended, which are intended for the building of ſhips, or for making the felloes of wheels in one piece.

By faſtening down young trees.

When trees are yet young and pliable, their ſtems are faſtened down by ropes, or poles, or ſtakes, or frames. In this ſituation they are confined till they will retain when let looſe the curvature that has been given them.

This the moſt eaſy proceſs;

Of all the modes of bending timber the moſt eaſy and commodious is that applied to young growing trees: for their pliantneſs and elaſticity enable them to acquire any form that may be deſired; ſo that there are few to which the moſt whimſical figures may not be given, with due care and the requiſite precautions; but at the ſame time we injure their natural conſtitution, retard their progreſs, and frequently reduce them to a ſtate of conſtraint and diſeaſe prejudicial to their growth.

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* Tranſlated from the Journal des Mines, N. 94, p. 475, July, 1804.

† In many parts of this paper the writer ſeems to have uſed the word elaſticité for the property of undergoing flexure without breaking. T.

but injurious to the timber.

II. *Of the bending of Dead Wood.*

The bending of dead wood is moſt advantageous.

The bending of wood that is cut down and dead, though more difficult, is yet more in uſe, becauſe we may chooſe ſuch as is beſt adapted to the purpoſe for which it is deſigned, and then give it the ſuitable curvature.

The principle is the action of heat, which increaſes the pliableneſs of timber.

The proceſs generally employed is founded on the property caloric poſſeſſes of augmenting the elaſticity of wood by penetrating it, and diminiſhing this elaſticity on quitting it.

Thus when we wiſh to bend thin pieces, as the ſtaves of barrels, or the planks of boats, we heat them at the part that is to be curved, and bend them gradually as they grow hot.

Partial heat affects the wood unequally, and occaſions it to crack or ſplinter.

But heat applied to one part of the wood, while the other is in contact with the air, heats it unequally, and increaſes the pliableneſs but partially; ſo that on bending it, ſome parts are ſtiff and others yield, occaſioning an unequal curvature, and ſometimes cracks or ſplinters in the inſide or on the ſurface of the wood. The only method of remedying this inequality is to heat the wood equally throughout.

Furnaces or ſtoves heat it uniformly, but may ſcorch it.

Furnaces or ſtoves gradually heated are adapted to the purpoſe of affording a uniform heat, and conſequently facilitating the curvature of the wood; but in uſing them there is reaſon to fear, that the caloric, while heating the wood, may expel from it the fluids contained in it, char it, and wholly deſtroy its elaſticity.

Humidity as well as heat neceſſary to render wood pliable.

The pliableneſs of wood is in proportion not to its temperature alone, but to its humidity likewiſe. The ſame wood at the ſame temperature will be more or leſs pliable in proportion to the water contained in it; and at an equal degree of moiſture its elaſticity will be proportional to its temperature.

The ſingular ſtructure of a mortiſe and tenon.

We have an inſtance of the double influence of heat and moiſture in joining two pieces of wood with a tenon and mortiſe, where the mortiſe is only a third of the breadth of the piece that is driven into it to form the joint. Theſe joints, ſo extraordinary in appearance, ſurpriſe people ſo much, that

Heat and moiſture are applied to bend timber;

moſt of thoſe who uſe them make a myſtery of them. The proceſs employed in this operation has given riſe to the method at preſent in uſe for bending with eaſe the largeſt and ſtiffeſt timber; it conſiſts in penetrating it with humidity, and at the ſame time imparting to it a uniform temperature, then bending it, and letting it cool, while it is kept in the form to which it has been brought.

—in three different ways:

For heating and moiſtening the timber, three different proceſſes have been employed: firſt, boiling water; ſecondly, ſteam; the third, wet ſand heated.

1ſt. By boiling water.

The ſtove for the firſt proceſs conſiſts of a large copper boiler, heated by three furnaces, cloſed by a movable cover, and varying in its dimenſions according to the ſize of the timber for which it is intended. Cranes are uſed for raiſing the timber, and putting it in or taking it out of the boiler, which is kept full of water. When the timber is in, the cover is put on and beaten down cloſe, to diminiſh the evaporation of the water; the three fires make the water boil, the timber is heated and penetrated with moiſture, and it is then taken out to be bent.

This diſſolves ſome of the component parts, and leſſens the dimenſions of the wood.

This proceſs, one of the firſt that was employed, has the defect of diſſolving a part of the proper ſubſtance of the wood in the boiling water; the timber ſhrinks in drying, ſo as to become narrower and ſhorter; its ſtrength and elaſticity are conſiderably diminiſhed, and from theſe alterations occaſioned by it the proceſs is diſuſed.

2d. By ſteam. Deſcription of the ſteamer.

Figures 2, 3, and 4, Pl. II. repreſent the plan and elevations of a ſteamer. It conſiſts of a large wooden box, formed of ſtout planks, held firmly together by ſquare frames. Within are ſupports for the timber that is to be expoſed to the action of the ſteam. The dimenſions of the box are regulated by the ſize and quantity of the wood intended to be ſoftened.

For ſmall ſteamers a boiler is fixed at one extremity of the wooden box, and the wood is introduced at the other through an opening, the door of which either ſlides in a groove or turns on hinges. For large ones the boiler is fixed in the centre, and there is an opening for the timber at each end. In the ſide oppoſite the boilers are openings *a a a* for arranging the timber on the ſupports. It is uſual to leave the wooden boxes expoſed to the air externally; but it would be of advantage to cover

It ſhould be covered with ſome bad conductor of heat.

cover the planks with ſome ſubſtance that is a bad conductor of heat, to confine the heat that is diſengaged from the ſteam within.

Each boiler having a communication with the interior of the box, by means of a pipe, the ſteam is diſtributed to each ſtage by the tubes *b b b*, *Fig.* 3. The vapour ariſing from the boiling water penetrates the timber with moiſture, heats it, increaſes its elaſticity, and renders it fit to be bent.

This proceſs is eaſy in uſe, and not expenſive; but it is not hot enough for large timber.

Steamers require little care, and little expenſe, but they cannot be uſed for timber of any great thickneſs, ſince they cannot impart a emperature higher than that of boiling water, and this is not ſufficient to give large pieces the degree of pliableneſs neceſſary for bending them.

3d. By wet ſand. Sand-ſtove deſcribed.

This lowneſs of temperature gave riſe to the invention of the ſand-ſtove, which is formed of four ſtone or brick walls. In the middle are two furnaces, with which ſeveral circular flues communicate, for conveying the heat, the heated air, and the ſmoke, to a chimney riſing from each end. On theſe flues are plates of caſt iron, which form the bottom of the cavity in which the ſand is placed; the flame and ſmoke circulating in the flues heat theſe plates, and theſe plates heat the ſand. This is an imitation of thoſe ſand-baths which have been long employed in many chemical proceſſes and in ſeveral manufactories.

As the ſand may be heated to a temperature above that of boiling water, it can communicate a greater heat to the timber; but were there nothing but ſand and timber in the ſtove, all the gaſeſiable ſubſtances in the timber might be expelled by the heat, and the timber charred.

Steam muſt be uſed with the ſand heat;

To prevent this, one or two boilers filled with water are placed in the middle of the ſtove. The water converted into ſteam by boiling penetrates the ſand with moiſture; this imparts moiſture to the timber; and thus the heat that pervades the timber expels from it no more moiſture than is replaced by the ſand, ſo that all the proper ſubſtances of the timber are preſerved.

and the timber will not be perceptibly injured.

We will not venture to affirm however, that no portion of the component parts of the wood is evaporated in this operation, and that conſequently it undergoes no alteration; but with the precaution of taking out the wood to bend it as ſoon as it is ſufficiently heated and penetrated with moiſture, the injury is imperceptible.

The ſand-ſtove is covered throughout its whole length, to retard the evaporation of the moiſture contained in it, and allow the heat to accumulate ſufficiently to give the wood the proper temperature.

Manipulation for the heat.

The pieces of timber are introduced at the ends, placed in the middle of the ſtove in the direction of its length on bars fixed for the purpoſe, and covered with ſand.

When the timber has been heated and penetrated with moiſture to the proper degree for enabling it to aſſume the degree of curvature required, it is bent to a line deſignating the curve.

The wood may be bended horizontally or vertically,

The timber may be bent in two ways, either horizontally or vertically; the former is uſed for pieces of ſmaller dimenſions and greater curvature.

—by any mechanic power;

In either way the force that produces the curve is applied by means of ropes, tackles, or even capſtans. The piece muſt be kept in the ſhape to which it is brought, and thus left to dry and grow cold, when it will retain the curvature given to it.

--which may be applied various ways.

Frequently when the piece of wood is thin, preſſure by hand, or by weights, will bend it ſufficiently, ſo that it will retain its ſhape on cooling. But the means of bending it may be varied to infinity, according to the elaſticity of the timber, its ſize, its temperature, and its humidity.

VII.

Experiments made in the great, in a reverberating Furnace on Caſt Iron, confirming the eſtabliſhed Theory reſpecting the Difference between caſt and malleable Iron. By G. A. Lampadius, *Prof. of Chemiſtry and Metallurgy at Freyberg.*[*]

The reverberatory furnace deſcribed.

I SHALL firſt deſcribe the reverberatory furnace uſed in theſe experiments. It had three principal parts: 1. the air tunnel

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[*] Extracted by J. F. Daubruſſon, in the J. des Mines, from the *Sammlung Practiſch-chemiſcher Abbandlungen* 'Practical Chemical Eſſays,' of Lampadius, Vol. II. p. 145.

Prize queſtion of Bohemian Society, 1795.

In 1795, the Royal Society of Bohemia propoſed as a prize queſtion to ſettle the theory of the refining of iron, taking as a baſis the labours of Vandermonde, Berthollet, and Monge, on the different ſtates

It was an air furnace having an hearth within and its chimney was 16 feet high.

tunnel and aſh-hole; 2. the fire-place; 3. the hearth and chimney. To obtain the proper degree of heat, the air was conducted through a vertical tunnel ſeveral ells long (the Saxon ell is near two Engliſh feet), the lower aperture of which was over a ſtream of water, and conſequently it brought rapidly to the fire-place a ſupply of freſh and condenſed air. The fuel was wood; the bottom of the furnace was an oval cavity, covered with a heavy coating, and capable of containing three or four hundred weight of metal. The flame, which traverſed the furnace with rapidity, eſcaped afterwards through a chimney eight ells high. The furnace had an opening capable of being cloſed at pleaſure by an iron door. There was another above the fire-place, a few inches ſquare, ſerving to admit the nozzle of a pair of bellows, or the neck of a retort.

Unoxided particles of carbon viſible in the flame of this cloſed furnace.

In the uſe we made of this furnace I had an opportunity of obſerving very diſtinctly, that in the flame of a cloſed reverberatory furnace there are always a multitude of unoxided particles of carbon, which impart to it the capability of reducing (diſoxiding) metal. This opinion I had already announced on occaſion of a memoir of Mr. Dacamdra. In ſome of our trials, making uſe of the wood of the Scotch fir, we obſerved, that the ſmoke iſſuing out was black and denſe, and this the more the freſher the wood; but as ſoon as we made uſe of the bellows, the flame appeared clear, becauſe the oxigen introduced by the air or vapour oxided the carbon that was in the flame, and thus produced a greater heat.

Firſt Experiment with the ſimple Fire of the Furnace.

Exp. I.

Gray fine grained caſt iron in the reverberatory furnace, covered with frothy ſcoria, chiefly carburet of iron.

The furnace having been heated for ſome hours, and the fire being very violent, about three hundred weight of metal was taken from the crucible of the high furnace, and poured into the reverberating furnace. This caſt iron, when become ſolid, was gray, and of a fine grain. At the expiration of an hour a frothy ſcoria appeared on the ſurface of the metal, which, to

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ſtates of iron. Mr. Lampadius ſhared the prize. His memoir may be conſidered in general as a confirmation and ſupplement to the labours of the French academicians; the experiments which he made at Muckenberg in Saxony, in the iron works of Count Von Einſiedel, affording him freſh proofs of this theory. Theſe experiments are here preſented to the reader. D.

Not removable for adhering metal.

The metal was brought to ebullition. Carburated hidrogen gas evolved.

In five hours it became white and coarſe grained, and a little malleable.

This was afterwards refined ſooner than common caſt iron.

judge from appearances, conſiſted chiefly in carburet of iron. We attempted to remove it; but as ſome of the metal adhered to it, and came away at the ſame time, we deſiſted. Soon after, the furnace being cloſed, we heard a continual boiling, reſembling that of a viſcous ſubſtance in a cloſe veſſel. On opening the furnace, we perceived that the whole matter in reality boiled, and that bubbles were continually riſing, which burſt on the ſurface with a beautiful bluiſh flame. Theſe jets of flame had the colour exhibited by carburated hidrogen gas. The boiling continued as long as the fire was kept up; at the ſame time a pretty large quantity of ſcoria was formed, which however could not be removed, on account of the viſcous conſiſtence now acquired by the metal. Beſides, as the metal was frequently ſtirred to preſent a freſh ſurface to the air, the ſcoria mingled with it. At the end of five hours it was no longer fluid, and appeared to be refined. It had loſt its gray colour and fineneſs of grain, was white and coarſe grained, and ſhowed itſelf more malleable, though it was not capable of being forged. The refiner carried it to his ordinary furnace, and there it was refined in leſs time, and required leſs labour than common caſt iron.

The proceſs of refining iron in a reverberatory furnace ſhews that carbon is burned off.

As in this trial we were unable to ſeparate the ſcoria, and no change had been made in the form of the hearth of the common refinery, which ought perhaps to have been done, nothing poſitive can be advanced with reſpect to the practical advantage of refining by the help of reverberatory furnaces; we were merely convinced of its poſſibility, and enabled to demonſtrate the theory of this proceſs, that is to ſay, of ſeeing clearly what paſſed in the operation. The caſt iron was here converted into malleable by means of the oxigen that was in the little atmoſpheric air, which, jointly with azote and carbonic acid gas, covered the metal in fuſion. This oxigen combined with the carburet of iron, and then carbonic acid gas and oxide of iron were formed; this produced the bubbles of air and the ſcoria. The lightneſs of the frothy ſcoria, which aroſe to the ſurface at the beginning, was the reaſon of their ſeparation from the reſt of the maſs; but as ſoon as the air began to act they were deſtroyed.

Second Experiment; the Fire of the Furnace being aſſiſted by the Vapour of Water.

Exp. II.

I had attempted to decompoſe carburet of iron in ſmall quantities by the help of water in the ſtate of vapour. By heating the carburet red hot, the water was decompoſed, and I obtained carbonic acid gas, hidrogen gas, and oxide of iron. As the chief difference between caſt and malleable iron conſiſts in a certain quantity of carburet of iron contained in the former, and which muſt be ſeparated to render the iron malleable, I was deſirous of trying the effect of water in vapour on caſt iron in a reverberatory furnace, principally in order to know how far the refining of iron might be carried on in this way.

The carburet of iron is decompoſed by aqueous vapour.

Application of this principle to refining iron.

About three hundred weight of caſt iron of the ſame quality as before, and juſt taken from the high furnace, were put into the reverberatory furnace as in the preceding experiment; we then took a large tubulated iron retort, put into it nine or ten quarts of water, fitted a gun barrel to its neck, and introduced the end of the gun-barrel into the little opening in the furnace. The water in the retort was made to boil, ſo that the ſteam diffuſed itſelf with the flame over the melted metal. At the expiration of half an hour all the marks of refining that had been obſerved before were perceptible; the ebullition was conſiderable, and the flame that iſſued from the chimney more bright. Two hours after the commencement of the proceſs freſh water was put into the retort. In about three hours the metal began to thicken, and at the end of four hours it exhibited the marks of refined iron, and we imagined the operation to be finiſhed. We found the grain of this iron finer, however, than that of the iron operated upon in the preceding experiment, and the maſs was full of little blebs.

Steam was introduced, by which the operation proceeded quickly,

but the iron was of fine grain, full of blebs,

We gave it to the refiner, who treated it like the former; but to our great aſtoniſhment we found that it wrought worſe in the refinery fire than caſt iron the moſt difficult to refine. It required much more labour, and an hour's time longer.

and was leſs capable of being refined than before.

Having aſſayed a ſpecimen in the ſtate in which it came out of the reverberatory furnace, I found it to contain a much larger quantity of oxigen. Experience had already taught me, that half a pound of gray caſt iron, treated in a retort with four ounces of charcoal from which all carbonic acid gas had

There was more oxigen in this than in other kinds of caſt iron.

been expelled, gave 32* cubic inches of carbonic acid gas. An equal quantity of white caſt iron afforded 165 cubic inches of the ſame air. Four ounces of the caſt iron juſt taken from the reverberatory furnace, mixed with two ounces of charcoal, yielded 96 inches, or 192 inches to half a pound.

Hence we may infer, that the proportions of oxigen contained in theſe different kinds of caſt iron are,

In iron ſuper-refined by the vapour of water	192
Common white caſt iron	165
Gray caſt iron	96

This ſuper-refined iron imbibed oxigen from the decompoſed water which deſtroyed its carburet.

To the iron produced in the experiment juſt mentioned, I give the epithet ſuper-refined †, becauſe I conceive it to have been formed as follows:—The water in vapour was decompoſed, and deſtroyed the carburet, as atmoſpheric air does in the ordinary refining; but at the ſame time this water imparted to the iron ſo large a quantity of oxigen, that in the refinery it was neceſſary, not only to ſeparate the ſcoria, but to diſoxide the metal likewiſe. This experiment farther confirms the property iron poſſeſſes of becoming oxided in different degrees.

If this experiment afforded nothing practically beneficial, it has at leaſt thrown ſome new light on the properties of caſt iron.

Exp. III.

Third Experiment; the Fire of the Furnace being aſſiſted by the Action of Bellows.

Bellows applied with the reverberatory furnace.

The ſame furnace was uſed, and the place of the retort in the preceding experiment was ſupplied by a pair of double bellows mounted with leather, 5 feet (4 f. 8 in.) long, 3 (2 f. 10 in.) broad, and 4 (3. f. 9 in.) high at the poſterior extremity when open. It was ſo placed, that the ſtream of air was parallel to the flame and to the middle of the furnace, and worked at the rate of eight or ten ſtrokes in a minute. We were deſirous of ſeeing how far the air thus aſſiſted would carry the refining; the furnace being managed and filled as before.

The heat was much greater.

At the end of half an hour the heat was perceived to be much greater than in the firſt and ſecond experiments. The phenomena of the refining already mentioned appeared in ſucceſſion;

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* Probably this is an error of the preſs in the original: as it does not agree with the proportion aſſigned in the next paragraph, one of the two muſt be wrong.

† Or ſurcharged with oxigen.

ceſſion; but inſtead of the frothy ſcoria obtained in the firſt eſſay, a very fluid ſtratum was formed, which diffuſed itſelf over the melted metal, and prevented its refining. This ſcoria, when grown ſolid, was of a blackiſh brown colour and vitreous fracture. We endeavoured more than once to remove it, but the ſtratum was ſo thin as to render it impracticable: As ſoon as one ſtratum was removed, another formed. At the expiration of four hours, the metal being ſtill very fluid, we began to ſtir it, in order to bring its different parts ſucceſſively into contact with the air; this produced an extraordinary heat in the furnace, combuſtion, and ſcintillation, reſembling that which takes place when iron wire is burnt in oxigen gas. This oxidation always produced freſh ſcoria: as ſoon as we deſiſted from ſtirring, every thing became quiet, and the ſtratum of ſcoria prevented the oxidation. At length, after three hours longer, making ſeven in all, during which the melted maſs had frequently been ſtirred, it ſeemed to thicken; perceiving too, that it diminiſhed conſiderably in quantity, the fire was damped; and the matter left to cool in the furnace. It was afterward weighed, and found to have loſt much of its weight. Its extraordinary fracture gave reaſon to preſume a high degree of oxidation; for inſtead of being gray and granulous, it was compact, and of a ſilver white. It was interſperſed with a large quantity of ſpherical cavities, greater or leſs in ſize, which evidently announced the exiſtence of an aeriform fluid, that had been extricated during the fuſion.

The ſcoria melted.

It was blackiſh brown, of a vitreous fracture, and not to be removed.

Stirring produced extreme heat, and ſcintillating combuſtion.

The iron had loſt much in weight.

Its fracture compact and ſilvery.

Porous.

This maſs was too ſmall to be refined. Having examined the quantity of oxigen it contained in the ſame manner as with the other ſpecimens, I found that four ounces yielded 87 cubic inches of oxigen gas, and conſequently nine inches leſs than that which had been treated by means of aqueous vapour. Thus probably here too the oxidation was too powerful, and the iron was ſuper-refined. As the metal did not become doughy in the courſe of the proceſs, it muſt have been ſuperſaturated with oxigen without paſſing through the ſtate of malleable iron. The carburet, it is true, muſt have been totally deſtroyed during the operation, which produced the ſilvery hue.

There was much loſs, and the remainder acquired much oxigen,

without paſſing through the malleable ſtate.

The carburet deſtroyed.

VIII.

Remarks on the burſting of two Muſquet Barrels by a Charge of Gun-powder confined by Sand. W. N.

A thin muſquet barrel burſt.

PART of the barrel of a muſquet of which the internal diameter was ſix and a half tenths of an inch, was corked at one end, and fine ſand to the depth of twelve inches was poured in: upon this was poured two inches of gunpowder and a ſmall tube (of glaſs) was then ſtuck in the gunpowder, and the bore of the tube, which was about one twentieth of an inch diameter, was filled alſo with gunpowder. The length of the tube was ſufficient to reach clearly above the top of the gun-barrel, and all the reſt of the ſpace in the barrel, being about thirteen inches above the charge, was filled with ſand lightly poured in. In this ſtate the barrel was ſet up in one corner of a furnace chimney, and a match ſtuck into the glaſs tube and lighted, afforded ſufficient time for the aſſiſtants to remove out of the direct line of exploſion before the effect could take place.

The diſcharge tore the barrel into ſeveral contorted pieces in the part near the charge; the upper part fell unaltered, and its contents of ſand ran out: the lower part alſo fell down, but neither its ſand nor cork were diſturbed, nor was that portion of the barrel affected.

As the thickneſs of the iron did not exceed one thirtieth of an inch, I was deſirous of repeating the experiment with a ſtronger piece.

A thicker barrel charged with gunpowder and ſand.

A muſquet barrel, 2½ feet long, diameter of bore five tenths of an inch, and thickneſs of metal at the breach full one quarter of an inch, was charged with 278 grains, or a little more than half an ounce troy of gunpowder, which occupied the ſpace of four inches. Upon this charge was poured fine ſand to the depth of twelve inches, weighing 1151 grains, or about 2⅓ ounces troy, and upon this was lightly preſſed down a ſoft wadding of gauze paper, for the purpoſe of allowing the barrel to be placed horizontally without any ſubſequent diſturbance of its charge. It was ſafely placed in an horizontal poſition and fired at the touch hole by means of a train.

Effect of the exploſion.

The barrel was torn aſunder for the length of eight inches, the part neareſt the breach-pin being opened nearly to flatneſs.

The ſand remained in the barrel. Its face neareſt the blaſt was conſolidated to a very ſmall depth, and I think the maſs had been removed or elſe jammed more cloſely together; for the ſpace unoccupied between the place where the breach pin had been and the ſurface of the ſand was full nine inches. But as the ſand was not immediately noticed, I cannot be ſure that none might have been driven or fallen out, during or after the blaſt; though I am diſpoſed to think not.

Remarks,

It muſt be remarked, that the powder was a very full charge, and that the ſand weighed as much as ſix muſket-balls of half an inch diameter. I do not however apprehend that the barrel would have burſt with ſix balls.

Uſes,

The blaſting of rocks, the ſplitting of logs of wood, and the deſtruction of artillery when on the point of being abandoned to the enemy, are the leading purpoſes in which the application of ſand to confine gunpowder is likely to become uſeful.

IX.

Report of a Method of meaſuring the initial Velocity of Projectiles diſcharged from Fire-arms, both horizontally and with different Elevations, made to the Phyſical and Mathematical Claſs of the National Inſtitute by Mr. PRONY, *Dec.* 11, 1803. *Abridged from the Original*[*].

Robins firſt examined the velocity of projectiles by the pendulum, and the recoil of a gun by ſuſpending it.

IT is not much above ſixty years ſince experiment began to be applied with ſucceſs to the theory of projectiles. Mr. Benj. Robins, who may be eſteemed the firſt in this career, employed a pendulum to determine the initial velocity of muſket balls, meaſuring it by the arc of oſcillation. He likewiſe meaſured the recoil, by ſuſpending the gun-barrel from the pendulum.

D'Arcy made ſimilar experiments with two pendulums.

About ten years after, the Chev. d'Arcy publiſhed a ſeries of experiments in the memoirs of the French Academy of Sciences, in which he employed two pendulums, againſt one of which the ball was projected, while the other, to which the gun-barrel was ſuſpended, ſerved to meaſure the recoil.

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[*] *Journal des Mines*, No. 92, p. 117, May, 1804.

Dr. Hutton with cannon balls.

Fifteen years after this Dr. Hutton made many experiments at Woolwich with cannon balls by means of the pendulum.

Count Rumford's improvement.

About the year 1778, Count Rumford improved this method of trial, and invented a very ſimple method of ſuſpending the gun-barrel ſo that the recoil took place without the axis ceaſing to be horizontal.

Dr. Hutton's exp. are the moſt complete on the ſubject.

Laſtly, Dr. Hutton reſumed the ſubject, and made a number of experiments from 1783 to 1786, with much care, and at great expenſe, on both kinds of pendulum. Theſe may be conſidered as forming the moſt complete and inſtructive treatiſe we have on experimental balliſtics.

Antoni's deſcription of Mathey's machine.

We have not mentioned the labours of Antoni, but we muſt not paſs over a machine deſcribed by him in his eſſay on gunpowder. This, which he ſays was invented by a mechanic named Mathey, conſiſts of a horizontal circle, the centre of which is ſupported by the ſuperior extremity of a vertical axis, and ſerves as a baſe to a cylindrical envelope of paper. A rotatory motion is given to this cylinder by means of a cord paſſing over a leading pulley; and the projectile being diſcharged horizontally, when the angular velocity of the machine is uniform, in a vertical plane in which the axis is found, pierces the cylinder in two points, the diſtance of the ſecond of which from the diameter paſſing through the firſt meaſures the arc deſcribed by the machine during the paſſage of the projectile.

Col. Grobert's newly invented machine deſcribed.

The machine recently invented by Col. Grobert is conſtructed as follows:

It conſiſts of two paſteboard diſks revolving ſwiftly at the extremities of an horizontal axis.

A horizontal rotatory axis about 34 dec. (11 feet) long carries at each extremity a paſteboard diſk perpendicular to it, and faſtened to it ſo that the whole may turn rapidly without deranging the reſpective poſitions of the parts.

A rotatory motion is given to the two diſks by means of a weight ſuſpended to the end of a cord, which, after having paſſed over a pulley ten or twelve yards from the ground, is rolled upon a wheel and axle level with the diſks. An endleſs chain, paſſing round the wheel and the rotatory axis of the diſks, communicates to this axis the motion which the weight in its deſcent imparts to the wheel.

The advantages this machine poſſeſſes over Mathey's conſiſt in the horizontal poſition of its axis, which admits the utmoſt degree of firmneſs and regularity in the poſition and motion of the diſks: in the projectile not traverſing a cylindrical ſurface, but two vertical planes, the extent and diſtance of which may be conſiderable, and thus give very accurate meaſures: and its being capable, which no other apparatus is, of meaſuring the velocities of balls of different ſizes projected at different elevations.

The projectile is fired through both diſks, and the rotation prevents the ſecond hole from being oppoſite that on the firſt paſteboard.

All that is neceſſary in uſing this apparatus is to give a uniform and known angular velocity to the diſks; and to meaſure the arc compriſed between two planes paſſing through the axis of the diſk, and one of them through the hole in one diſk, the other through the hole in its oppoſite.

Method of uſing the inſtrument.

In the trials made, the motion became ſenſibly uniform, when the weight arrived nearly in the middle of the vertical ſpace it had to traverſe, as was found by twice meaſuring the times of the third and fourth quarters of the deſcent, and afterwards comparing theſe times with the correſponding ſpaces paſſed through. An excellent ſtop-watch by Lewis Berthoud, and another by Breguet, were uſed for this purpoſe.

The deſcent of the moving weight becomes uniform.

In moſt of the experiments the vertical ſpace paſſed through by the weight was meaſured by the turns and parts of turns of the cord wound off in a given number of ſeconds, as in all reſpects moſt accurate and commodious.

The ſpace of deſcent was meaſured by turns of the cord.

To meaſure the arc a ſcreen, or paſteboard, was fixed before each diſk, a very little diſtance from it, and the hole in the firſt diſk being brought oppoſite to the hole in its correſponding ſcreen, a rod carried through the centre of theſe two holes and of the hole in the other ſcreen which would be oppoſite them, muſt pierce the ſecond diſk in the plane of the hole in the firſt; and the arc compriſed between this point and the centre of the hole in the farther diſk would meaſure the angle deſcribed by the apparatus while the ball was traverſing the length of the axis.

Method of meaſuring the arc between hole and hole.

It is obvious, that the fixed ſcreens, which give the abſolute direction of the path of the ball, afford the means of ſhewing the defect of paralleliſm, if there be any, between this path and the axis on which the diſks revolve.

Two fixed ſcreens are uſed to ſhew any defect of paralleliſm between the path of the ball and the axis.

The gun-barrel was fixed horizontally, parallel to the axis of the diſks, and at ſuch a diſtance, that the concuſſion of the air by the exploſion could not affect the motion of the neareſt diſk.

The time ſuppoſed too ſhort to allow a meaſurable arc.

One thing may naturally ſuggeſt itſelf, which is, that the time of the ball's paſſing from one diſk to the other, through a ſpace of three or four yards, muſt be leſs than $\frac{1}{100}$ of a ſecond; and it is difficult to conceive, that in ſo ſhort a ſpace the diſk could deſcribe an arc capable of being meaſured.

But it did not prove ſo.

But this difficulty is eaſily ſolved by the fact. When the motion became uniform, the wheel and axle commonly made 0·333 of a turn in a ſecond; and every turn of the wheel produced 7·875 turns of the axis of the diſks, which conſequently made 6·56 turns in a ſecond. Thus a point on the diſk three feet from the axis would move about 41 yards in a ſecond, and in $\frac{1}{100}$ of a ſecond $\frac{41}{100}$ of a yard, or nearly 15 inches, a length more than ſufficient for the moſt accurate meaſurement.

The fire-arms experimented with.

The experiments were made with a ſoldier's firelock and a horſeman's carbine, the lengths of which in the bore were 3 f. 8 in. and 2 f. 5 in. The balls were accurately weighed, found to be on a medium 382 grains troy, and each was impelled by half its weight of powder.

Formula for calculating the velocity.

The following formula was employed for calculating the velocity of the balls. Putting π for the ſemiperiphery, when radius is unity $\doteq 3.141$; k for the ratio between the turns made by the wheel and axle and the arbor of the diſks; t the time employed by the wheel and axle to make a number of turns n; r the diſtance of the hole in the ſecond diſk from the centre; a the arc deſcribed by this hole while the ball paſſes from one diſk to the other; b the diſtance between the diſks; and V the velocity of the ball: we ſhall have the equation

$$V = \frac{2\pi n}{k t} \cdot \frac{r}{a} b.$$

Mean velocity with a carbine, 1269 f. per ſec. with a muſket, 1397.

The mean velocity deduced from ten experiments with the carbine was 1269 feet and a half in a ſecond; that from the experiments with the muſket, 1397 feet. Theſe being in the ratio of 11 to 10 nearly, it would appear, that the length of the ſoldiers's firelock might be reduced without much diminiſhing its range*: but there are other circumſtances in a military view, by which the length of the weapon uſed by the infantry requires to be regulated.

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* The differences of the range are much leſs than thoſe of the velocity. *See Dr. Hutton.*—T.

With ¼ of powder they were 822½ and 829.

The commiſſioners made ſome experiments with half charges or with powder only to the quantity of one fourth of the weight of the ball. In theſe the mean velocities were, for the firelock 829 feet, for the carbine 822½. Theſe velocities do not differ ſo much from each other, and conſiderably exceed the half of thoſe given by the full charge, which may be aſcribed chiefly perhaps to the more complete firing of the powder.

The reſiſtance of the air in 20 yards diminiſhed the velocity nearly one-fifth.

The commiſſioners were deſirous likewiſe of making ſome experiments on the reſiſtance of the air to the motion of the ball, the diameter of which was from 15 to 16 millemetres (5·8755 lines to 6·2672.) For this purpoſe the mouth of the gun-barrel, which at firſt was 7 f. 9 in. from the neareſt fixed ſcreen, was removed to the diſtance of 67 f. 9 in. In this ſituation the mean velocity of the muſket-ball was 1127 f. inſtead of 1397, ſo that it was diminiſhed nearly in the ratio of 42 to 34. The experiments of this kind however were few in number.

The apparatus might be enlarged with advantage.

There is no doubt but the dimenſions of Col. Grobert's apparatus may be enlarged, ſo as to adapt it to experiments with cannon balls; though it is not eaſy to ſay without trial what dimenſions would be compatible with accuracy of experiment.

Mode of adapting it to different elevations.

The Colonel likewiſe propoſes an alteration in it, for meaſuring the velocity of projectiles at different elevations, as far as 45°. The following is his contrivance for this purpoſe. Each of the diſks has a ſeparate axis. The wheel and axle has a wheel at each end, with an endleſs chain, one turning the arbor of one diſk, the other that of the other. Thus the rotatory motion imparted by the deſcending weight is communicated equally to both diſks at the ſame time, the wheels and arbors being made exactly of correſponding dimenſions. The ſtand of the diſk fartheſt from the gun is moveable in a vertical direction, ſo that it may be raiſed to the neceſſary elevation; a few links being added to the endleſs chain for every different height. As the diſk is raiſed indeed, it becomes inclined to the path of the ball; but as the greateſt diminution that can take place in this way is in the ratio of about 7 to 5, a ſufficient field is ſtill left for pointing with preciſion.

Additional machinery for counting time, &c.

To prevent any miſtake from want of attention in the perſons employed, Col. Grobert has added certain pieces of mechaniſm to his apparatus, by means of which the weight, when it has deſcended to a certain point, puts in motion a ſecond pendulum to count the time, and a ſyſtem of wheels and pinions connected with the wheel and axle to indicate the number of turns made by it. By ſimilar contrivances it diſcharges the gun, and ſtops the pendulum and the counter of the turns at the proper time. Theſe may occaſionally be of uſe, but complicated machinery is always liable to get out of order, and it may be diſpenſed with here, if the obſervers be ever ſo little expert and attentive.

The motion of the diſks does not affect the path of the ball.

It might be ſuſpected, that the motion of the firſt diſk would cauſe ſome deflection of the ball from its true path before it reached the ſecond. To aſcertain this, three ſcreens were fixed at equal diſtances, the ſecond and third being placed before the firſt and ſecond diſks reſpectively. Now it is obvious, that the hole in the third ſcreen would not be in the ſame vertical plane with thoſe made in the firſt and ſecond, if any deviation took place.

Experimental proof.

A ball being fired through the apparatus thus arranged, a plumb line was ſuſpended before the centre of the hole in the firſt ſcreen, and the moſt accurate obſervation could not diſcover any deviation, but that the ſame line cut the centres of all the three holes. This experiment was ſeveral times repeated with the ſame event.

This owing to the velocity.

The fact no doubt is, that the extreme ſhortneſs of the time, (for the ſemidiameter of the ball is not the forty thouſandth part of a ſecond paſſing the diſk) does not allow the diſk ſenſibly to affect the path of the ball; much leſs can the ball have any effect on the motion of the diſk.

It may not be amiſs to obſerve, that the diſtance of the fartheſt ſcreen being about twelve yards only, the inflexions obſerved by Robins in diſtances of a hundred yards were not likely to take place.

Fact

X.

Fact concerning the inviſible Emiſſion of Steam into the Air.

W. N.

SOON after Mr. Giddy had mentioned to me the very remarkable and curious facts of which an account is given at page 1 of the preſent Number, I was engaged in the experiments on the ſimmering of water related at p. 216 of Vol. X. I then made an experiment which may perhaps in a ſmall degree elucidate thoſe phenomena. A ſmall glaſs tube was ſtuck through a cork, and this was then preſſed into the neck of the retort in which water was boiling over the lamp. The ſteam was emitted through this ſmaller aperture in a viſible jet of upwards of a foot in length. But when a candle was held with its flame immediately beneath the end of the tube, the jet became perfectly inviſible. To determine whether the water might be decompoſed, or the ſteam ſimply expanded ſo far as to be abſorbed by the air, or if condenſed to form a vapour too thin to be perceived, I ſuffered the hot inviſible current which had paſſed through the candle, to paſs through a larger glaſs tube. In this caſe viſible ſteam iſſued plentifully from the farther end: Hence, I am diſpoſed to judge that the large tube having kept the very hot ſteam together and cooled it ſo as to render it viſible again, there was little if any decompoſition of the water. But at the ſame time, when we conſider the diſappearance of the denſe ſmoke in Mr. Giddy's experiment, there ſeems to be great reaſon to think that the charcoal was oxigenated and gazified. If ſo, the products muſt have been expanded and inviſible ſteam, hidrogen and carbonic acid. By collecting the products in an experiment of this kind, theſe conjectures will either be verified or refuted. If the former, we ſhall have the decompoſition of water and oxigenation of carbon at a lower temperature than has hitherto been ſhewn or expected.

Steam was viſibly emitted in a current:

It became inviſible when it paſſed above the flame of a candle;

but the ſteam was not decompoſed:

Perhaps ſome part may have been changed.

XI.

Experiments made with the Water blowing Machines of the Iron Works of Poullaouen; by Citizens BEAUNIER *and* GALLOIS, *Mine-Engineers* *.

The experiments made to ſhew the effects of a blowing machine.

OUR object was to aſcertain the differences in denſity of the air within a blowing machine, under the various circumſtances by which it might be affected; and at the ſame time, we endeavoured to find what may be the moſt advantageous mode of conſtructing the machine, to produce the greateſt effect with the leaſt expenditure of water.

Former accounts are obſcure.

One of the chief cauſes of the doubts that have ariſen reſpecting the ſuppreſſion or retaining of certain arrangements in the conſtruction, was the omitting to deſcribe the machines, the experiments with which have been compared. We ſhall therefore previouſly notice the principal diſtinctions that may be made between theſe machines, from the manner in which their effect is produced.

Two kinds of water blowing engines, as the air is received at top, or from the ſide.

Dr. Lewis obſerves, that there are two general methods of cauſing the air to be conveyed by the water in the blowing machines. In the firſt, the water receives the air by the ſummit of the machine; in the ſecond it receives it by lateral apertures: and he lays it down as a principle, that thoſe circumſtances, which promote the effect in the one caſe, are detrimental to it in the other.

General obſervations of Dr. Lewis. The engine is an upright pipe through which a ſhower of water and air deſcended.

He obſerved further, that if the water be at reſt in the funnel of the machine, (ſee Plate III. Fig. 2.) and afterward have liberty to run off, it carries little or no air with it; that if the water have a gyratory motion in the funnel, it carries down a conſiderable quantity; and that if it fall from a certain height, ſo as to have been greatly divided, it carries ſtill more: that if the water flow through a pipe with lateral apertures, it receives air through theſe apertures, even when its motion is ſlow: that if the pipe be of equal diameter throughout, the quantity of air thus received is inconſiderable; but if the diameter be diminiſhed to a certain degree at the part where the apertures are made, the quantity of air is greater than could have been introduced through the funnel without

Lateral apertures preferable for admitting the air.

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* Tranſlated from the Journal des Mines, No. 91.

without any lateral openings to the air: laſtly he obſerves, that air conveyed downward from the top of the tube, or the funnel, prevents the introduction of the freſh air by the lateral apertures, which in this caſe, inſtead of receiving more air, let that which has been already introduced eſcape.

Lewis concludes, that the two methods by which air may be made to deſcend with a ſtream of water, ought not to be united in one machine; and that the machine conſtructed with a pipe, a funnel, and apertures to let the air enter around or below the throat, produces the moſt powerful effect.

The machine of Poullaouen deſcribed.

The machine on which we made our experiments was of the conſtruction which Dr. Lewis has deemed moſt advantageous. See Fig 2.

Height of the fall.

The height of the fall, taken from the bottom of the channel that conveys the water to the upper part of the barrel B, is 21 feet 6 inches.

Funnel at top of the pipe.

The height of the funnel, from the bottom of the ſame channel to the throat *x*, is ſeven feet. This funnel is of the ſhape of a fruſtum of an inverted cone, the larger diameter of which is 12 inches, the ſmaller four. The remainder of the tube down to the barrel, is a cylinder eight inches in diameter.

Barrel or air veſſel.

The plank N, 12 or 13 inches wide, is fixed one foot below the head of the barrel. The barrel is ſix feet high.

The water flows off beneath;

The water iſſues out of the barrel by the triangular apertures *t t t*, and is conveyed away to a drain by the channel M, the bottom of which is four feet higher than that of the barrel.

and the air is conveyed through a pipe at top.

The air compreſſed by the external water, the level of which, as will ſoon be proved, is from 27 to 30 inches above that of the water in the barrel, eſcapes through the tube P, which is a hollow cylinder five inches in diameter.

Air-holes in the upright pipe.

This tube P, called alſo the air-pipe, terminates in a conical nozzle, having an aperture of two inches only.

Immediately below the throat *x*, are four air-holes *y y*.

This being premiſed, we proceed to the inſtrument employed by us for determining the denſity of the air in the machines.

Gage for meaſuring the denſity of the air.

It was invented by Citizen Vergnies Bouiſchére, proprietor of the iron works at Vic-Deſſos, in the ci-devant county of Foix, and is a particular kind of barometer, to which the name of water anemometer has been given. See Fig. 6.

It is a ſhort barometer gage inſerted, the fluid being water.

It is compoſed, 1ſt. of a cylinder A; 2d. of a tube *c*, bent twice, the lower extremity of which is ſlightly conical, and terminates about two inches below the bottom of the cylinder; 3d. of a graduated tube *d* inſerted in a vertical poſition into the cylinder, and reaching below the level *n* of the water contained in it.

The tube *c* being inſerted into an auger hole made in the ſide of the blowing machine, and ſtopping that hole cloſely, the internal compreſſed air communicates with the water contained in the anemometer, preſſes upon it, and in proportion to its denſity raiſes to a leſs or greater height in the graduated tube.

The cylinder A and the tube *c* are of tin. The lower part of the tube *d* to the height of nine inches is of the ſame material, to which is fitted a glaſs tube about 36 inches long.

The cylinder A is four inches high and as many in diameter. The greateſt diameter of the curved tube is half an inch, the ſmalleſt, at the extremity, a third of an inch. On obſerving however, that the ſize of this opening contributed to increaſe the extent of the oſcillations in the graduated tube, we endeavoured to diminiſh it as much as poſſible. For this purpoſe we cloſed the lower part of the tube c with ſealing wax, in which we afterward made a very ſmall aperture by paſſing a heated needle through it.

The tube *d* was divided by a ſcale of inches, beginning from the ſurface of the water contained in the cylinder *.

ACCOUNT OF THE EXPERIMENTS.

Experiments with the blowing machine.

I. *Experiments relative to the Expenditure of Water, and the Quantity of Air diſengaged.*

The blowing machine No. 1, ſee the horizontal projection, Fig. 1. to which for the ſake of clearneſs we ſhall refer our different experiments, ſerved for the trial. It was placed in a T, oppoſite the machine No. 2, deſtined for the ſame purpoſe.

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* The great difference between the diameter of the tube *d*, and that of the cylinder A allows the level *n* to be conſidered as conſtant.

poſe. The afflux of water into each was regulated by means of the hatches *a* and *b*, and the diſtant floodgate Q, placed in the principal channel D. See the plan Fig. 1. Plate .

The anemometer was placed in *o*, Fig. 2, in the direction of the vertical tube P protracted. The hatch placed in *b* was let down, ſo as to prevent the paſſage of the water that way. The hatch placed in *a* was raiſed, and the flow of water regulated by means of the floodgate Q. This flow we varied, till we found the water in the graduated tube raiſed as high as could be effected without any other change of circumſtance. When we were certain we had attained this point, and that no variation in the quantity of water flowing off took place, we made the following obſervations.

Obſervations.

1. The mean depth of the water in C, in the little channel, juſt before the T, was 15 inches 6 lines.

2. The mean depth of the water in the great channel, was 18 inches 9 lines.

3. The water roſe to 26 inches in the graduated tube. The oſcillations varied between 25 and 27 inches, but ſeldom reached the latter height.

4. The velocity of the water in the great channel having been obſerved, the following data were obtained.

Examined by means of ſimple floaters of paper, on an extent of 24 feet, we had,

1ſt. The ſpace paſſed through in two minutes = 61 feet, 8 inches, 6 lines.

2d. The ſpace paſſed through in four minutes = 120 feet, 6 inches.

Method of meaſuring the velocity of the water.

The ſame velocity examined with cork floats, ſupporting little balls of wax, the weight of which was augmented by bits of lead, ſo that they ſwam in the middle of the ſtream with a gravity little exceeding that of water, we had for a mean of the ſpace paſſed in two minutes, 63 feet, 7 inches, 4 lines.

If we compare theſe different reſults, we ſhall find, that the mean velocity of the water may be eſtimated at 30 feet, 11 inches, 1 line, a minute: but as it appears to us, that the reſults afforded by the cork floats muſt approach neareſt the truth, we will pay no regard to the quantities before obtained, and eſtimate the mean velocity of the water in the greater channel, at 31 feet, 9 inches, 8 lines, a minute.

Conſumption of water.

Now the breadth of the channel employed is 3 feet, 6 inches, and we obſerved, that the current, the velocity of which we have given, is 18 inches 9 lines deep. Hence we may conclude, that the conſumption of water by the machine, under the circumſtances above mentioned, is 173 cubic feet in a minute, the height of the column of water in the inſtrument being 26 inches.

Quantity of air emitted.

From the method deſcribed in the Hydrodynamics of Boſſut, we calculated the quantity of air which this maſs of water cauſes to iſſue from the machine in a given time. This quantity of air was found to be 7.35 cubic feet in a ſecond, or 441 in minute *.

II. *Experiments on the Effect of the Air-Holes.*

Effect of the air-holes aſcertained by experiment.

1. On ſtopping the four air-holes, the water in the tube of the inſtrument deſcended to nine inches, and oſcillated very little. The efflux of the water from the machine acquired a velocity ſufficient to diminiſh the depth of the water in the little channel C, Fig. 1 and 2, near the T, ſix inches.

2. One of the air-holes being opened, the water in the tube oſcillated between 22 and 24 inches. The mean = 23 inches.

3. A ſecond air-hole being opened, the mean height of the water in the tube was 25 inches.

4. A third air-hole being opened, the columns of water in the tube roſe to its former height of 26 inches.

5. The fourth hole being opened, no perceptible alteration in the inſtrument took place, which proves, that this hole has no effect on the machine.

III. *Experiments on the Uſe of Croſſes placed at the ſuperior Orifice of the Machine.*

ther croſs bars in the top of the tube be advantageous.

Some iron-maſters are accuſtomed to place two round bars in the form of a croſs at the upper orifice of the funnel of the machine. Theſe they imagine increaſe the effect of the machine by dividing the water at the moment of its fall.

Cylindrical bellows of Namur give more air with leſs water.

* If theſe reſults be compared with thoſe of the cylindrical bellows of the country of Namur, deſcribed by Cit. Bailbet, in No. 16, of the Journal des Mines, it will appear, that, to give out an equal quantity of air, the quantity of water expended by the blowing machines, with a fall more than twice its height, is nearly double that employed to move the cylindrical bellows.

To judge of this in the caſe before us, we fitted in one of theſe croſſes, all the other circumſtances remaining as above, and then obſerved the progreſs of the inſtrument.

The column of water in the tube frequently deſcended to 24 inches, and ſeldom roſe to 26: whence we may eſtimate the mean height, which before was 26 inches, only 24¾.

They diminiſh the effect.

Now this difference occaſions a diminution of velocity in the efflux of the air, and conſequently ſhews the faultineſs of this method under the circumſtances here mentioned.

IV. *Experiments on the Effect of Hatches placed near the Orifice of the Machine.*

The hatch *a* Fig. 1. was replaced in the grooves adapted to the channel. We altered its height from the bottom of the channel, obſerving the movements of the anemometer, in order to find the poſition moſt favourable for the effect of the machine.

Advantage of regulating the influx of water.

The mean height of the column of water in the tube never exceeded 28 inches, the elevation of the lower part of the hatch above the bottom of the channel, being then five inches one line; and it is remarkable, that the difference of a ſingle line in this elevation lowered the water in the tube conſiderably.

V. *Experiments on the Croſſes when the Hatch is uſed.*

The hatch being placed as has juſt been ſaid, we fitted the croſs again at the ſuperior aperture of the funnel, when the water in the tube of the anemometer ſunk. We then varied the height of the hatch above the bottom of the channel; obſerving the progreſs of the inſtrument, to determine the moſt advantageous poſition for it under the preſent circumſtances.

When thus regulated.

The elevation of five inches eight lines was now found the moſt favourable to the effect. With this the water oſcillated in the tube between 28 and 30 inches, moſt frequently reaching the latter height, which we could never bring it to exceed, whatever changes we made in the arrangement of the parts that compoſe the machine.

The croſs produced more air,

If we compare the ſituation of the hatch before the addition of the croſs, with that which is moſt ſuitable in the caſe before us, we find an increaſe of ſeven lines in the height

but expended more water.

from the bottom of the channel: now this addition to the height conſiderably increaſes the quantity of water expended by the machine.

Concluſions from theſe Experiments.

General concluſions. The engine by 173 cubic feet falling through 21 feet, drove out 441 cubic feet of air in a minute, under a preſſure of 26 inches of water on nearly two inches of mercury; which is, not quite one pound per ſquare inch.

(A.) Under the circumſtances related in the firſt ſet of experiments.

1. The expenditure of water for the blowing machine with which they were made was 173 cubic feet in a minute.

2. The air emitted from the aperture of the nozzle, being two inches in diameter, when the anemometer was at 26 inches, was 441 cubic feet in a minute.

(B.) Of the four air-holes in the machine, three only contribute to the effect produced.

(C.) The hatch placed near the orifice of the machine increaſed its effect, when the lower part of it was raiſed five inches one line above the bottom of the channel to which it was fitted.

(D.) A croſs placed at the upper orifice of the machine diminiſhes its effect when the hatch is taken away: on the contrary they increaſe it, if the hatch be ſo placed, as to be five inches eight lines above the bottom of the channel, an elevation greater than that mentioned in the preceding paragraph (C.) and which increaſes the expenditure of water.

From theſe reſults it may be inferred, that the croſs ſhould not be uſed in ſeveral caſes, where the quantity of water with which the machine is ſupplied, is confined within certain limits.

ANNOTATION. W. N.

The water blowing engine farther explained.

The blowing engine deſcribed in the preceding memoir acts upon the principle of the lateral adheſion of fluids, upon which Venturi has ſo ably written, in a treatiſe given entire in our Quarto Series of this Journal, and ſeparately publiſhed afterwards by Taylor in Holborn. The ſhower of water in its deſcent through the vertical pipe carries down a maſs of air along with it, in the ſame manner as a ſhower of rain on the flat ſurface of the ſea produces that temporary blaſt of wind, which ſeamen term a ſquall, and is ſufficiently violent to carry away the maſts of a ſhip, if the ſails be not reduced in time.

It is evident that this engine poſſeſſes the deſireable qualities of cheapneſs and ſimplicity; and Lewis who has written ſomewhat fully upon it, in his Philoſophical Commerce of Arts, from experiments of his own, aſſerts, that it requires much leſs water for working it than any other kind of bellows in uſe. I have no doubt but that many occaſions muſt offer in which it would be beneficial; but whether its expence of water be comparatively ſmall, and its power in any caſe equal to the ſupply of our ſmelting furnaces, may be deſerving of more enquiry.

Numerical ſtatement by Mr. Roebuck, of the effect of a ſteam engine in affording a blaſt of air,

In the excellent paper of Mr. Roebuck on the Devon iron works, inſerted in the fifth Volume of the Edinburgh Tranſactions, and alſo in the Quarto Series of this Journal, there are ſome numerical facts reſpecting the blaſt afforded to iron furnaces by iron cylinder bellows worked by the ſteam engine; and as they agree very well with others given in my Chemical Dictionary, under the article *Trompe*, I will ſtate them in this place. Mr. Roebuck affirms, that one iron furnace was excited by a blowing cylinder, which gave 155 cubic feet of air 16 times per minute, which numbers give a product of 2480 cubic feet. This is 5½ times the quantity emitted by the blowing engine in the text. The ſteam engine was eſtimated to act by a preſſure of 13062 lbs. anſwering to 2¼ lbs. on the ſquare inch of the air piſton, and this multiplied by four feet eight inches, the length of ſtroke, and by 16, the number of ſtrokes, gives 975296 for the weight multiplied by its fall in feet.

compared with the water blowing engine.

Now the machine in the text was worked by 173 cubic feet, or 10812 lbs. of water falling through 21 feet, which gives a product of 227052, or more than one fourth of the firſt mover of the ſteam engine blaſt, inſtead of one fifth and a half. The blowing engine therefore conſumed more water by one fourth than would have been required to produce its effect, according to what was done by the ſteam engine But the ſteam engine drove out its air under a reaction of between five and ſix inches of mercury in the gage; a velocity which being more than Mr. Roebuck found neceſſary, was a diſadvantageous waſte of power. The velocity of the water blowing engine produced by its preſſure of two inches, is moſt probably too ſmall; and if ſo, the multiplication of theſe

The ſteam engine has more power.

theſe engines would not be adviſeable, even if Lewis had been in the right in ſuppoſing them to ſave water. Theſe rough computations, or rather eſtimates, are ſufficiently near for data ſo looſe as thoſe upon which we have operated; and they appear to ſhew that the principal, and perhaps the only recommendation of the water engines is, that many of them may be made and applied at a ſmall charge, in ſituations where water with a proper fall is plentifully to be had.

XII.

A Method of rendering the long and ſhort Vibrations of a Balance, governed by a ſpiral Spring, preciſely equal in Duration. By Mr. Charles Young. *In a Letter from the Inventor.*

To Mr. NICHOLSON.

SIR,

No explanation has been given why the long and ſhort vibrations of a balance are different.

I HAVE lately tried many experiments upon ſprings, with a view to obtain ſome knowledge of the cauſes which govern an effect that is very troubleſome to all makers of chronometers; namely, that the vibrations of the balance through ſhort arcs, conſiſting of perhaps ninety degrees, are in ſome inſtruments performed in longer, and in others in ſhorter times than thoſe long arcs, ſuch as of four hundred. It is certain that no ſatisfactory reaſons have been given, either in England or in France, to ſhow how this irregularity is produced.

A balance ſuſpended by a ſtrait wire had its long and ſhort vibrations equal.

I made a piece of braſs to ſerve as a large watch balance, and ſuſpended it by a bit of ſpring wire, on which it could vibrate as an axis, then having turned it four or five times, I left it to regain its natural poſition [*]. It performed all its oſcillations preciſely in equal times, whatever was their extent, whether they conſiſted of thirty degrees, or of three thouſand. It therefore returned to the place at which it was at reſt with velocity exactly proportioned to the forces employed to remove it. From this experiment I concluded, that the balance ſpring of a watch is not in a ſituation to exert this natural quality, but that the diſtortion into which it is thrown, is ſuch as deſtroy this valuable property of iſochroniſm.

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[*] This method of ſuſpenſion has been uſed for philoſophical purpoſes, by Mr. Mitchell, (ſee Prieſtley's Optics,) by Mr. Cavendiſh, (ſee Philoſ. Tranſ. and alſo this Journal quarto II. 446.) by Mr. Coulomb, in his numerous experiments on Electricity and Magnetiſm; and by Mr. Berthoud, in his Time Piece, No. 24. See his Treatiſe de la Meſure du Temps, p. 50. It does not appear that this ſpring has yet been uſed by itſelf in time pieces. N.

The principal circumſtance by which the ſpiral balance ſpring appears to me to be cramped, and prevented from operating by its natural action throughout, is, that the outer extremity is fixed by the ſtud, ſo that it cannot expand and contract in its coils every where alike, as it ought to do. To remedy this, I attached the ſtud to a ſtraight ſpring, lying in the direction of the tangent of the ſpiral, continued from that extremity. This ſpring by its eaſy action allows the ſpiral to approach the centre, and retire from it with great regularity; and, what is moſt material, it can with certainty be reduced to ſuch a ſtrength, that the long and ſhort vibrations of the balance will prove perfectly equal when this adjuſtment is made. For upon the ſtrength of this ſhort ſpring depends the freedom with which the axis of the balance is enveloped by that ſpring which regulates its motion. The ſpring ſtud affords a good banking; for the banking pin on the balance may be eaſily placed ſo as to ſtrike upon the end of the ſtud in the caſe of extreme vibration.

The ſpiral ſpring is cramped by the ſtud,

but when its outer end is made free by a ſtraight ſpring its vibrations are all iſochronal.

I am, Sir,

Your's moſt reſpectfully,

CHARLES YOUNG.

Wood Street, Aug. 23, 1805.

SCIENTIFIC NEWS.

Compoſition of Muriatic Acid.

Letter on the compoſition of muriatic acid.

IN the third number of the Edinburgh Medical and Surgical Journal, publiſhed July 1 laſt, is the tranſlation of a letter forwarded to the editors of that work by the celebrated Fabbroni. It bears date from Piſa, May 9, 1805, and is written by Dr. Franciſco Pacchioni, profeſſor of natural philoſopy in the univerſity of that city, to Sig. Lorenzo Pignotti.

Water decompoſed by galvaniſm ſhewed

After ſome prefatory obſervations, the writer announces that he has ſucceeded by galvaniſm in obtaining ſatisfactory evidence of the nature of the conſtituent principles of muriatic acid. He expreſſes his confidence that the ſimplicity of his apparatus and means have ſecured him againſt illuſion; but for want of time he forbears to relate the whole ſeries of his experiments. His reſults are,

—that muriatic acid is an oxide of hidrogen.

1. Muriatic acid is an oxide of hidrogen. 2. In the oxigenated muriatic acid and therefore, *à fortiori*, in muriatic acid there is a much leſs proportion of oxigen than in water. 3. Hidrogen may have very many and different degrees of oxidation.

Some account of the experiment.

The author informs us that having, by accurate experiments, aſcertained the true theory of galvaniſm, he readily diſcovered a very ſimple and exact apparatus, in which he could diſtinctly perceive the changes which happen to water, which, from the continued action of the galvanic pile, is conſtantly loſing its oxigen at the ſurface of a wire of very pure gold immerſed in it.

Water deprived of oxigen firſt became acid;

—and then appeared to have diſſolved part of the gold wire.

With this apparatus, which I conjecture muſt have been the ſame as that of Davy, in which the oxigen and hydrogen were given off in ſeparate veſſels of water, he obſerved that pure oxigen was emitted from the gold wire, that the water became acid, and when by proceeding in the operation until the reſidual fluid occupied about half the capacity of the receiver (that is, I preſume, when half the fluid in one of the veſſels had diſappeared) the remainder was found to be of an orange colour, more deep the leſs quantity of fluid. It reſembled a ſolution of gold. From the lower orifice of the veſſel, which was cloſed with a piece of taffetas and then with double bladder, a ſmell was emitted of oxigenated muriatic acid. The gold wire appeared corroded. The bit of taffetas which had been in contact with the coloured fluid had undergone an action which rendered it eaſily to be torn. Round the edges of the veſſel on the bladder there was a deep purple ring and within that a circular ſpace rendered colourleſs or white. A drop of the fluid itſelf tinged the ſkin of the hand after ſome hours, of a beautiful roſe colour.

Qualities of the fluid.

It contained muriatic acid.

The ſame liquid, poſſeſſing conſtantly the ſame qualities, was obtained in various repetitions of the experiment. It was ſhewn to contain a volatile acid by the white vapours which were formed by ammonia placed near it. It threw down a curdy precipitate from nitrate of ſilver, which the author concludes to have been the muriate of that metal; and from the whole of the facts he deduces the reſults firſt enumerated at the beginnig of this abſtract, reſpecting the compoſition of muriatic acid from water by depriving it of part of its oxigen. He promiſes to treat of the other oxides of hydrogen in a memoir ſhortly to appear.

Hence the origin of the ſalt of the ſea.

The origin and nature of the muriatic acid being thus, as the author obſerves, determined, there is no longer any myſtery in its formation, nor in that of the muriatic ſalts in the vaſt extent of the ocean.

Acid and alkali obſerved by Cruickſhank in 1801 to be formed by galvaniſm,

—and common ſalt by Peele in 1805,

Or muriate of ſoda.

The editors of the reſpectable Journal, from which I have made this extract take notice of the early diſcovery of Cruickſhank (publiſhed by him in our quarto ſeries for 1801) that infuſion of litmus was reddened by one end of the pile and infuſion of Brazil wood rendered purple by the other, which he aſcribed to the formation of nitrous acid and ammonia; and they alſo quote the diſcovery of muriatic acid being formed by the galvanic action by Mr. Peele of Cambridge, which was announced in Mr. Tilloch's Philoſophical Magazine a few days before Profeſſor Pacchioni's letter was publiſhed at Piſa. Mr. Peele's letter bears date April 23, 1805. He took a pint of diſtilled water and decompoſed half of it by means of galvaniſm; the other half, being then evaporated, left a ſmall quantity of muriate of ſoda or common ſalt. Great attention had been paid to the purity of the water; and upon a careful repetition the ſame reſult was again had. In a poſtſcript he mentions that a friend of his had tried the experiment and ſucceeded in the ſame manner.

Literary and Philoſophical Society of Newcaſtle upon Tyne.

Twelfth report of the Newcaſtle Lit. and Phil. Society.

THE Literary and Philoſophical Society of Newcaſtle-upon-Tyne have publiſhed their twelfth year's report. The ſpirited union of literature, ſcience and practical reſearch continues to form the character of their proceedings. Their library encreaſes no leſs in value than in magnitude, and they have liberally reſolved "that the ſubſcribers to the public li-"brary at North Shields (and to other ſimilar inſtitutions which "ſhall afford an equal accommodation to the members of the "Newcaſtle Society) ſhall be admitted to the rooms without "introduction on producing to the librarian a certificate of "their being members of ſuch inſtitutions." I will not ſuppoſe that any of my readers will conſider this information as merely local. The advantages of provincial ſocieties of eſtimable and well informed men is of high national importance, and it cannot but be of general intereſt that ſuch enlightened proceedings as are adopted in one part of the kingdom ſhould be known and imitated in every other quarter, where ſimilar circumſtances may render them deſirable.

Blaſting rocks in Mr. Jeſſop's method,

I have much pleaſure in adding the teſtimony of Northumberland in favour of the improvement in blaſting rocks, which Mr. Jeſſop communicated laſt December, through the channel of our Journal.

——tried with ſucceſs in Northumberland.

At* the meeting in April, 1804, Mr. Fogget of Sheriff-Hill reported, that the new mode of blaſting with ſand, deſcribed in the Philoſ. Journal had been tried by him, and that, contrary to his expectation, it had anſwered every purpoſe of the old mode, with a conſiderable ſaving of powder, and of more than one-third of the labour, and with an entire freedom from riſk.

At the meeting in May, Mr. Fogget preſented a ſection of two holes drilled and prepared for blaſting according to the new method: One perpendicular, in which the charge of powder being introduced and the communication-ſtraw placed, the remainder of the hole is filled up with fine dry ſand: the other a horizontal or aſcending hole; in which the powder and ſand, being made up into a cartridge, is in the act of being thruſt up to the fartheſt extremity of the hole, by a blunt-pointed pricker put in by the ſide of the communication ſtraw;

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* Report, page 5.

Danger of the old method of ramming down the charge.

At the meeting in June an account was communicated by Mr. Thornhill of an accident having happened in Gateſhead-Park colliery, by which one man loſt his life and another had been ſeverely wounded, in conſequence of the powder having taken fire in the common mode of ſtemming, or ramming down the charge with fragments of ſtone. A caſe was alſo cited by Mr. Horn of a perſon who had lately been brought from Alſton Moor with his ſkull fractured by a ſimilar exploſion.

New Proceſs for Steeping Hemp.

Steeping of hemp.

THE new proceſs of M. Bralle for ſteeping hemp, which has the advantage of ſaving time, capital, and the health of numerous individuals, and of which an account was given at page 86 of our laſt volume, has been repeated in one of the provinces of France, to the entire ſatisfaction of the inhabitants, who might be ſuppoſed the leaſt inclined to deviate from their accuſtomed habits. The ſtaple was found to be excellent, and of a ſuperior ſtrength and quality when ſpun into thread, and alſo after it had paſſed the loom in the form of cloth.

Medical Theatre, St. Bartholomew's Hoſpital.

THE following courſes of Lectures will be delivered at this theatre during the enſuing winter.

On the theory and practice of medicine, by Dr. Roberts and Dr. Powell.

On anatomy and phyſiology, by Mr. Abernethy.

On the theory and practice of ſurgery, by Mr. Abernethy.

On comparative anatomy and phyſiology, by Mr. Macartney.

On chemiſtry, by Dr. Edwards.

On the materia medica, by Dr. Powell.

Anatomical demonſtrations and practical anatomy, by Mr. Lawrence.

The anatomical lectures will begin on Tueſday, October 1ſt, at two o'clock, and the other lectures on the ſucceeding days of the ſame week.

Further particulars may be learned by applying to Mr. Nicholson, at the apothecary's ſhop, St. Bartholomew's hoſpital.

Medical Inſtitution.

AN inſtitution has been lately eſtabliſhed in London for the purpoſe of promoting a liberal and uſeful intercourſe among the different branches of the medical profeſſion, and of affording a centre for the reception of communications, and for the formation of a ſelect and extenſive profeſſional library. It is called the *Medical and Chirurgical Society of London*, and it comprizes a conſiderable number of profeſſional men of the firſt character. The meetings (which will commence in October) will be held at the Society's apartments, Verulam-buildings, Gray's-Inn, where any communications, or donations of books are requeſted to be ſent, directed to the ſecretaries.

The following is a liſt of the officers and council for the preſent year.

PRESIDENT, WM. SAUNDERS, M.D. F.R.S.

John Abernethy, Eſq. F.R.S. *Vice-Pres.*
Charles Rochemont Aikin, Eſq. Sec.
Wm. Babington, M.D. F.R.S. *Vice-Pres.*
Matthew Baillie, M.D. F.R.S.
Thos. Bateman, M.D. F.L.S.
Gilbert Blane, M.D. F.R.S.
Sir Wm. Blizard, F.R.S. *Vice-Pres.*
John Cooke, M.D. F.A.S. *Vice-Pres.*
Aſtley Cooper, Eſq. F.R.S. *Treas.*
James Curry, M.D. F.A.S.
Sir Walter Farquhar, Bart. M.D.
Thompſon Forſter, Eſq.
Algernon Frampton, M.D.
John Heaviſide, Eſq. F.R.S.
Alex. Marcet, M.D. *For. Sec.*
David Pitcairne, M.D. F.R.S.
Hen. Revell Reynolds, M.D. F.R.S.
H. Leigh Thomas, Eſq.
James Wilſon, Eſq. F.R.S.
John Yelloly, M.D. *Sec.*

Properties

Properties of blued Steel not generally known.

Method of making ſprings.

Hardening, blazing and blueing.

A blue ſpring injured by brightening and reſtored by blueing again.

IN making ſprings of ſteel the metal is drawn or hammered out and faſhioned to the deſired figure. It is then hardened by ignition to a low red heat and plunging it in water, which renders it quite brittle. And laſtly, it is tempered either by blazing or blueing. The operation of blazing conſiſts in ſmearing the article with oil or fat, and then heating it till thick vapours are emitted and burn off with a blaze. I ſuppoſe this temperature to be nearly the ſame as that of boiling mercury, which is generally reckoned to be at the 600° of Fahrenheit, though, for reaſons I ſhall in future mention, I think this point requires to be examined. The operation of blueing conſiſts in firſt brightening the ſurface of the ſteel, and then expoſing it to the regulated heat of a plate of metal or a charcoal fire, or the flame of a lamp until the ſurface acquires a blue colour by oxidation. The remarkable facts which I have here to preſent to the notice of philoſophers are that Mr. Stodart aſſures me that he has found the ſpring or elaſticity of the ſteel to be greatly impaired by taking off the blue with ſand paper or otherwiſe; and, what is ſtill more ſtriking, that it may be reſtored again by the blueing proceſs without any previous hardening or other additional treatment.

Saw makers harden their ſteel; then ſoften it by blazing; and render it elaſtic by blueing.

Soft ſteel blued. Hard ſteel expands more by heat.

Mr. Hardy, who is meritoriouſly known as a ſkilful artiſt,* aſſured me ſome time ago that the ſaw-makers firſt harden their plates in the uſual manner, in which ſtate they are more or leſs contorted or warped, and are brittle;—that they then blaze them; which proceſs deprives them of all ſpringineſs, ſo that they may be bended and hammered quite flat, which is a delicate part of the art of ſaw making;—and that they blue them on an hot iron which renders them ſtiff and ſpringy without altering the flatneſs of their ſurface. Mr. H. finds that ſoft unhardened ſteel may be rendered more elaſtic by blueing, and that hard ſteel is more expanſible by heat than ſoft.

It is very difficult to reaſon or even to conjecture upon theſe facts. They certainly deſerve to be verified by a direct proceſs of examination, which I intend to make, and ſhall ſtate the reſults in our next number. N.

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* See his banking for time pieces in our XI. Vol. page 114.

Cor-

Preſervation of Succulent Plants.

Whether the fact be generally in poſſeſſion of the collectors of plants I know not, but it will certainly be inſtructive to many readers to be informed that green ſucculent plants are much better preſerved after a momentary immerſion in boiling water than otherwiſe. This treatment, which I am told is adopted for the economical preſervation of cabbage and other plants which are dried for keeping, deſtroys the vegetable life at once, and ſeems to prevent an after proceſs of decay or mortification, by which the plant would have been more conſiderably changed, if it had not been ſo ſuddenly killed.

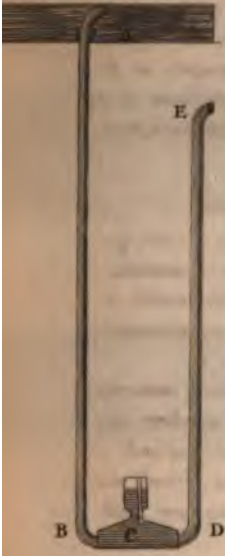
Corrections to the 11th Volume.

P. 159, l. 2, *area* r. *era*.—l. 21, *Agy* r. *Aqy*.—l. 36, $q \times p - 2a$ r. $q + p - 2a$.—l. 12, *for* r. *of*.—l. 28, put a comma before *that*.—p. 236, l. 20 from the bottom, after the words *preſerved as* add *as perfect as poſſible, but the preſs recommended by Dr. Withering does not appear calculated, &c.*—

Dr. Boſtock's eſſay upon animal fluids which was communicated by the author and inſerted in our laſt number, appeared alſo in the third number of the Edinburgh Medical and Surgical Journal. This by a caſual omiſſion of the friend who forwarded me manuſcript was not intimated to me until ſeveral days after the paper came to hand.

Engine for raising Water, by Mr. Wm. Close.

Fig. 1.



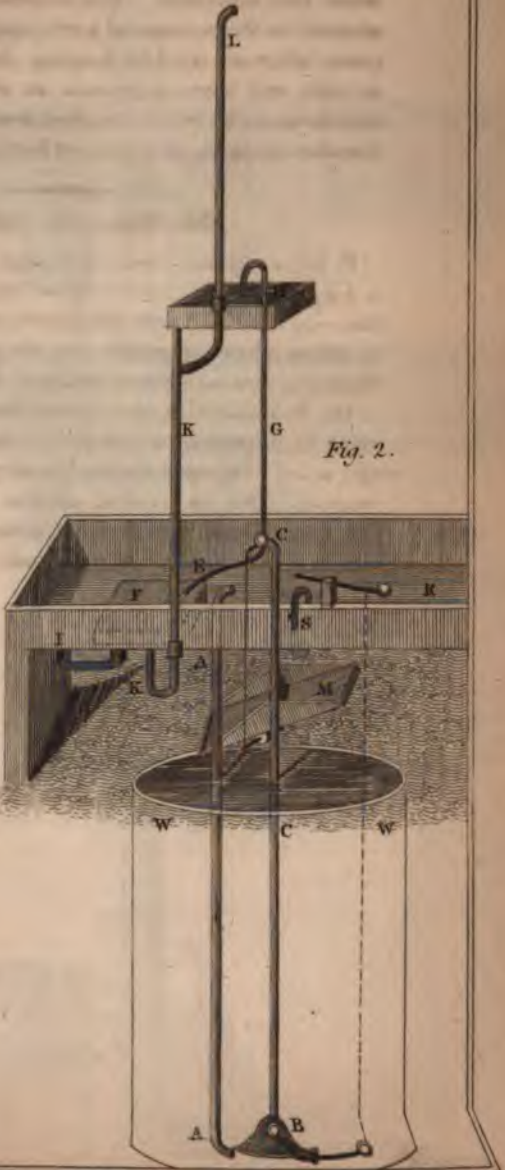


Fig. 2.

Fig. 3.

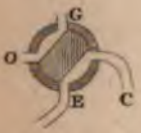


Fig. 4.

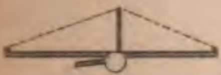
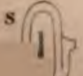


Fig. 5.



Engraved by Mutlow.

PUBL

Furnace for bending Wood.

Fig. 1.

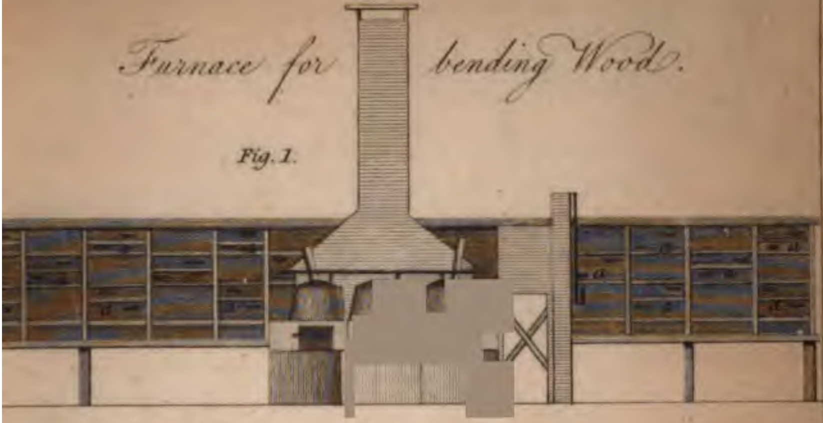


Fig. 2.

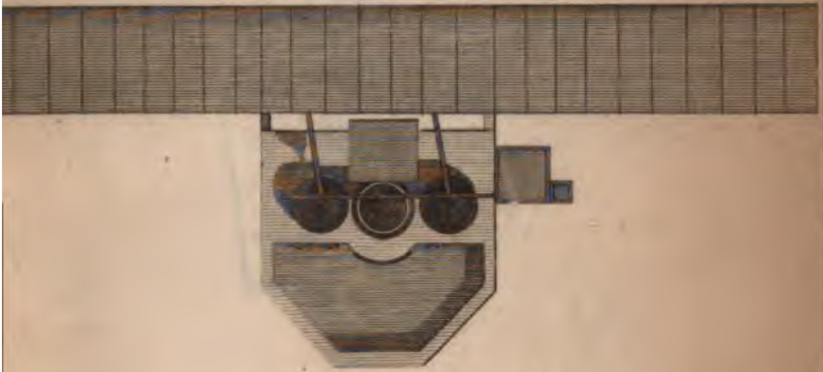
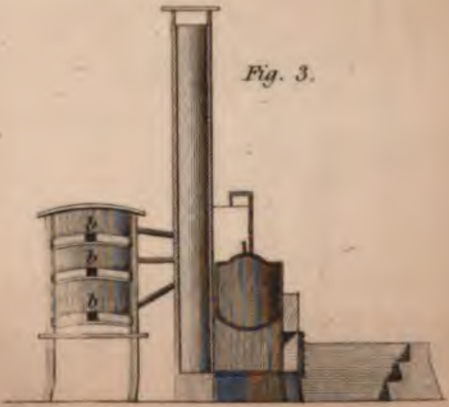


Fig. 3.



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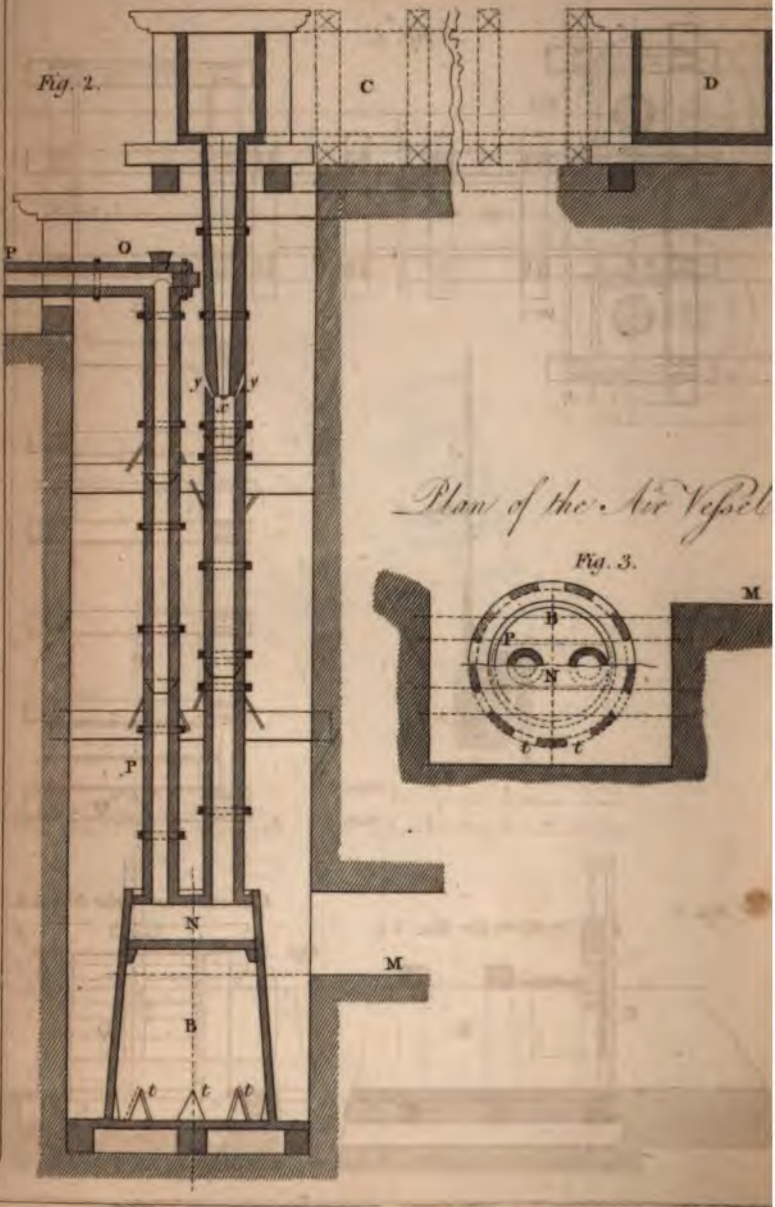
ASTOR, LENOX AND
TILDEN FOUNDATIONS
L

Blowing Engine by the fall of Water at Poullaouen

Profil on the line 11.

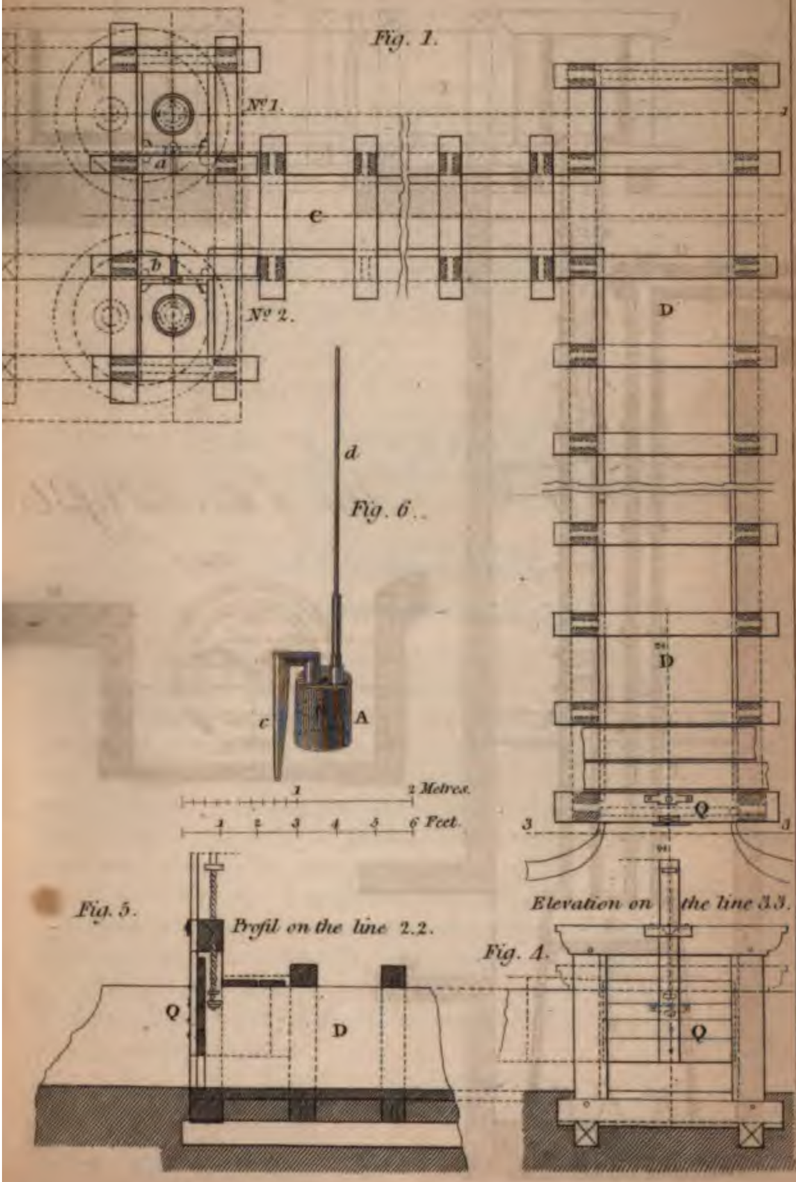
Fig. 2.



Plan of the Air Vessel

Fig. 3.

Plan of the Water Ways



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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

OCTOBER, 1805.

ARTICLE I.

Experimental Inveſtigations concerning Heat. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c. Received from the Author.*

To Mr. NICHOLSON.

DEAR SIR, *Munich, Auguſt* 29, 1805.

HAVING learned by a letter which I received this day from England, that you have publiſhed in your Journal of Natural Philoſophy the memoir I ſent you on the temperature at which the denſity of water is a maximum, I take the liberty to ſend you herewith incloſed three memoirs on heat, which are deſtined to appear in the next volume of the publications of the firſt claſs of the National Inſtitute.—Three other memoirs of mine will appear in that volume, but as they contain little that would be new to you, I do not ſend them to you.

I continue my reſearches on heat, and have lately made ſeveral new and very intereſting experiments, the reſults of which it is my intention to communicate to you, as ſoon as I ſhall have compleated the particular courſe of experiments in which I am now engaged.

I am, Dear Sir, with much eſteem,
Your moſt obedient ſervant,
RUMFORD.

SECT. I. *Short Account of a new Experiment on Heat.*

I have lately made a new experiment, the reſult of which appears to me ſufficiently intereſting to deſerve the attention of the claſs.

Qu. Whether the heating and cooling of poliſhed and of blackened bodies follow the ſame law in ſmall cloſed ſpaces as in ſpaces more enlarged?

Having found by experiments often repeated that metallic bodies expoſed in the free air of a large apartment are much more ſpeedily heated and cooled when their ſurfaces have been blackened (over the flame of a candle for example) than when they are clean and poliſhed; I was curious to know whether the ſame phenomena would take place when, inſtead of expoſing theſe bodies in the open air, they ſhould be placed in cloſe metallic veſſels ſurrounded by a certain thickneſs of included air, and theſe veſſels ſhould be then plunged in a large maſs of hot or cold water. In order to clear up this important point, I made the following experiment:

A cylindrical veſſel of thin braſs was ſupported in the middle of a larger veſſel, ſo as to leave a thin interval of air between them.

A cylindrical veſſel of braſs, three inches in diameter and four inches long was encloſed in another larger cylindrical veſſel, in the centre of which it was ſuſpended by its neck, ſo as to touch it in no other part, leaving on all ſides an interval of one inch between the veſſels.

The external veſſel as well as the ſmaller one included within it is made of thin ſheets of braſs; its diameter is five inches and its height ſix. It is one inch and a half in diameter and ſix inches high. Its neck is one inch and a quarter in diameter and two inches and a half long.

The interior veſſel is ſuſpended in the centre of the external one by a ſtopper of cork. This ſtopper is adjuſted to the neck of the external veſſel, and there is a cylindrical hole of three quarters of an inch diameter through the cork, and having the ſame axis, which perforation receives the neck of the interior veſſel and retains it in its place.

The interior veſſel was introduced and fixed in its place before the bottom of the exterior veſſel was ſoldered in.

The larger veſſel was ſupported on a foot.

At the centre of the bottom of the great veſſel is a ſmall metallic tube of three quarters of an inch diameter and one inch and a half long, by means of which this inſtrument is attached to a ſolid heavy foot of metal which ſupports it in a vertical poſition when the whole inſtrument is ſubmerged in a veſſel of water.

This inſtrument, which greatly reſembles that deſcribed in my 7th eſſay on the propagation of heat in fluids, which I have called the Paſſage Thermometer*, may be uſed to make a number of intereſting experiments on the cooling of bodies through different fluids. In the preſent experiment I employed it in the following manner:

The interior veſſel was entirely filled with hot water to the height of half an inch in its neck, and a good thermometer, having its cylindric bulb four inches long, was inſerted therein. The inſtrument was then plunged in a mixture of pounded ice and water, and the time was noted by means of the thermometer, during which the hot water in the ſmall veſſel became cold.

The inner veſſel was filled with hot water, and a thermometer placed in its neck.

The whole inſtrument was then plunged in ice and water.

I was careful to plunge the inſtrument in this frigorific mixture, ſo that the large veſſel was completely ſubmerged except the upper extremity of its neck; and I added from time to time a ſufficient quantity of pounded ice, to keep the frigorific mixture conſtantly and throughout at the temperature of melting ice.

The following were the reſults afforded by two ſimilar inſtruments employed at the ſame time:

Theſe two inſtruments, which I ſhall diſtinguiſh reſpectively by the letters A and B, are of the ſame form and dimenſions; there is no difference between them but in the ſtate of their ſurfaces. In the inſtrument A, the exterior ſurface of the ſmall veſſel and the interior ſurface of the great veſſel which incloſes it, are bright and poliſhed; but in the inſtrument B, the exterior ſurface of the ſmall veſſel and the interior ſurface of the large veſſel are black, having been blackened over the flame of a candle before the bottom of the great veſſel was ſoldered in its place.

Reſults with two inſtruments one of which, B, had the interior ſurface of the larger veſſel and the exterior of the ſmaller blacked; and the other inſtrument, A, had the like ſurfaces poliſhed.

Having filled the interior veſſel of each of theſe inſtruments with boiling water till the water roſe to the height of half an inch in the neck, I placed a thermometer in each, and then plunging both inſtruments at the ſame time into a tub filled with cold water mixed with pounded ice, I obſerved the courſe of their refrigeration during ſeveral hours.

The interior veſſels contained boiling water.

Each of the inſtruments was completely ſubmerged in the frigorific mixture, excepting about one inch of the ſuperior extremity of the neck of the exterior veſſel, and I was careful

The refrigerating veſſel contained ice and water.

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* See our Journal, Vol. IX.

to add new quantities of pounded ice from time to time, in order to keep the frigorific mixture conſtantly at the preciſe temperature of melting ice.

Caution to inſure equality of temperature.

As the ſpecific gravity of water at the temperature of three or four degrees of the thermometer of Reaumur, is greater than that of melting ice, the water which lies at the bottom of a veſſel containing a mixture of water and pounded ice, is uſually warmer than the fluid which occupies the upper part of the veſſel. To remedy this inconvenience my refrigeratory for the frigorific mixture was a tin veſſel ſupported on three feet of one inch in length, and I placed this firſt veſſel in a larger of wood, containing a certain quantity of ice ſurrounding the bottom and part of the ſides of the metallic veſſel.

Method of obſervation.

As in the firſt moments of the experiment the thermometers deſcended too quickly to be obſerved with preciſion, I waited till each of them had arrived at the 55th degree of Reaumur; after which I carefully obſerved the number of minutes and ſeconds employed in paſſing through each interval of five degrees of the lower part of the ſcale of the thermometer to the fifth degree above zero.

Table of the courſe of cooling.

The following table exhibits the depreſſion of the thermometers during eight hours employed in the experiment.

Degrees of the Thermometer.		*Time employed in cooling,*			
		By the Inſtrument A.		By the Inſtrument B.	
From 55 to	50	11^m	6^s	7^m	50^s
50	45	13	15	8	10
45	40	15	12	9	5
40	35	19	10	10	50
35	30	22	24	12	18
30	25	27	50	15	10
25	20	37	6	21	15
20	15	54	15	28	15
15	10	80	25	41	25
10	5	183	45	85	15
Time employed in cooling from 55° to 5°,		478	4	254	5

It is evident from the reſults of this experiment, that the blackened body is conſtantly cooled in leſs time than the poliſhed body; but it appears by the courſe of the thermometers, that the difference between the quickneſs of cooling of theſe two bodies varies, and that this difference was leſs conſiderable in proportion as the temperature of the bodies was more elevated in compariſon to that of the medium in which they were expoſed to cool.

The blackened body always cooled more quickly than the other.

In cooling from the 55th degree to the 50th above the temperature of the ſurrounding medium, the poliſhed body employed $11^{m}\ 6^{s}$, and the blackened body employed $7^{m}\ 50^{s}$ to paſs through the ſame interval. But from the 10th to the 15th degree above the temperature of the medium, the poliſhed body employed $183^{m}\ 45^{s}$, while the blackened body employed only $85^{m}\ 15^{s}$; but it is extremely probable that this difference between the proportion of the times employed in cooling the two bodies at different temperatures, is only apparent, and that it depends on the greater or leſs time required for the thermometers in the veſſels to arrive at the mean temperatures of the maſſes of water which ſurround them.

The difference was greateſt at the loweſt temperatures; probably becauſe the thermometers then ſhewed the mean temperature more correctly.

In order to compare the reſults of this experiment with thoſe I made laſt year with metallic veſſels poliſhed and blackened, and left to cool in the undiſturbed air of a large chamber, it is neceſſary to aſcertain how much time the two bodies in queſtion employed in cooling, from the 50th to the 40th degree of Fahrenheit above the temperature of the medium. Now I found by obſervation, that the poliſhed veſſel A employed $39^{m}\ 30^{s}$ to paſs over that interval of cooling, while the blackened veſſel B employed only 22^{m}. Theſe times are in the proportion of 10000 to 5810. By one of my experiments made laſt year, I found that the times employed in paſſing through the ſame interval of cooling in the open air by a clean poliſhed metallic veſſel, and another of the ſame form and capacity, but blackened without, were as 10000 to 5654.

From theſe experiments it appears that the rate of cooling in the poliſhed body, compared with the other, is nearly the ſame as was formerly determined with bodies in a large chamber.

Reflecting on the conſequences which ought to reſult from the radiations of bodies, on the ſuppoſition that the temperatures of bodies are always changing by means of theſe radiations, I was led to the following concluſion: If the intenſity of the action of the rays which proceed from a body, be univerſally as the ſquares of the diſtances of bodies inverſely,

If the intenſities of radiated heat be inverſely as the ſquares of the diſtances, bodies will cool in the ſame time in an encloſure of the ſame temperature, whether large or ſmall.

which is extremely probable, a hot body expoſed to cool in a cloſe place, or ſurrounded on all ſides by walls, ought to cool with the ſame celerity, or in the ſame time, whatever may be the magnitude of this encloſure, provided the temperature of the ſides or walls be at a conſtant given temperature; and the reſults of the experiment here deſcribed, in which the hot body was encloſed in a veſſel of a few inches diameter, compared with thoſe of ſeveral experiments made laſt year, in which the heated bodies expoſed to cool between the walls of a large chamber, appear to confirm this concluſion.

Theſe facts confirm that truth;

and that the air has little effect.

As to the effect produced by the air in cooling a heated body expoſed to cool in a cloſe place filled with that fluid, I have reaſon to believe that it is much leſs conſiderable than has been ſuppoſed.

Former experiments proved that it receives only 1-27th part.

I have ſhewn by direct and concluſive experiments, that bodies cool and are heated, and that with conſiderable celerity, when placed in a ſpace void of air[*]; and, by experiments made laſt year with the intention of clearing up this point, I found reaſons to conclude, that when a hot body cools in tranquil air not agitated by winds, one twenty-ſeventh only of the heat loſt by this body (or to ſpeak more corectly, which it excites in ſurrounding bodies) is communicated to the air, all the reſt being carried to a diſtance through the air, and communicated by radiation to the ſurrounding ſolid bodies.

The reſt paſſes by radiation.

SECT. II. *Experiments on cooling Bodies.*

It is only by careful obſervation of the phenomena which accompany the heating and cooling of bodies, that we can hope to acquire exact notions of the nature of heat and its manner of acting.

Conducting power of bodies with regard to heat.

Many experiments have been made by different perſons at different times, with a view to determine what has been called the conducting quality of different ſubſtances with regard to heat: I have myſelf made a conſiderable number; and it is from their reſults, often no leſs unexpected than intereſting, that I have been gradually led to adopt the opinions on the nature of heat which I have preſumed to ſubmit to the judgment of this illuſtrious aſſembly. The flattering attention with which the Claſs has honoured the three Memoirs I have lately preſented, encourages me to communicate the continuation of my reſearches.

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[*] See my Memoir on Heat in the Philoſophical Tranſactions for 1786, and in my eighth Eſſay.

All philoſophers are agreed in conſidering glaſs as one of the worſt conductors of heat which exiſts; and when it is propoſed to confine the heat in a body of which the temperature has been raiſed, or to hinder its diſſipation as much as poſſible, care is taken to ſurround the heated body with ſubſtances known to be bad conductors of heat.

Glaſs is allowed to be one of the worſt conductors.

The reſults of many of my experiments having led me to ſuſpect that the cooling of bodies is not effected in the manner which is generally ſuppoſed, I made the following experiment with the intention of clearing up this intereſting part of the ſcience.

Bodies are not, probably, cooled by conducting off.

I procured two bottles nearly cylindrical, of the ſame form and the ſame dimenſions when meaſured externally; one being of glaſs and very thick, and the other of tin or tinned iron, which was very thin. Each of them is three inches ten lines in diameter very nearly, and five inches in height, and each has a neck one inch three lines in diameter, and one inch two lines in height. The glaſs bottle weighs 13 ounces 1 gros and 18 grains poids de marc, and the other thin metallic veſſel weighs only 5 ounces 1 gros and 65 grains.

Having very exactly weighed the bottle of tinned iron, I found its exterior ſurface to be 54,462 inches, which give 0,21142 of a line for the thickneſs of its ſides, taking the ſpecific gravity of the metal at 7,8404.

The mean thickneſs of the ſides of the glaſs bottle is more than ſix times as great, as may be eaſily deduced from a calculation founded on the weight of the bottle, the quantity of its ſurface, and the ſpecific gravity of glaſs.

Experiment: A thick glaſs bottle and a thin one of tin, were prepared, weighed, and meaſured.

Having filled theſe two bottles with boiling water, I hung them up by ſlender ſtrings in the midſt of the tranquil air of a large chamber, at the height of five feet from the floor, and at the diſtance of four feet aſunder.

They were filled with hot water, and left to cool in the air.

The temperature of the air of the chamber, which did not vary a quarter of a degree during the whole time of the experiment, was 9¼ degrees of Reaumur's ſcale.

An excellent mercurial thermometer, with a cylindrical bulb, of four inches long and two lines and a half in diameter, ſuſpended in the axis of each of theſe bottles, indicated the temperature of the contained water; and the time employed in its cooling for every five degrees of Fahrenheit's thermometer, was carefully obſerved during eight hours.

The glaſs being conſidered as a very bad conductor of heat, and the ſides of the bottle being ſo thick, who would not have expected that the water in this bottle would have been more ſlowly cooled than that in the very thin bottle of tin.

The glaſs bottle cooled twice as quick as that of tin.

The contrary however was the event; the bottle of glaſs was cooled almoſt twice as quickly as that of tin.

While the water included in the bottle of tinned iron employed 56 minutes to paſs through a certain interval of cooling, namely through ten degrees, between the 50th and 40th degree of the thermometer of Fahrenheit above the temperature of the air of the chamber, the water in the glaſs bottle employed only 30 minntes for the ſame change.

Inference.

It appears to me, that the reſult of this experiment throws great light on the myſterious operation of the communication of heat.

Heat is not likely to be a material ſubſtance.

If we admit the hypotheſis that hot bodies are cooled, not by loſing or acquiring ſome material ſubſtance, but by the action of colder ſurrounding bodies, communicated by undulations or radiations excited in an etherial fluid, the reſults of this experiment may be eaſily explained; but if this hypotheſis be not adopted, I cannot explain them.

Bodies are not cooled by the ſurrounding air.

It might perhaps be ſuſpected that the air attached by a certain attraction, but with unequal forces, to the ſurfaces of the two bottles, might have been the cauſe of this remarkable difference in the time of their cooling; but thoſe who will take the trouble to reflect attentively on the reſults of the experiments I have deſcribed in a preceding memoir, which were made with a view to clear up this point, with a metallic veſſel firſt naked, and afterwards with one, two, four and five coatings of varniſh, will be perſuaded that this cauſe is not ſufficient to explain the facts.

All metallic veſſels have the ſame diſpoſition to cool.

By a courſe of experiments made at Munich laſt year, of which the details are given in a Memoir ſent to the Royal Society of London *, I have found that a given quantity of hot water included in a metallic veſſel of a given form and capacity, always cools with the ſame quickneſs in the air,

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* See our Journal, Vol. IX. p. 194.

whatever may be the metal employed to conſtruct the veſſel; provided always that the external ſurface of the veſſel be very clean, and the temperature of the air the ſame.

In order that the cooling ſhall be effected in the ſame time, nothing more is required than that the external ſurface of the veſſel be truly metallic, and not covered with oxide or other foreign bodies.

The ſurface only need be metallic.

On the enquiry, what quality all the metals might have in common, and poſſeſs in the ſame degree, to which this remarkable equality of their ſuſceptibility of cooling might be attributed, I found it in their opacity.

This ariſes from the opacity of metals,

The rays which cannot penetrate the ſurface of a body, muſt neceſſarily be thrown back or reflected; and as the rays of light, which have much analogy with the inviſible calorific or frigorific rays, eaſily penetrate glaſs, though they are reflected, at leaſt for the greateſt part, by metallic ſurfaces, I ſuſpected beforehand the reſult of the experiment with the two bottles, one of glaſs and the other of tinned iron.

by which the heat is reflected.

The ſtate of a heated body, or a body which contains a certain quantity of caloric, has been compared to that of a ſponge which contains a certain quantity of water. Suppoſing this compariſon to be juſt, we might compare the loſs of heat by the emiſſion of the calorific rays, to the loſs of water by evaporation. Let us try if this compariſon can ſupply us with the means of throwing ſome light on the intereſting ſubject of our reſearches.

Uſual compariſon of heat to water in a ſponge.

Inſtead of the ſponge filled with water, let us ſubſtitute the earth, and ſuppoſe for a moment that the earth is every where equally heated, and its ſurface in all parts covered with a bed of the ſame kind of ſoil equally moiſt.

The ſame amplified.

As a ſquare league in a mountainous country contains more ſurface or more ſuperficial acres than a ſquare league ſituated in the plain, it is evident that more water would be evaporated from the whole ſurface of the earth in a given time if the earth were covered with mountains, than if its ſurface were an immenſe plain, and conſequently, that more caloric ought to be projected from the ſurface of any ſolid body broken with aſperités, than from the ſurface of another body of the ſame form and dimenſions, which is ſmooth or well poliſhed.

If it were true, a rough ſurface would emit more heat than a ſmooth one.

This

But the facts are contrary.

This reaſoning appears to me to be juſt, and if I am not deceived, the concluſions which may be drawn from the facts in queſtion, well confirmed by experiment, ought to be conſidered as demonſtrative. I have taken every poſſible care to eſtabliſh theſe facts; and the reſults of all my experiments have conſtantly ſhewn that more or leſs perfect poliſh, or the greater or leſs brightneſs of the ſurface of a metallic veſſel, does not ſenſibly influence the time of its cooling.

A poliſhed and unpoliſhed veſſel of braſs cooled in the ſame time.

I took two equal veſſels of braſs and poliſhed the external ſurface of one of them as highly as poſſible; and I deſtroyed the poliſh of the other by rubbing it in all directions with coarſe emery. When theſe two veſſels were filled with hot water, I did not find that the unpoliſhed veſſel employed more or leſs time in cooling than that which was poliſhed.

Caution.

I was careful to waſh the ſurface of the unpoliſhed veſſel effectually with water before the experiment; as I knew that if I did not take the precaution of removing all the dirt which might be lodged in the aſperités of the ſurface, the preſence of theſe ſmall foreign bodies would influence the reſult of the experiment in a ſenſible manner.

A rough ſurface may reflect as much light as a ſmoother.

We ought carefully to diſtinguiſh thoſe ſurfaces which appear unpoliſhed to our eyes, but which in fact are not ſo, from thoſe which reflect little or no light.

Metals not leſs reflective for loſing their poliſh.

It is more than probable that the ſurface of a metal is always poliſhed, and even always equally ſo in all the caſes wherein the metal is naked and clear and clean, notwithſtanding all the mechanical means which may be uſed to ſcratch its ſurface and break the glare of its luſtre.

If the radiation of heat deſcend on ſurrounding bodies, it will be of no conſequence whether the radiating body be poliſhed or not.

Let us return to the compariſon of the evaporation of water from the ſurface of the earth, with the emiſſion of caloric radiating from the ſurface of a heated body, and let us ſuppoſe for an inſtant, that the evaporation of the water from the ſurface of the earth does not depend on the heat of the earth itſelf, but that it is cauſed merely by the influences of ſurrounding bodies, as for example, by the rays of light received from the ſun. It is evident that, in this caſe, the evaporation could not be ſenſibly greater in a mountainous country than in the plain; and by an eaſy analogy we ſee, that if hot bodies be cooled, not in conſequence of the emiſſion of ſome material ſubſtance from their ſurfaces, but by the poſitive action of rays ſent to them by colder ſurrounding bodies the more or leſs perfect poliſh of their ſurfaces, ought not ſenſibly to influence the rapidity of their cooling.

Experiments prove this poſition.

This is preciſely what all my experiments concur to prove. I have long ſought, and with that patience which the love of the ſciences inſpires, to reconcile the reſults of my experiments with the opinions generally received concerning the nature of heat and its mode of action, but without being able to ſucceed.

It is in the hands of two of the moſt illuſtrious bodies of learned men that ever exiſted that I have thought it incumbent on me to depoſit my labours, my diſcoveries, my doubts, and my conjectures.

I am earneſtly deſirous of engaging the philoſophers of all countries to turn their attention towards an object of enquiry too long neglected.

Importance of the ſcience of heat.

The ſcience of heat is not only of great curioſity, from the multitude of aſtoniſhing phenomena it offers to our contemplation, but it is likewiſe extremely intereſting from its intimate connection with all the uſeful arts, and generally with all the mechanical occupations of human life.

Without a knowledge of heat it is not poſſible either to excite it with economy, or to direct its different operations with facility and preciſion.

(The Remainder in our next.)

II.

On pure Nickel, diſcovered to be one of the noble Metals, and on its Preparation and Properties. By J. B. Richter.*

Cobalt ſeparated from ſulphate of ammonia and nickel by repeated cryſtalliſation. Copper, by ſubliming with ſal ammoniac.

On repeatedly cryſtallizing ſulphate of ammonia and nickel, the whole of the cobalt, an extremely ſmall quantity excepted, will be ſeparated; but after this there ſtill remains ſome copper mixed with the ſalt. I have already announced that this metal may be ſeparated from the nickel by ſubliming the latter with ſal ammoniac; but at that time I had never obtained pure nickel. With the compound ſalt of nickel and ammonia

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* Tranſlated from the *Journal de Chimie* of Van Mons, vol. VI. p. 183, March, 1805; and abridged by Van Mons from the *Allgemeines Journal der Chemie*, 1804, vol. III. p. 244.

Arſenic and iron left.

a little arſenic ſtill remains; and there may be iron likewiſe, when we have been a little too ſparing in the addition of nitric acid to the ſulphuric ſolution of cobalt containing calx of nickel.

The triple ſalt decompoſed by carbonate of potaſh.

I endeavoured to ſeparate theſe extraneous metals in the humid way, but not with complete ſucceſs. I decompoſed by means of carbonate of potaſh the triple ammoniacal ſalt of nickel, free from iron, and as much as poſſible from cobalt: the precipitate ſtill was viſibly of a greeniſh blue. Having edulcorated it, and heated it to redneſs, it changed its colour, as it loſt its carbonic acid, to a blackiſh gray, which however inclined evidently to a green. The water of edulcoration, which had a greeniſh appearance, was evaporated to dryneſs, and the reſiduum, after being heated red hot, was waſhed again. A green powder remained, which did not loſe its colour in the fire, and conſiſted in great part of arſeniate of nickel.

The metal reduced.

I mixed each of the two reſiduums ſeparately with a fifth part of charcoal, and expoſed them to the heat of a potter's furnace for eighteen hours in a cupel with a little porcelain glaze. The metallic buttons obtained differed a little from each other. Each endured a few blows with a hammer without cracking; but that of the latter reſiduum was much more white and fragile than that of the former, the colour of which approached that of ſteel and was ſlightly reddiſh. They were both attacked with avidity by nitric acid, and they were attracted by the magnet, but the former only weakly.

The reſult not pure nickel.

As from ſeveral effects on porcelain it appeared to me probable, that pure nickel was a noble metal, I diſſolved afreſh in nitric acid the whole quantity reduced, which amounted to ſeveral ounces, and evaporated the ſolution to dryneſs. I then poured water on the ſaline maſs, and a beautiful green ſolution was formed; but a greeniſh white reſiduum remained, in which I eaſily detected the preſence of iron, nickel, and arſenic acid.

The ſolution precipitated and expoſed to a ſtrong heat.

The ſolution, which beſide arſenic, contained a conſiderable portion of copper, was precipitated by carbonate of potaſh, and the reſiduum, the colour of which was ſtill very lively, though not ſo green as that of carbonate of copper, was well waſhed and expoſed to a white heat. This changed its apple green colour to a deep green inclining to gray and brown. With a ſtronger heat the maſs aſſumed a grayer brown, and at the ſame time appeared to coagulate. There were likewiſe portions of reduced metal in it, that could not be miſtaken.—I could not, however, accompliſh its fuſion in a wind-furnace, ſurmounted with a cupellating furnace dome, and having a long chimney. In conſequence, I divided it into ſeveral portions, which I expoſed in crucibles to the ſtrongeſt heat of a potter's furnace, in which capſules of the moſt refractory clay are frequently ſoftened.

Very refractory.

Fuſed in the ſtrongeſt heat of a potter's furnace.

In thoſe crucibles which were placed where the porcelain is longeſt taking, the matter had experienced no change but a coagulation. In the other crucibles it had entered into complete fuſion, yet not into a liquid fuſion, and the crucibles had partly experienced the ſame effect. Here and there in the melted maſs metallic globules were found, the largeſt of which were the ſize of a ſmall nut, and the leaſt that of a cherry-ſtone. Their brilliancy was a mean between that of ſilver and that of Engliſh tin. The ſcoriæ were greeniſh brown, mixed with an amethyſt colour, and in ſome places a deep blue entirely like fuſed oxide of cobalt. The brown colour aroſe from the oxide of copper, which was completely vitrified, and the blue from that of cobalt. The green, on the contrary, proceeded from arſeniate of nickel, which, as I have learned by experience, ſtrongly reſiſts fuſion, without the addition of ſome combuſtible ſubſtance.

Scoriæ.

Arſeniate of nickel.

The metal malleable, and magnetic.

I attempted to hammer the metallic globules on an anvil, and to my great ſatisfaction I found that they poſſeſſed conſiderable malleability. They were eagerly attracted by the magnet.

Re-fuſion.

As it was impoſſible to ſeparate the ſcorie with the hammer from the little globules to which they adhered, I collected them together by trituration and decantation, and expoſed them to fuſion afreſh. It was again complete only in the places of the furnace moſt heated.

Reduced without any addition.

Convinced from theſe reſults, that nickel is reducible in the fire without the addition of any combuſtible matter, I attempted to reduce ſome oxide of this metal, obtained by the decompoſition of the triple ammoniacal ſalt of nickel, which during an uninterrupted labour of eighteen months, I had procured in a very large quantity. On this occaſion the ſame phenomena occurred as in the preceding reductions.

A button of an ounce and half procured.

I repeated the melting till the metal had undergone a complete fuſion, and was found collected together in a button at the bottom of the crucible. In one crucible which had been expoſed to the ſtrongeſt heat, I obtained a button that weighed an ounce and a half.

Beſt reduced alone.

I was leſs ſucceſsful in my fuſion when I mixed the oxide of nickel with porcelain glaze, or when I ſimply covered it with this glaze; ſo that I was convinced the beſt proceſs was to reduce the oxide of nickel directly.

After much time and patience, I ſucceeded in obtaining ſeveral ounces of nickel, which I muſt conſider as abſolutely pure, and I ſhall now proceed to deſcribe the principal characters that I have perceived in it in this ſtate.—To begin with the external characters.

External character of pure nickel.
Colour.
Unchangeable in the air.
Malleable.

The colour of pure nickel is a mean between ſilver and tin. It undergoes no alteration either from the air of the atmoſphere or from the water in it: in other words it is inſuſceptible of being oxided by the air.

It is perfectly malleable; as it may not only be forged into bars when red hot, but hammered on the anvil while cold into very thin plates. This character removes nickel from the claſs of ſemi-metals to that of perfect metals.

Specific gravity.

Its denſity or ſpecific gravity is pretty conſiderable: from repeated experiments with my hidrometer caſt nickel weighs 8.279, and forged nickel 8.666.

Ductile.

Its tenacity likewiſe appears conſiderable, to judge from its great ductility. A piece of caſt nickel, weighing five drams allowed itſelf to be flattened, but not without cracking, into a plate of 13 ſquare inches Rhynland meaſure, which gives leſs than $\frac{1}{100}$ of an inch for its thickneſs. It might probably be drawn into a wire of the ſame tenuity.

Refractory.

The reſiſtance of nickel to fuſion is very conſiderable, and equals, if it do not ſurpaſs, that of mangane ſe. The reader may have obſerved, from my attempts to fuſe it, how difficult it is to obtain any thing deciſive on this head.

A noble metal.

At a temperature ſufficiently high the pure oxide of nickel is reducible without the addition of any combuſtible matter. Its great reſiſtance to fuſion is the only cauſe why this reduction preſents ſo many difficulties. Very little oxidation too is perceptible on keeping this metal in a ſtate of incandeſcence: it is merely tarniſhed a little more than platina, gold, or ſilver, Nickel therefore belongs not to the claſs of perfect metals merely, but to that of noble metals.

Magnetic.

The action of the magnet on nickel is not only very conſiderable

ible, and little inferior to that on iron; but nickel becomes
f magnetical, or acquires polarity, by the touch, and even
art by ſtriking it with a hammer, or filing it, with the pre-
tions ſuitable for producing this effect. I diſcovered the
er property by preſenting to the magnet a ſlip of forged
cel; when, notwithſtanding it was poliſhed by the file, it
ered more feebly to the magnet than other ſlips leſs poliſhed;
on my preſenting its other extremity to the magnet, it ad-
ed to it with great force. It likewiſe attracted by either
not only iron needles, but plates of nickel half an inch
are, which it cauſed to move about on a ſmooth table.

Its magnetic property weakened by copper, deſtroyed by arſenic.

Magnetic and malleable in proportion to its purity.

The property which nickel poſſeſſes of becoming magnetic
ot deſtroyed, though weakened by its alloy with copper;
arſenic deſtroys it completely. I had frequent opportuni-
of making this obſervation in the courſe of my experiments.
ne nickel, from which I had ſeparated the iron* and the
nic in the humid way, and which I had afterwards reduced
h the addition of a combuſtible ſubſtance, was malleable,
l attracted the magnet, though not ſo forcibly as pure nickel.
e ſame metal, purified with leſs care, was leſs malleable,
l proportionally leſs attractable by the magnet. Repeated
oſure of the metal to the moſt powerful heat of a porce-
furnace did not in the leaſt reſtore to it this property.—

Copper muſt be ſeparated by fire.

ne experiments, which I ſhall hereafter relate, have con-
ced me, that copper cannot be entirely ſeparated from
kel in the humid way, and that the only means of ſeparat-
them is to reduce the cupreous oxide of nickel by fire.

Action of the acids upon nickel.

The ſulphuric and muriatic acids have little action upon
kel. The oxide of this metal by the air does not diſſolve
n in the latter of theſe acids without the aſſiſtance of a
ng ebullition. The moſt appropriate ſolvents of nickel are
nitric and nitro-muriatic acids. I have already mentioned,
t impure nickel, particularly the cupreous is attacked by
nitric acid with heat and vivacity. The action of the ſame
d on pure nickel is a little different, and particularly on the

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Beſt mode of freeing it from iron.

* The ſeparation of the iron ſucceeds beſt by a rapid evaporation
the nitric ſolution of the ferruginous nickel, by which the iron
recipitated in the form of an inſoluble oxide. At the ſame time
ittle arſenic is ſeparated in union with the iron. It is preferable,
wever, to ſeparate the arſenic firſt, which is effected by the help
a nitric ſolution of lead. The lead is afterward to be precipi-
ed by a ſolution of ſulphate of potaſh.

hammered metal. I have poured nitric acid on nickel both in buttons and laminated, expecting a very active ſolution; but it has proceeded ſlowly, and I have even been obliged to have recourſe to the heat of a ſpirit lamp to accelerate it. The diſſolution however having appeared to ceaſe, I decanted the liquid and poured on the reſiduum a freſh quantity of acid of the ſame ſtrength as the preceding, when on a ſudden ſuch a briſk action came on, accompanied with the evolution of heat *, that I could not remove the capſule to the fire-place quickly enough.

I ſhall now go on to conſider ſome of the characters of pure nickel in the ſtate of oxidation.

Characters of oxide of nickel. The nitric ſolution of pure nickel has a beautiful graſs-green colour. Carbonate of potaſh ſeparates from it a pale apple green precipitate. Precipitate very light. This precipitate well waſhed and dried is very light. A thouſand parts of metallic nickel reduced to this precipitate weigh 2,927 parts.

If this precipitate be expoſed to a white heat it becomes of a blackiſh gray, ſcarcely inclining to green, and weighing only 1,285. On continuing the fire, the maſs approaches the metallic ſtate more and more, and becomes magnetic. This is Oil promotes its reduction. effected much more ſpeedily if the oxide be moiſtened with a little oil.

Precipitate by ammonia. On adding cauſtic ammonia in exceſs to a nitric ſolution of nickel, a precipitate is formed, reſembling in colour ammoniure of copper, but not ſo deep. This colour ſometimes changes in a couple of hours to an amethyſt red, and to a violet, which colours are converted into an apple-green on the addition of an acid, and again to a blue and violet on the addition of ammonia. If however we add to the ſolution of nickel a ſolution of copper, ſo as to produce no perceptible change, the colour of the precipitate formed by ammonia ceaſes to aſſume a red tinge, and the red colour of the ammoniure of nickel diſappears on the addition of a little ammoniure of copper; whence it follows, that every precipitate of nickel by ammonia, which retains its blue colour, has copper combined with it.

Its being a noble metal queſtioned. * From this it is difficult to believe that nickel, under favourable circumſtances, would not become oxided by the combined influence of air and fire. VAN MONS.

On

III.

On the Analyſis of Soils, as connected with their Improvement.

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I. *Utility of Inveſtigation relating to the Analyſis of Soils.*

THE methods of improving lands are immediately connected with the knowledge of the chemical nature of ſoils, and experiments on their compoſition appear capable of many uſeful applications.

The importance of this ſubject has been already felt by ſome very able cultivators of ſcience; many uſeful facts and obſervations with regard to it have been furniſhed by Mr. Young; it has been examined by Lord Dundonald, in his treatiſe on the connexion of Chemiſtry with Agriculture, and by Mr. Kirwan in his excellent eſſay on Manures; but the enquiry is ſtill far from being exhauſted, and new methods of elucidating it are almoſt continually offered, in conſequence of the rapid progreſs of chemical diſcovery.

Analyſis of ſoils; attended to by Mr. Young, Lord Dundonald, and Mr. Kirwan.

In the following pages I ſhall have the honour of laying before the Board, an account of thoſe methods of analyſing ſoils which appear moſt preciſe and ſimple, and moſt likely to to be uſeful to the practical farmer; they are founded partly upon the labours of the gentlemen, whoſe names have been juſt mentioned, and partly upon ſome later improvements.

II. *Of the Subſtances found in Soils.*

The ſubſtances which are found in ſoils, are certain mixtures or combinations of ſome of the primitive earths, animal and vegetable matter in a decompoſing ſtate, certain ſaline compounds, and the oxide of iron. Theſe bodies always retain water, and exiſt in very different proportions in different lands; and the end of analytical experiments is the detection of their quantities and mode of union.

Soils contain earths, animal and vegetable remains, ſaline compounds, and oxide of iron.

The *earths* found in common ſoils are principally ſilex or the earth of flints, alumine or the pure matter of clay, lime, or calcareous earth, and magneſia.

Silex.

Silex, or the earth of flints, when perfectly pure, appears in the form of a white powder, which is incombuſtible, infuſible, inſoluble in water, and not acted upon by common acids; it is the ſubſtance which conſtitutes the principal part of rock chryſtal; it compoſes a conſiderable part of hard gravelly ſoils, of hard ſandy ſoils, and of hard ſtony lands.

Alumine. *Alumine*, or pure clay, in its perfect ſtate is white like ſilex; it adheres ſtrongly to the tongue, is incombuſtible, inſoluble in water, but ſoluble in acids, and in fixed alkaline menſtrua. It abounds moſt in clayey ſoils and clayey loams; but even in the ſmalleſt particles of theſe ſoils it is uſually united to ſilex and oxide of iron.

Lime. *Lime* is the ſubſtance well known in its pure ſtate under the name of quicklime. It always exiſts in ſoils in combination, and that principally with fixed air or carbonic acid, when it is called carbonate of lime; a ſubſtance which in the moſt compact form conſtitutes marble, and in its looſer form chalk. Lime, when combined with ſulphuric acid (oil of vitriol), produces ſulphate of lime (gypſum), and with phoſphoric acid, phoſphate of lime. The carbonate of lime, mixed with other ſubſtances, compoſes chalky ſoils and marles, and it is found in ſoft ſandy ſoils.

Magneſia. *Magneſia*, when pure, appears as white, and in a lighter powder, than any of the other earths; it is ſoluble in acid, but not in alkaline menſtrua; it is rarely found in ſoils; when it does exiſt, it is either in combination with carbonic acid, or with ſilex and alumine.

Animal decompoſing matter. *Animal decompoſing matter* exiſts in very different ſtates, according as the ſubſtances from which it is produced are different; it contains much carbonaceous ſubſtance, and may be principally reſolved by heat into this ſubſtance, volatile alkali, inflammable aeriform products, and carbonic acid; it is principally found in lands that have been lately manured.

Vegetable decompoſing matter. *Vegetable decompoſing matter* is likewiſe very various in kind, it contains uſually more carbonaceous ſubſtance than animal matter, and differs from it in the reſults of its decompoſition principally in not producing volatile alkali; it forms a great proportion of all peats; it abounds in rich mould, and is found in larger or ſmaller quantities in all lands.

Saline compounds. The *ſaline compounds* found in ſoils are very few, and in quantities ſo ſmall, that they are rarely to be diſcovered. They are principally muriate of ſoda (common ſalt), ſulphate of magneſia (Epſom ſalt), and muriate and ſulphate of potaſh, nitrate of lime, and the mild alkalies.

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Oxide of iron.

The *oxide of iron* is the ſame with the ruſt produced by expoſing iron to the air and water; it is found in all ſoils, but is moſt abundant in yellow and red clays, and in yellow and red ſiliceous ſands.

A more minute account of theſe different ſubſtances would be incompatible with the object of this paper. A full deſcription of their properties and agencies may be found in the elementary books on chemiſtry, and particularly in the Syſtem of Chemiſtry by Dr. Thomſon (2d Ed.); and in Henry's Epitome of Chemiſtry.

III. *Inſtruments required for the Analyſis of Soils.*

Inſtruments for analyſis. A balance, weights, ſieve, lamp, bottles, crucibles, baſons, p. and mortar, filters, knife, app. for gaſes.

The really important inſtruments required for the analyſis of ſoils are few, and but little expenſive. They are a balance capable of containing a quarter of a pound of common ſoil, and capable of turning when loaded, with a grain; a ſeries of weights from a quarter of a pound Troy to a grain; a wire ſieve, ſufficiently coarſe to admit a pepper corn through its apertures; an Argand lamp and ſtand; ſome glaſs bottles; Heſſian crucibles; porcelain, or queen's ware evaporating baſons; a Wedgewood peſtle and mortar; ſome filters made of half a ſheet of blotting paper, folded ſo as to contain a pint of liquid, and greaſed at the edges; a bone knife, and an apparatus for collecting and meaſuring aeriform fluids.

Re-agents. Mur. and ſulp. acids; vol. alk. pr. potaſh, ſoap lye, carb. amm. mur. amm. carb. pot. nitr. amm.

The chemical ſubſtances or reagents required for ſeparating the conſtituent parts of the ſoil, are muriatic acid (ſpirit of ſalt), ſulphuric acid, pure volatile alkali diſſolved in water, ſolution of pruſſiate of potaſh, ſoap lye, ſolution of carbonate of ammoniac, of muriate of ammonia, ſolution of neutral carbonate of potaſh, and nitrate of ammoniac. An account of the nature of theſe bodies, and their effects, may be found in the chemical works already noticed; and the reagents are ſold together with the inſtruments mentioned above, by Mr. Knight, Foſter Lane, Cheapſide, arranged in an appropriate cheſt.

IV. *Mode of collecting Soils for Analyſis.*

How ſamples of ſoils are to be collected.

In caſes when the general nature of the ſoil of a field is to be aſcertained, ſpecimens of it ſhould be taken from different places, two or three inches below the ſurface, and examined as to the ſimilarity of their properties. It ſometimes happens, that upon plains the whole of the upper ſtratum of the land is of the ſame kind, and in this caſe, one analyſis will be ſufficient; but in vallies, and near the beds of rivers, there are very great differences, and it now and then occurs that one part of a field is calcareous, and another part ſiliceous; and in this caſe, and in analogous caſes, the portions different from each other ſhould be ſeparately ſubmitted to experiment.

and preſerved if needful.

Soils when collected, if they cannot be immediately examined, ſhould be preſerved in phials quite filled with them, and cloſed with ground glaſs ſtoppers.

The quantity of ſoil moſt convenient for a perfect analyſis, is from two or four hundred grains. It ſhould be collected in dry weather, and expoſed to the atmoſphere till it becomes dry to the touch.

The ſpecific gravity

The ſpecific gravity of a ſoil, or the relation of its weight to that of water, may be aſcertained by introducing into a phial, which will contain a known quantity of water, equal volumes of water and of ſoil, and this may be eaſily done by pouring in water till it is half full, and then adding the ſoil till the fluid riſes to the mouth; the difference between the weight of the ſoil and that of the water, will give the reſult. Thus if the bottle contains four hundred grains of water, and gains two hundred grains when half filled with water and half with ſoil, the ſpecific gravity of the ſoil will be 2, that is, it will be twice as heavy as water, and if it gained one hundred and ſixty-five grains, its ſpecific gravity would be 1825, water being 1000.

is of importance to be known.

It is of importance, that the ſpecific gravity of a ſoil ſhould be known, as it affords an indication of the quantity of animal and vegetable matter it contains; theſe ſubſtances being always moſt abundant in the lighter ſoils.

Other phyſical properties.

The other phyſical properties of ſoils ſhould likewiſe be examined before the analyſis is made, as they denote, to a certain extent, their compoſition, and ſerve as guides in directing the experiments. Thus ſiliceous ſoils are generally rough to the touch, and ſcratch glaſs when rubbed upon it; aluminous ſoils adhere ſtrongly to the tongue, and emit a ſtrong earthy ſmell when breathed on; and calcareous ſoils are ſoft, and much leſs adheſive than aluminous ſoils.

V. Mode

V. *Mode of aſcertaining the Quantity of Water of Abſorption in Soils.*

Evaporation of the abſorbed water, to ſhew its quantity in ſoils.

Soils, though as dry as they can be made by continued expoſure to air, in all caſes ſtill contain a conſiderable quantity of water, which adheres with great obſtinacy to the earths and animal and vegetable matter, and can only be driven off from them by a conſiderable degree of heat. The firſt proceſs of analyſis is, to free the given weight of ſoil from as much of this water as poſſible, without in other reſpects, affecting its compoſition; and this may be done by heating it for ten or twelve minutes over an Argand's lamp, in a baſon of porcelain, to a temperature equal to 300 * Fahrenheit; and in caſe a thermometer is not uſed, the proper degree may be eaſily aſcertained, by keeping a piece of wood in contact with the bottom of the diſh; as long as the colour of the wood remains unaltered, the heat is not too high; but when the wood begins to be charred, the proceſs muſt be ſtopped. A ſmall quantity of water will perhaps remain in the ſoil even after this operation, but it always affords uſeful comparative reſults; and if a higher temperature were employed, the vegetable or animal matter would undergo decompoſition, and in conſequence the experiment be wholly unſatisfactory.

One eighth is an extreme proportion.

The loſs of weight in the proceſs ſhould be carefully noted, and when in four hundred grains of ſoil it reaches as high as 50, the ſoil may be conſidered as in the greateſt degree abſorbent, and retentive of water, and will generally be found to contain a large proportion of aluminous earth. When the loſs is only from 20 to 10, the land may be conſidered as only ſlightly abſorbent and retentive, and the ſilicious earth as moſt abundant.

VI. *Of the Separation of Stones, Gravel, and vegetable Fibres from Soils.*

Stones, &c. to be ſeparated after the drying, &c.

None of the looſe ſtones, gravel, or large vegetable fibres ſhould be divided from the pure ſoil till after the water is drawn off; for theſe bodies are themſelves often highly abſorbent and retentive, and in conſequence influence the fertility of the land.

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* In ſeveral experiments, in which this proceſs has been carried on by diſtillation, I have found the water that came over pure, and no ſenſible quantity of other volatile matter was produced.

The next proceſs, however, after that of heating, ſhould be their ſeparation, which may be eaſily accompliſhed by the ſieve, after the ſoil has been gently bruiſed in a mortar. The weights of the vegetable fibres or wood, and of the gravel and ſtones ſhould be ſeparately noted down, and the nature of the laſt aſcertained; if calcareous, they will effervеſce with acids; if ſiliceous, they will be ſufficiently hard to ſcratch glaſs; and if of the common aluminous claſs of ſtones, they will be ſoft, eaſily ſcratched with a knife, and incapable of efferveſcing with acids.

VII. *Separation of the Sand and Clay, or Loam, from each other.*

Sand, clay, and loam ſeparated from each other by elutriation.

The greater number of ſoils, beſides gravel and ſtones, contain larger or ſmaller proportions of ſand of different degrees of fineneſs; and it is a neceſſary operation, the next in the proceſs of analyſis, to detach them from the parts in a ſtate of more minute diviſion, ſuch as clay, loam, marle, and vegetable and animal matter. This may be effected in a way ſufficiently accurate, by agitation of the ſoil in water. In this caſe, the coarſe ſand will generally ſeparate in a minute, and the finer in two or three minutes, whilſt the minutely divided earthy, animal, or vegetable matter will remain in a ſtate of mechanical ſuſpenſion for a much longer time; ſo that by pouring the water from the bottom of the veſſel, after one, two, or three minutes, the ſand will be principally ſeparated from the other ſubſtances, which, with the water containing them, muſt be poured into a filter, and after the water has paſſed through, collected, dried and weighed. The ſand muſt likewiſe be weighed, and their reſpective quantities noted down. The water of lixiviation muſt be preſerved, as it will be found to contain the ſaline matter, and the ſoluble animal or vegetable matters, if any exiſt in the ſoil.

VIII. *Examination of the Sand.*

The ſand ſeparated into ſiliceous and calcareous.

By the proceſs of waſhing and filtration, the ſoil is ſeparated into two portions, the moſt important of which is generally the finely divided matter. A minute analyſis of the ſand is ſeldom or never neceſſary, and its nature may be detected in the ſame manner as that of the ſtones or gravel. It is always either ſilicious ſand, or calcareous ſand, or a mixture of both. If it conſiſts wholly of carbonate of lime, it will be rapidly ſoluble in muriatic acid, with effervefcence; but if it conſiſt partly of this ſubſtance, and partly of ſiliceous matter, the reſpective quantities may be aſcertained by weighing the reſiduum after the action of the acid, which muſt be applied till the mixture has acquired a ſour taſte, and has ceaſed to effervefce. This reſiduum is the ſilicious part: it muſt be waſhed, dried, and heated ſtrongly in a crucible; the difference between the weight of it and the weight of the whole, indicates the proportion of calcareous ſand.

IX. *Examination of the finely divided Matter of Soils, and Mode of detecting mild Lime and Magneſia.*

The finely divided matter of the ſoil is uſually very compound in its nature; it ſometimes contains all the four primitive earths of ſoils, as well as animal and vegetable matter; and to aſcertain the proportions of theſe with tolerable accuracy, is the moſt difficult part of the ſubject.

The finely divided matter treated.

The firſt proceſs to be performed, in this part of the analyſis, is the expoſure of the fine matter of the ſoil to the action of the muriatic acid. This ſubſtance ſhould be poured upon the earthy matter in an evaporating baſon, in a quantity equal to twice the weight of the earthy matter; but diluted with double its volume of water. The mixture ſhould be often ſtirred, and ſuffered to remain for an hour or an hour and a half before it is examined.

Muriatic acid takes up lime, magneſia, or iron.

If any carbonate of lime or of magneſia exiſt in the ſoil, they will have been diſſolved in this time by the acid, which ſometimes takes up likewiſe a little oxide of iron; but very ſeldom any alumine.

The fluid ſhould be paſſed through a filter; the ſolid matter collected, waſhed with rain water, dried at a moderate heat, and weighed. Its loſs will denote the quantity of ſolid matter taken up. The waſhings muſt be added to the ſolution, which if not ſour to the taſte, muſt be made ſo by the addition of freſh acid, when a little ſolution of common pruſſiate of potaſh muſt be mixed with the whole. If a blue precipitate occurs, it denotes the preſence of oxide of iron, and the ſolution of the pruſſiate muſt be dropped in till no farther effect is produced. To aſcertain its quantity, it muſt be collected in the ſame manner as other ſolid precipitates, and heated red; the reſult is oxide of iron.

Precip. of iron (if preſent) by pruſſ. alkali;

Into

and earth, by carbonate of potaſh.

Into the fluid freed from oxide of iron, a ſolution of neutralized carbonate of potaſh muſt be poured till all efferveſcence ceaſes in it, and till its taſte and ſmell indicate a conſiderable exceſs of alkaline ſalt.

The precipitate that falls down is carbonate of lime; it muſt be collected on the filter, and dried at a heat below that of redneſs.

The remaining fluid muſt be boiled for a quarter of an hour, when the magneſia, if any exiſt, will be precipitated from it, combined with carbonic acid, and its quantity is to be aſcertained in the ſame manner as that of the carbonate of lime.

Alumine if taken up.

If any minute proportion of alumine ſhould, from peculiar circumſtances, be diſſolved by the acid, it will be found in the precipitate with the carbonate of lime, and it may be ſeparated from it by boiling for a few minutes with ſoap lye, ſufficient to cover the ſolid matter. This ſubſtance diſſolves alumine, without acting upon carbonate of lime.

Carbonate of lime if in plenty, may be eſtimated by the quantity of carbonic acid.

Should the finely divided ſoil be ſufficiently calcareous to efferveſce very ſtrongly with acids, a very ſimple method may be adopted for aſcertaining the quantity of carbonate of lime, and one ſufficiently accurate in all common caſes.

Carbonate of lime, in all its ſtates, contains a determinate proportion of carbonic acid, *i. e.* about 45 per cent. ſo that when the quantity of this elaſtic fluid, given out by any ſoil during the ſolution of its calcareous matter in an acid is known, either in weight or meaſure, the quantity of carbonate of lime may be eaſily diſcovered.

When the proceſs by diminution of weight is employed, two parts of the acid and one part of the matter of the ſoil muſt be weighed in two ſeparate bottles, and very ſlowly mixed together till the efferveſcence ceaſes; the difference between their weight before and after the experiment, denotes the quantity of carbonic acid loſt; for every four grains and a half of which, ten grains of carbonate of lime muſt be eſtimated.

The beſt method of collecting the carbonic acid, ſo as to diſcover its volume, is by the pneumatic apparatus, the conſtruction and application of which is deſcribed at the end of this paper. The eſtimation is, for every ounce meaſure of carbonic acid, two grains of carbonate of lime.

X. *Mode of aſcertaining the Quantity of inſoluble finely divided Animal and Vegetable Matter.*

After the fine matter of the ſoil has been acted upon by muriatic acid, the next proceſs is to aſcertain the quantity of finely divided inſoluble animal and vegetable matter that it contains.

Ignition in an open veſſel deſtroys vegetable and animal matters.

This may be done with ſufficient preciſion, by heating it to ſtrong ignition in a crucible over a common fire till no blackneſs remains in the maſs. It ſhould be often ſtirred with a metallic wire, ſo as to expoſe new ſurfaces continually to the air; the loſs of weight that it undergoes denotes the quantity of the ſubſtance that it contains deſtructible by fire and air.

It is not poſſible to aſcertain whether this ſubſtance is wholly animal or vegetable matter, or a mixture of both. When the ſmell emitted during the incineration is ſimilar to that of burnt feathers, it is a certain indication of ſome animal matter; and a copious blue flame at the time of ignition, almoſt always denotes a conſiderable proportion of vegetable matter. In caſes when the experiment is needed to be very quickly performed, the deſtruction of the decompoſible ſubſtances may be aſſiſted by the agency of nitrate of ammoniac, which at the time of ignition may be thrown gradually upon the heated maſs in the quantity of twenty grains for every hundred of reſidual ſoil. It affords the principle neceſſary to the combuſtion of the animal and vegetable matter, which it cauſes to be converted into elaſtic fluids; and it is itſelf at the ſame time decompoſed and loſt.

The ſmell ſhews whether it be animal or vegetable.

XI. *Mode of ſeparating aluminous and ſilicious Matter and Oxide of Iron.*

The ſubſtances remaining after the decompoſition of the vegetable and animal matter, are generally minute particles of earthy matter, containing uſually alumine and ſilex with combined oxide of iron.

The reſidual ſilex, alumine and oxide of iron ſeparated.

To ſeparate theſe from each other, the ſolid matter ſhould be boiled for two or three hours with ſulphuric acid, diluted with four times its weight of water; the quantity of the acid ſhould be regulated by the quantity of ſolid reſiduum to be acted on, allowing for every hundred grains two drachms or one hundred and twenty grains of acid.

Dilute ſulphuric acid takes up the two firſt.

The ſubſtance remaining after the action of the acid, may be conſidered as ſilicious; and it muſt be ſeparated and its weight aſcertained, after waſhing and drying in the uſual manner.

Carbonate of ammonia throws down the alumine.

The alumine and the oxide of iron, if they exiſt, are both diſſolved by the ſulphuric acid; they may be ſeparated by carbonate of ammoniac, added to exceſs; it throws down the alumine, and leaves the oxide of iron in ſolution, and this ſubſtance may be ſeparated from the liquid by boiling.

Should any magneſia and lime have eſcaped ſolution in the muriatic acid, they will be found in the ſulphuric acid; this, however, is ſcarcely ever the caſe; but the proceſs for detecting them and aſcertaining their quantities, is the ſame in both inſtances.

More accurate proceſs.

The method of analyſis by ſulphuric acid, is ſufficiently preciſe for all uſual experiments; but if very great accuracy be an object, dry carbonate of potaſh muſt be employed as the agent, and the reſiduum of the incineration muſt be heated red for half an hour, with four times its weight of this ſubſtance, in a crucible of ſilver, or of well baked porcelain. The maſs obtained muſt be diſſolved in muriatic acid, and the ſolution evaporated till it is nearly ſolid; diſtilled water muſt then be added, by which the oxide of iron and all the earths, except ſilex, will be diſſolved in combination as muriates. The ſilex, after the uſual proceſs of lixiviation, muſt be heated red; the other ſubſtances may be ſeparated in the ſame manner as from the muriatic and ſulphuric ſolutions.

This proceſs is the one uſually employed by chemical philoſophers for the analyſis of ſtones.

XII. *Mode of diſcovering ſoluble Animal and Vegetable Matter, and Saline Matter.*

Matters ſoluble in water.

If any ſaline matter, or ſoluble vegetable or animal matter, is ſuſpected in the ſoil, it will be found in the water of lixiviation uſed for ſeparating the ſand.

This water muſt be evaporated to dryneſs in an appropriate diſh, at a heat below its boiling point.

If the ſolid matter obtained is of a brown colour and inflammable, it may be conſidered as partly vegetable extract. If its ſmell, when expoſed to heat, be ſtrong and fetid, it contains animal mucilaginous or gelatinous ſubſtance; if it be white and tranſparent, it may be conſidered as principally ſaline matter. Nitrate of potaſh (nitre) or nitrate of lime, is indicated in this ſaline matter, by its ſcintillating with a burning coal. Sulphate of magneſia may be detected by its bitter taſte; and ſulphate of potaſh produces no alteration in ſolution of carbonate of ammoniac, but precipitates ſolution of muriate of barytes.

XIII. *Mode of detecting Sulphate of Lime (Gypſum) and Phoſphate of Lime in Soils.*

Sulphate of Lime.

Should ſulphate of phoſphate of lime be ſuſpected in the entire ſoil, the detection of them requires a particular proceſs upon it. A given weight of it, for inſtance four hundred grains, muſt be heated red for half an hour in a crucible, mixed with one-third of powdered charcoal. The mixture muſt be boiled for a quarter of an hour, in a half pint of water, and the fluid collected through the filter, and expoſed for ſome days to the atmoſphere in an open veſſel. If any ſoluble quantity of ſulphate of lime (gypſum) exiſted in the ſoil, a white precipitate will gradually form in the fluid, and the weight of it will indicate the proportion.

Phoſphate of lime.

Phoſphate of lime, if any exiſt, may be ſeparated from the ſoil after the proceſs for gypſum. Muriatic acid muſt be digeſted upon the ſoil, in quantity more than ſufficient to ſaturate the ſoluble earths; the ſolution muſt be evaporated, and water poured upon the ſolid matter. This fluid will diſſolve the compounds of earths with the muriatic acid, and leave the phoſphate of lime untouched.

It would not fall within the limits aſſigned to this paper, to detail any proceſſes for the detection of ſubſtances which may be accidentally mixed with the matters of ſoils. Manganeſe is now and then found in them, and compounds of the barytic earth; but theſe bodies appear to bear little relation to fertility or barrenneſs, and the ſearch for them would make the analyſis much more complicated without rendering it more uſeful.

XIV. *Statement of Reſults and Products.*

Products ſtated.

When the examination of a ſoil is compleated, the products ſhould be claſſed, and their quantities added together, and if they nearly equal the original quantity of ſoil, the analyſis may be conſidered as accurate. It muſt, however, be noticed, that when phoſphate or ſulphate of lime are diſcovered by the independent proceſs XIII. a correction muſt be made for the general proceſs, by ſubtracting a ſum equal to their weight from the quantity of carbonate of lime, obtained by precipitation from the muriatic acid.

In arranging the products, the form ſhould be in the order of the experiments by which they were obtained.

Thus 400 grains of a good ſilicious ſandy ſoil may be ſuppoſed to contain

		Grains.
Of water of abſorption - - - -		18
Of looſe ſtones and gravel principally ſilicious -		42
Of undecompounded vegetable fibres - -		10
Of fine ſilicious ſand - - -		200
Of minutely divided matter ſeparated by filtration and conſiſting of		
Carbonate of lime - - -	25	
Carbonate of magneſia - - -	4	
Matter deſtructible by heat, principally vegetable - - - -	10	
Silex - - - - -	40	
Alumine - - - -	32	
Oxide of iron - - - - -	4	
Soluble matter, principally ſulphate of potaſh and vegetable extract - -	5	
Gypſum - - - -	3	
Phoſphate of lime - - -	2	
		125
Amount of all the products		395
Loſs - - -		5

In this inſtance the loſs is ſuppoſed ſmall; but in general, in actual experiments, it will be found much greater, in conſequence of the difficulty of collecting the whole quantities of the different precipitates; and when it is within thirty for four hundred grains, there is no reaſon to ſuſpect any want of due preciſion in the proceſſes.

XV. *This general Method of Analyſis may in many Caſes be much ſimplified.*

Simplification, &c. of the analyſis.

When the experimenter is become acquainted with the uſe of the different inſtruments, the properties of the reagents, and the relations between the external and chemical qualities of ſoils, he will ſeldom find it neceſſary to perform, in any one caſe, all the proceſſes that have been deſcribed. When his ſoil, for inſtance, contains no notable proportion of calcareous matter, the action of the muriatic acid IX. may be omitted. In examining peat ſoils, he will principally have to attend to the operation by fire and air X.; and in the analyſis of chalks and loams, he will often be able to omit the experiment by ſulphuric acid XI.

In the firſt trials that are made by perſons unacquainted with chemiſtry, they muſt not expect much preciſion of reſult. Many difficulties will be met with; but in overcoming them, the moſt uſeful kind of practical knowledge will be obtained; and nothing is ſo inſtructive in experimental ſcience, as the detection of miſtakes. The correct analyſt ought to be well grounded in chemical information; but perhaps there is no better mode of gaining it, than that of attempting original inveſtigations. In purſuing his experiments, he will be continually obliged to learn from books, the hiſtory of the ſubſtances he is employing or acting upon; and his theoretical ideas will be more valuable in being connected with practical operation, and acquired for the purpoſe of diſcovery.

XVI. *On the Improvement of Soils, as connected with the Principle of their Compoſition.*

Improvement of lands from the known compoſition of fertile or ſterile ſoils.

In caſes when a barren ſoil is examined with a view to its improvement, it ought in all caſes, if poſſible, to be compared with an extremely fertile ſoil in the ſame neighbourhood, and in a ſimilar ſituation: the difference given by their analyſes would indicate the methods of cultivation; and thus the plan of improvement would be founded upon accurate ſcientific principles.

If the fertile ſoil contained a large quantity of ſand, in proportion to the barren ſoil, the proceſs of amelioration would depend ſimply upon a ſupply of this ſubſtance; and the method would be equally ſimple with regard to ſoils deficient in clay or calcareous matter.

In the application of clay, ſand, loam, marle, or chalk to lands, there are no particular chemical principles to be obſerved; but when quick lime is uſed, great care muſt be taken that it is not obtained from the magneſian limeſtone; for in this caſe, as has been ſhewn by Mr. Tennant, it is exceedingly injurious to land *. The magneſian limeſtone may be diſtinguiſhed from the common limeſtone by its greater hardneſs, and by the length of time that it requires for its ſolution in acids, and it may be analyſed by the proceſs for carbonate of lime and magneſia IX.

When the analytical compariſon indicates an exceſs of vegetable matter, as the cauſe of ſterility, it may be deſtroyed by much pulverization and expoſure to air, by paring and burning, or the agency of lately made quicklime. And the defect of animal and vegetable matter muſt be ſupplied by animal or vegetable manure.

XVII. *Sterile Soils in different Climates and Situations muſt differ in Compoſition.*

Different climates and local circumſtances require different compounds for fertile ſoils.

The general indications of fertility and barrenneſs, as found by chemical experiments, muſt neceſſarily differ in different climates, and under different circumſtances. The power of ſoils to abſorb moiſture, a principle eſſential to their productiveneſs, ought to be much greater in warm and dry countries, than in cold and moiſt ones; and the quantity of fine aluminous earth they contain larger. Soils likewiſe that are ſituated on declivities, ought to be more abſorbent than thoſe in the ſame climate on plains or in valleys †. The productiveneſs of ſoils muſt likewiſe be influenced by the nature of the ſubſoil, or the earthy or ſtony ſtrata on which they reſt; and this circumſtance ought to be particularly attended to, in conſidering their chemical nature, and the ſyſtem of improvement. Thus a ſandy ſoil may ſometimes owe its fertility to the power of the ſubſoil to retain water; and an abſorbent clayey ſoil may occaſionally be prevented from being barren, in a moiſt climate, by the influence of a ſubſtratum of ſand or gravel.

XVIII. *Of the chemical Compoſition of fertile Corn Soils in the Climate.*

Actual compoſition of ſome fertile ſoils.

Thoſe ſoils that are moſt productive of corn, contain always certain proportions of aluminous and calcareous earth in a finely divided ſtate, and a certain quantity of vegetable or animal matter.

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* Phil. Tranſactions for 1799, p. 305. This limeſtone is found abundantly in Yorkſhire, Derbyſhire, and Somerſetſhire.

† Kirwan. Tranſ. Iriſh Academy, Vol. V. p. 175.

The quantity of calcareous earth is however very various, and in ſome caſes exceedingly ſmall. A very fertile corn ſoil from Ormiſton in Eaſt Lothian afforded me in an hundred parts, only eleven parts of mild calcareous earth; it contained twenty-five parts of ſilicious ſand; the finely divided clay amounted to forty-five parts. It loſt nine in decompoſed animal and vegetable matter, and four in water, and afforded indications of a ſmall quantity of phoſphate of lime.

This ſoil was of a very fine texture, and contained very few ſtones or vegetable fibres. It is not unlikely that its fertility was in ſome meaſure connected with the phoſphate; for this ſubſtance is found in wheat, oats, and barley, and may be a part of their food.

A ſoil from the low lands of Somerſetſhire, celebrated for producing excellent crops of wheat and beans without manure, I found to conſiſt of one-ninth of ſand, chiefly ſilicious, and eight-ninths of calcareous marle tinged with iron, and containing about five parts in the hundred of vegetable matter. I could not detect in it any phoſphate or ſulphate of lime, ſo that its fertility muſt have depended principally upon its power of attracting principles of vegetable nouriſhment from water and the atmoſphere *.

Mr. Tillet, in ſome experiments made on the compoſition of ſoils at Paris, found that a ſoil compoſed of three-eighths of clay, two-eighths of river ſand, and three-eighths of the parings of limeſtone, was very proper for wheat.

XIX. *Of the Compoſition of Soils proper for bulbous Roots and for Trees.*

Soils proper for bulbous roots and trees.

In general, bulbous roots require a ſoil much more ſandy, and leſs abſorbent than the graſſes. A very good potatoe ſoil, from Varſel in Cornwall, afforded me ſeven-eighths of ſilicious ſand; and its abſorbent power was ſo ſmall, that one hundred parts loſt only two by drying at 400 Fahrenheit.

Plants and trees, the roots of which are fibrous and hard, and capable of penetrating deep into the earth, will vegetate to advantage in almoſt all common ſoils, which are moderately dry, and which do not contain a very great exceſs of vegetable matter.

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* This ſoil was ſent to me by T. Poole, Eſq. of Nether Stowey. It is near the opening of the river Parret into the Britiſh Channel; but, I am told, is never overflowed.

I found the ſoil taken from a field at Sheffield-place in Suſſex, remarkable for producing flouriſhing oaks, to conſiſt of ſix parts of ſand, and one part of clay and finely divided matter. And one hundred parts of the entire ſoil ſubmitted to analyſis, produced

	Parts.
Water - - - - - -	3
Silex - - - - - -	54
Alumine - - - - - -	28
Carbonate of lime - - -	3
Oxide of iron - - - -	5
Decompoſing vegetable matter -	4
Loſs - - - - - - -	3

XX. *Advantages of Improvements made by changing the Compoſition of the earthy Parts of Soils.*

Soils rendered fertile by changing the compoſition of the earthy parts, are more permanent than manured ſoils.

From the great difference of the cauſes that influence the productiveneſs of lands, it is obvious that in the preſent ſtate of ſcience, no certain ſyſtem can be deviſed from their improvement, independent of experiment; but there are few caſes in which the labour of analytical trials will not be amply repaid by the certainty with which they denote the beſt methods of amelioration; and this will particularly happen, when the defect of compoſition is found in the proportions of the primitive earths.

In ſupplying animal or vegetable manure, a temporary food only is provided for plants, which is in all caſes exhauſted by means of a certain number of crops; but when a ſoil is rendered of the beſt poſſible conſtitution and texture, with regard to its earthy parts, its fertility may be conſidered as permanently eſtabliſhed. It becomes capable of attracting a very large portion of vegetable nouriſhment from the atmoſphere, and of producing its crops with comparatively little labour and expence.

Deſcription of the Apparatus for the Analyſis of Soils.

Apparatus for experiments.

A. Retort.

B. B. Funnels for the purpoſe of filtrating.

D. Balance.

E. Argand's lamp.

F, G, H, K. The different parts of the apparatus required for meaſuring the quantity of elaſtic fluid given out during the action of an acid on calcareous ſoils. F. Repreſents the bottle for containing the ſoil. K. The bottle containing the acid furniſhed with a ſtopcock. G. The tube connected with a flaccid bladder. I. The graduated meaſure. H. The bottle for containing the bladder. When this inſtrument is uſed, a given quantity of ſoil is introduced into F; K is filled with muriatic acid diluted with an equal quantity of water; and the ſtop-cock being cloſed is connected with the upper orifice of F, which is ground to receive it. The tube G is introduced into the lower orifice of F, and the bladder connected with it placed in its flaccid ſtate into H, which is filled with water. The graduated meaſure is placed under the tube of H. When the ſtop-cock of K is turned, the acid flows into F, and acts upon the ſoil; the elaſtic fluid generated paſſes through G into the bladder, and diſplaces a quantity of water in H equal to it in bulk, and this water flows through the tube into the graduated meaſure; the water in which gives by its volume the indication of the proportion of carbonic acid diſengaged from the ſoil; for every ounce meaſure of which two grains of carbonate of lime may be eſtimated.

L. Repreſents the ſtand for the lamp.

M, N, O, P, Q, R, S. Repreſent the bottles containing the different reagents.

IV.

Diſcovery of a new Vegetable Subſtance, by Mr. Rose [*].

A CONCENTRATED decoction of the root of elecampane, *inula helenium*, after ſtanding ſome hours, depoſits a white powder, appearing at firſt ſight much like ſtarch, but differing from it both in its principles and in its manner of comporting itſelf with other ſubſtances. Decoction of elecampane root depoſits a powder reſembling ſtarch.

1. This ſubſtance is generally inſoluble in cold water. Being triturated with it a white milky liquor is formed, which ſoon depoſits a heavy white powder, and leaves the ſupernatant water clear and limpid. It is inſoluble in cold water.

2. It diſſolves very well in boiling water. On heating to ebullition one part of the white powder, with four parts of water, Soluble in boiling water.

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[*] From Gehlen's Journal for 1804, Vol. III. p. 217.

But much ſubſides on cooling.

water, a complete ſolution is obtained, which paſſes through filtering paper while hot, but on cooling acquires a mucilaginous conſiſtence and a dull colour. In the courſe of ſome hours this ſolution depoſits the greater part of the ſubſtance diſſolved in the form of a compact white powder.

Differs from ſolution of gum-arabic.

A ſolution of one part of gum arabic, in four parts of water is much thicker, of a more tenacious conſiſtence, and froths lightly, which the ſolution of the powder from the elecampane root does not.

Alcohol ſeparates it from water,

does not gum-arabic.

3. On mixing the ſolution of the white powder with an equal quantity of alcohol, the mixture is at firſt clear, but in a little time the powder ſeparates in the form of a tumid white ſediment, leaving the fluid above it tranſparent. A ſolution of gum-arabic on the addition of alcohol becomes immediately milky, and long retains this appearance, no kind of powder ſeparating even in ſeveral days.

Melts, emits a thick ſmoke, and leaves little reſiduum. Thus differs from ſtarch,

and from gum.

4. When thrown on burning coals, the white powder melts like ſugar and evaporates, diffuſing a white, thick, pungent ſmoke, with a ſmell of burnt ſugar. After this combuſtion a light reſiduum only remains, which runs into the coal. Starch emits a ſimilar ſmoke, but does not melt, and leaves a coally reſiduum much greater in quantity. Gum-arabic under the ſame circumſtances gives out ſcarcely any ſmoke.

On red hot iron burns.

Starch.

Gum.

Heated in an iron ſpoon over charcoal the powder firſt melts, and emits the ſmoke above deſcribed. As ſoon as the ſpoon becomes red hot, it burns with a vivid light flame, and leaves a very trifling coally reſiduum. Starch under the ſame circumſtances does not melt, is much longer before it burns, and leaves a conſiderable reſiduum of coally matter. Gum-arabic only ſparkles, does not take fire, and leaves a great deal of coal, which is readily convertible into grayiſh aſhes.

Dry diſtillation produces an acid, but no oil.

5. By dry diſtillation we obtain from this powder of the elecampane root a brown empyreumatic acid, having the ſmell of pyroxalic acid, but not an atom of empyreumatic oil.

Nitric acid produces malic, oxalic, and in exceſs acetic. Gum the ſaccholactic acid. Starch fat.

6. The nitric acid transforms the powder only into malic acid and oxalic acid, and when uſed in great exceſs into acetic acid: but we do not obtain an atom of the ſaccholactic acid, which gum-arabic treated in the ſame manner furniſhes ſo abundantly; or of the fatty matter which is generated by the action of nitric acid on ſtarch.

From all theſe phenomena it follows, that this farinaceous powder extracted from elecampane root, is neither ſtarch nor gum, but a peculiar vegetable ſubſtance holding a middle rank between the two. It is probable, that it exiſts in many other vegetables, and that ſeveral products hitherto conſidered as ſtarch are of the ſame nature as this farina.

Hence of a nature between gum and ſtarch, and probably exiſts in other vegetables.

V.

New Galvanic Diſcoveries by Mr. RITTER, *extracted from a Letter from Mr.* CHRIST. BERNOULLI [*].

I HERE tranſmit you the information you requeſted reſpecting the late experiments of Mr. Ritter, to which I ſubjoin ſome account of that gentleman.

1. *Charging of a Louis d'Or by the Pile.*

The pile with which Mr. Ritter uſually makes his experiments conſiſts of a hundred pairs of metallic plates, two inches in diameter. The pieces of zinc have a rim to prevent the liquid preſſed out from flowing away. The apparatus is always inſulated by ſeveral plates of glaſs.

Mr. Ritter's common pile.

As Mr. Ritter at preſent reſides in a village near Jene, I have not been able to ſee his experiments with his grand battery of two thouſand pieces, or with his battery of fifty pieces, each thirty-ſix inches ſquare, the action of which continues very perceptible for a fortnight. Neither have I ſeen his experiments with the new battery of his invention, conſiſting of a ſingle metal, and which he calls *the charging pile.*

His grand battery, large battery, and charging pile.

I have frequently however, ſeen him galvaniſe louis d'or lent him by perſons preſent. To effect this, he places the louis between two pieces of paſteboard thoroughly wetted, and keeps it ſix or eight minutes in the chain of circulation connected with the pile. Thus the louis becomes charged, without being immediately in contact with the conducting wires. If this louis be applied afterward to the crural nerves of a frog recently prepared, the uſual contractions will be

Louis d'or charged by being kept in the galvanic circuit, excites contractions, and may thus be diſtinguiſhed among others,

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[*] Tranſlated from the *Journal de Chimie and de Phyſique* of Van Mons, No. 17, p. 133, March, 1805.

as it does not loſe its charge for ſome minutes.

excited. I had put a louis thus galvaniſed into my pocket, and Mr. Ritter ſaid to me a few minutes after, that I might find out this louis from among the reſt, by trying them in ſucceſſion upon the frog. Accordingly I made the trial, and in reality diſtinguiſhed among ſeveral others a ſingle one, in which the exciting quality was very evident. This charge is retained in proportion to the time that the piece has remained in the circuit of the pile. Of three different louis which Mr. Ritter charged in my preſence, neither loſt its charge in leſs than five minutes. All theſe experiments ſucceeded completely, and nothing ſeemed ſo eaſy as to repeat them.

This ſhows the affinity of the galvanic with the magnetic fluid, between which and the electric, it holds a middle place.

This retention of the galvanic charge by a metal in contact with the hand, and with other metals, ſhews this communication of the galvanic virtue to have more affinity with magnetiſm than with electricity, and aſſigns to the galvanic fluid an intermediate rank between the other two.

Several pieces may be charged at once.

Ritterian pile.

Metals thus charged acquire polarity.

In the manner which I have juſt deſcribed, Mr. Ritter can charge at once as many pieces as he wiſhes. It is ſufficient if the two extreme pieces of the number communicate with the pile through the intervention of wet paſteboards. It is with metallic diſcs charged in this manner, and placed upon one another with pieces of wet paſteboard alternately interpoſed, that Mr. Ritter conſtructs his charging pile, which ought in remembrance of its inventor to be called the *Ritterian pile*. The conſtruction of this pile ſhows, that each metal galvaniſed in this way acquires polarity, as the needle does when touched with a magnet. Though I have had no opportunity of ſeeing this new pile, I have convinced myſelf of the reality of the phenomenon by an experiment of the higheſt importance to ſcience, and for the invention of which we are equally indebted to the ſame ingenious philoſopher.

2. *Different Excitability of the Parts of Animals.*

Different excitability of the parts of animals.

During the courſe of ſeveral years in which Mr. Ritter has been employed in galvanic purſuits, and during which he has made many thouſands of experiments on the excitation produced in the frog by the contact of two different metals, for Mr. Ritter has not entirely abandoned the original mode of galvaniſing, like moſt other experimentaliſts, who employ Volta's pile excluſively; he had perceived not only a very ſtriking difference in the excitability of the different parts of animals, but alſo a difference of excitement between the extenſor and flexor muſcles, according as the poſitive or negative pole was applied to them, or as they were acted upon the inſtant after the metals were brought into contact or ſeparated from each other.

When the excitability is at its higheſt point of energy, as in very young frogs the moment after they are prepared, or in adult frogs during the coupling ſeaſon, the flexors alone contract, and in particular the flexor muſcles of that thigh to which the ſilver or negative metal is applied, contract at the inſtant when the metals come into contact, while thoſe of the thigh to which the zinc or poſitive metal is applied, contract at the inſtant of their ſeparation.

When the excitability of the animal is greateſt, the flexors contract by poſitive galvaniſm:

The oppoſite effects are obſervable in frogs, the excitability of which is on the point of being extinguiſhed, (Ritter's fifth degree.) In this caſe the extenſors only contract, and the flexors remain abſolutely motionleſs. At the moment of contact of the metals the muſcles on the zinc ſide alone are thrown into action, and at the moment of ſeparation thoſe on the ſilver ſide.

when it is loweſt the extenſors contract by negative.

Mr. Ritter diſtinguiſhes three degrees of mean excitability. At the ſecond degree (the firſt of the three mean degrees,) when the metals are brought into contact, a ſtrong excitement of the flexors is diſplayed on the ſilver ſide, and a weak excitement of the extenſors on the zinc ſide; and when the metals are ſeparated a ſtrong excitement of the flexors is ſeen on the zinc ſide, and a weak excitement of the extenſors on the ſilver ſide.

When the eccitability is between the medium and either extreme, the effect on the flexors and extenſors ſimultaneous but unequal.

At the fourth degree of excitability the contrary takes place. At the third or middle degree the excitability appears to be equally diſtributed, the contractions on each ſide appear equal, and at the moment of contact the flexors contract on the ſilver ſide, the extenſors on the zinc ſide; while at the moment of ſeparation the extenſors contract on the ſilver ſide, and the flexors on the zinc ſide.

At the medium, equal as well as ſimultaneous.

Mr. Ritter ſhowed me all theſe phenomena, and it was very eaſy to diſtinguiſh the different contractions. I have not yet had time to repeat theſe experiments, but I am afraid, eaſy as they appeared to be, they will require an experienced hand, to produce ſuch diſtinct effects as I ſaw. None of the experiments which Mr. Ritter performed before me ſucceeded with him

The experiments do not always ſucceed on the firſt trial.

Mr. Ritter's merit not ſufficiently appreciated,

him the firſt time. Moſt of theſe experiments have never yet been made public, and few philoſophers have juſtly appreciated the value of thoſe which have been given to the world. There are ſome people, who, habituated ſolely to the ſtriking effects of groſſer phyſics, ſuppoſe it impoſſible for a young philoſopher to ſee any thing more than themſelves in the delicate phenomena of a more refined order of phyſical experiments. What has greatly contributed to prevent Mr. Ritter from attaining the high reputation he deſerves is his ſtyle, which, by endeavouring to give it preeiſion, he has rendered obſcure; but in converſation it is quite otherwiſe, as here he combines the ſtricteſt logic with the greateſt ſimplicity of expreſſion.

partly owing to his ſtyle.

Account of Mr. Ritter.

Mr. Ritter is one of thoſe men, who owe every thing to the inſpiration of genius, nothing to education. He was intended for a mechanical occupation, when the diſcoveries of galvani excited in him that innate taſte for the phyſical ſciences, which has carried him over every obſtacle, and raiſed him to rank among the firſt natural philoſophers. Deſtitute of every ſource for procuring himſelf the apparatus indiſpenſable to ordinary phyſics, but ſwayed by the enthuſiaſm of inquiry, he greedily ſeized the opportunity of obeying this impulſe by purſuing a ſeries of experiments, that require only a ſimple and not a very expenſive apparatus. Europe has rung with the ſucceſs he has obtained within the ſeven years he has given to his reſearches. He muſt have written much to procure himſelf a large pile, and the moſt neceſſary books of natural philoſophy.

Not leſs indefatigable as an experimenter than ingenious as a theoriſt, he has committed to writing thouſands of experiments, which his time divided between galvanic experiments, application to other branches of phyſics, and the ſtudy of languages, has not yet allowed him to put in order for publication. But this ſtate of conſtraint is about to be at an end. The elector Bavaria, that enlightened prince, whoſe philoſophical beneficence attracts to his dominions the moſt diſtinguiſhed men of ſcience and learning throughout Europe, has juſt appointed Mr. Ritter a Member of the Academy of Munich, with a ſalary of about 200l. a year.

He is compoſing a ſyſtematic work on galvaniſm.

Mr. Ritter has been employed theſe ſix months in compoſing a ſyſtematic work on galvaniſm, but he does not think he ſhall be able to finiſh it in leſs than two or three years. When I left him he was going to publiſh *Tables of Galvanic Affinity*, including all the ſubſtances on which he has made experiments. Theſe tables will be of as much importance to galvaniſm as thoſe of Bergman were to chemiſtry: they will ſhow, though not yet in a complete manner, the order in which ſubſtances follow each other with reſpect to exciting or receiving the galvanic action.

Publiſhing tables of galvanic affinity.

But to return to the experiments reſpecting the charging of metals. Mr. Ritter, after having ſhown me his experiments on the different contractibility of various muſcles, made me obſerve, that the piece of gold galvaniſed by communication exerts at once the action of two metals, or of one conſtituent part of the pile; and that the half which was next the negative pole while in the circle became poſitive, and the half toward the poſitive pole became negative. I was completely convinced of the reality of theſe different phenomena, ſo important to phyſic in general, and to phyſiology in particular.

The galvaniſed piece of metal has two poles.

Mr. Ritter having diſcovered the method of galvaniſing metals, as iron is rendered magnetic, and having obſerved that galvaniſed metals always exhibit two poles, as the magnetic needle does, had the curioſity to obſerve the effect of golden needles charged with galvaniſm and balanced on a pivot. To his ſurpriſe he perceived, that theſe needles had a certain dip and variation, and that the angle of variation, the quantity of which I am ſorry I cannot recollect, was uniformly the ſame in all his experiments. It differs however from that of the magnetic needle, and the poſitive pole always dips.

Golden needles galvaniſed and ſuſpended, have both dip and variation, but different from the magnetic.

VI.

Improvement in applying the Points in Electrical Machines. By Mr. G. J. Singer.

To Mr. NICHOLSON,

SIR, *Princes Street, Sep. 19th, 1805.*

IN the ordinary conſtruction of electrical machines, the collecting points are fixed, and by the leaſt accidental motion are liable to ſcratch the glaſs, to obviate this inconvenience, I place my points in a cylindrical wire, terminated by ſmooth wooden balls, whoſe ſemidiameter is leſs than the length of the points: This wire is moveable on its axis, by means of a ſpring ſocket annexed to the ſtem which enters the conductor: The points may of courſe be placed at any required elevation, and the greateſt intenſity any variation in their ſituation produces, be obtained. When the points are elevated a little above the horizontal line, the danger of ſcratching the glaſs is effectually prevented, by the balls coming in contact while the points are kept at a ſmall diſtance. The ſecurity this application produces, and the additional intenſity it affords, have induced me to trouble you with this communication.

I am,
Dear Sir,
Your's, &c.
G. J. SINGER.

VII.

Queſtion whether Light as a Body may not have its Temperature raiſed or lowered, and produce the Effects aſcribed to reflected Heat. By J. P.

To Mr. NICHOLSON.

SIR,

Queſtion reſpecting light.

POSSESSING no differential thermometer, nor any time to employ it, I cannot prove whether my opinion is well founded or not, reſpecting the ingenious experiments of Mr. Leſlie or of M. Pictet, by which the reflection of inviſible (not radiant) heat, and even of cold, appears to have been proved.

Inſtead of there being an actual reflection of heat as a ſubſtance, or of cold as a ſubſtance, is it not in all theſe caſes a reflection of heated or of cooled light? In the experiments with the heated canniſter, the light of the room is, I doubt not, heated by the canniſter; and if collected in a focus, muſt produce an effect on the thermometer, anſwerable to the increaſed quantity of heat with which it is impregnated. Thus alſo in Monſ. Pictet's experiment, the light intercepted by the mirror and thence reflected, has been deprived of a portion of its caloric, or in other words cooled, by the ice; at the focal point therefore will be a collection of cooled rays of light, which muſt neceſſarily occaſion an effect on the thermometer, the reverſe of that of the former experiment. That light is a body capable of being united with caloric, and that heated or cooled light ſhould thus be reflected and occaſion all the phenomena of Mr. Leſlie's and of M. Pictet's experiments, appears to be much more probable, than that this calorific and frigorific fluid ſhould be the ambient air, or that cold, as a body, ſhould be reflected from mirrors in ſuch a manner as light is perhaps only capable of being reflected. Were the experiments ſo made that no light ſhould be in the room, and only a ſmall confined portion of light uſed to examine the thermometer, theſe conjectures would be put to the trial, and I truſt the myſtery would be removed.

Sir, your's,

J. P.

VIII.

Experiments on a Mineral formerly called falſe Tungſtein, now Cerite, in which a new Metal has been found.[*]

Klaproth ſuppoſed he had diſcovered a new earth in the falſe tungſtein; Hiſinger and Berzelius a new metal.

MR. Klaproth, about eight months ago, ſays Mr. Vauquelin, ſent me word, that he had diſcovered in the tungſtein of Baſtnas a new earth, to which he gave the name of ochroit, on account of the red colour it acquired by calcination. Meſſrs. Hiſinger and Berzelius, hearing this, wrote to Mr. V. claiming the priority of diſcovery, but affirming at the ſame time, that what they had found was a new metal. Theſe gentlemen afterward ſent Mr. V. ſpecimens of the mineral, which he analyſed in company with two experienced practical chemiſts, Meſſrs. Taſſaert and Bergman. The following were the reſults of their analyſis:

Characters of pure cerite.

The pure cerite[†] is ſemitranſparent, with a ſlight roſy tinge, or of a light or deep fleſh-colour[*]. It is ſufficiently hard to ſcratch glaſs[†], ſtrikes fire with difficulty, and its ſpecific gravity is 4·530. It has no determinate cryſtalline figure. Its fracture is compact[‡], and a little ſhining. Its powder is of a greyiſh colour; it becomes yellow by calcination, and loſes twelve per cent[§].

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[*] Abridged from a paper by Vauquelin in the *Annales de Chimie*, Vol. LIV. p. 28, and another by Meſſrs. Hiſinger and Berzelius in van Mons's *Journal de Chimie*, Vol. VI. p. 142.—C.

[†] Meſſrs. H. and B. have given to the metal the name of *cerion* or *cerium*, from the new planet Ceres, and to the mineral in which they diſcovered it that of *cerite*.

Treated with nitro-muriatic acid.

Exp. 1. Two hundred parts of this mineral treated with nitro-muriatic acid three times ſucceſſively, gave abundance of nitrous acid and oxigenated muriatic acid gas. The firſt and ſecond ſolutions being diluted with water were of a gold colour; the third was colourleſs. The former two being mixed depoſited ſpontaneouſly in time a ſmall quantity of white ſediment. The reſiduum left by the nitro-muriatic acid was of a gray colour with a ſlight roſeate tinge, and weighed 62, ſo that 138 parts were diſſolved.

The ſolution precipitated

Exp. 2. The ſolutions being evaporated to the conſiſtence of ſyrup to volatiliſe, the ſuperfluous acid remained clear to the end of the operation. Their reſiduum, diluted with water, afforded a milky liquor, with a ſlight roſy tint, and a very aſtringent taſte.

by pruſſiate of potaſh and ammonia.

Pruſſiate of potaſh produced in it a greeniſh blue precipitate: the colour of which was changed to a brown by a ſmall quantity of ammonia.

All the liquor into which a ſmall quantity of ammonia had been put to precipitate the iron alone was poured into a filter, but would not paſs through. It was heated therefore, and filtered, when it appeared of a gold colour, and had a very ſaccharine taſte. Pruſſiate of potaſh and oxalate of ammonia threw down from it perfectly white precipitates.

The matter left on the filter continued for a long time to impart a yellow tinge to the water with which it was waſhed. It was of a red colour, and appeared like oxide of iron at a maximum of oxidation.

Examined by different reagents.

The ſolution thus deprived of the red matter by ammonia, was examined by various reagents. Pruſſiate of potaſh gave with it a white, flocculent, gelatinous precipitate. Infuſion of galls, a brown, flocculent ſediment, unaffected by muriatic acid. Carbonate of potaſh, a very copious white gelatinous precipitate. Cauſtic potaſh, the ſame: and an exceſs of this reagent produced no change. Oxalate of ammonia, a very copious, white, flocculent precipitate, inſoluble in an exceſs of oxalic acid. Sulphuric acid, a yellow cryſtalline precipitate ſoluble in water. Muriate of tin whitened the ſolution without forming any precipitate.

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[*] Opake, and ſometimes but very rarely, yellowiſh. Meſſrs. H. and B.

[†] Does not ſcratch glaſs. H. and B.

[‡] Unequal and angular. H. and B.

[§] Six or ſeven. H. and B.

Attempts to free it from iron.

Exp. 3. After this the ſolution was evaporated, when it inſtantly became turbid, and formed an abundant fleſh-coloured depoſit. This was treated with acidulous oxalate of potaſh to diſſolve the iron without ſucceſs: the addition of nitric acid was as unſucceſsful: but muriatic acid added to the preceding diſſolved the precipitate with efferveſcence and the emiſſion of oxigenated muriatic acid gas. A white cryſtalline ſubſtance however, remained, conſiſting of oxide of cerium with oxalic acid. The greater part of the exceſs of acid in the ſolution being ſaturated with ammonia, oxalate of ammonia was added till no more precipitate was formed. This precipitate had all the properties of oxalate of cerium. Ammonia threw down from the filtered liquor oxide of iron.

Muriate precipitated by oxalate of ammonia.

Exp. 4. The matter precipitated from the ſolution of cerium by ammonia in *Exp.* 2, diſſolved with efferveſcence in muriatic acid. Oxalate of ammonia threw down from this ſolution oxide of cerium, and the filtered liquor contained oxide of iron tolerably pure.

Nitrate precipitated by oxalate of ammonia.

Exp. 5. The liquor freed from the greater part of the iron by ammonia and heat, which had notwithſtanding a ſlight roſeate tinge, was precipitated by oxalate of ammonia. The precipitate at the moment of its formation had the appearance of muriate of ſilver, but ſoon became granulous and ſubſided in this form. The liquor paſſed through the filter colourleſs, and the roſy tinge remained in the oxalate.

Oxalate of lime ſeparated.

Exp. 6. As the liquor from which the oxalate of cerium was precipitated contained an exceſs of acid, it might be preſumed to hold in ſolution moſt of the oxalate of lime formed at the ſame time, if the cerite contained any. Accordingly it was mixed with the water that had waſhed the precipitate and concentrated by evaporation, when on the addition of ammonia a ſmall quantity of oxalate of lime was thrown down.

Reſt of the lime ſeparated.

Exp. 7. As notwithſtanding ſome oxalate of lime might have been precipitated with the oxalate of cerium, a portion of the red oxide of cerium ariſing from the decompoſition of the oxalate by calcination was diſſolved in muriatic acid. A briſk effervefcence inſtantly took place, with the evolution of oxigenated muriatic gas, which continued till the whole was diſſolved, and differed in no reſpect from that prepared with oxide of mangancſe.

Muriatic ſolution of cerium rendered ſolid by ammonia.

The ſolution of cerium in muriatic acid was clear, and had only a light roſy tinge. To ſeparate it from the lime, if there were any, ammonia was added, when the ſolution, having been diluted with but a ſmall quantity of water, congealed into a ſemitranſparent gelatinous maſs, which it was neceſſary to agitate with a great deal of water, before it could be gotten out of the bottle.

The precipitate being waſhed and calcined was very compact, and had a brilliant fracture.

The liquor thus decompoſed by ammonia contained lime, as appeared on precipitating it with oxalate of ammonia.

Oxalate of cerium.

At the inſtant when the oxalate of cerium is precipitated by ammonia it is white and ſemitranſparent; but by agitation in the air and deficcation it aſſumes a yellowiſh colour, and becomes opake. A remarkable circumſtance is, that, if it be boiled with ammonia or potaſh before it is dry, it becomes again perfectly white and opake. This is not owing to any combination of the alcalies with the cerium, for when it has been well waſhed, no trace of them can be diſcovered by the moſt careful analyſis.

Does not combine with alkalies.

Component parts of cerite.

The reſiduum left untouched by the acids was afterwards examined; when it appeared, that the pureſt ore of cerium * from Baſtnas contained in 100 parts,

Oxide of cerium	63
Silex	17·5
Oxide of iron	2
Lime	3 or 4
Water	12
	98·5 †

Cerium,

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* Mr. Vauquelin analyſed other ſpecimens, which were mixed with green actinote and cupreous pyrites; but as nothing particular occurred in theſe analyſes, it is unneceſſary to enter into them.

† Meſſrs. H. and B. ſay: ſilex 23 parts, carbonate of lime 5·5, oxide of iron 22, and of oxide of cerium after calcination more than 50.

Cerium has two oxides.

Cerium, like ſeveral other metals, appears ſuſceptible of two very diſtinct degrees of oxigenation: the oxide which contains leaſt oxigen is white; that which is ſaturated with it is of a fallow red. Though they differ conſiderably in certain reſpects, their quantities of oxigen are not very diſſimilar, whence they are readily and eaſily commutable into each other.

Expoſed to the blowpipe with borax.

The white oxide expoſed to the blowpipe ſoon becomes red, but does not melt, or even agglutinate. With a large proportion of borax it melts into a tranſparent yellow globule*: with leſs the globule becomes opake on cooling. On heating gently a tranſparent compound of borax and oxide of cerium it becomes milky like a tin enamel.

Takes oxigen and carbonic acid from the air.

The white oxide of cerium becomes yellowiſh in the open air, but never ſo red as by calcination, becauſe it readily combines with carbonic acid, which oppoſes its union with oxigen to the point of ſaturation, and becauſe it always retains a portion of water, which diminiſhes its colour.

Alcalies do not act on it.

Cauſtic potaſh by the aſſiſtance of heat deprives the red oxide of part of its oxigen, and renders it white. This being dried, however, and urged to the ſtate of fuſion, becomes red again. Alcalies have no other action on it.

Sulphuric acid with the red oxide.

Sulphuric acid diſſolves the red oxide with great difficulty. Equal parts of it and of ſulphuric acid diluted with four times its weight of water combine readily when heated: the whole maſs aſſuming a cryſtalline form and brilliant aſpect. On adding freſh acid, and heating them together a long time, a complete ſolution takes place. This ſolution being evaporated by a gentle heat cryſtallizes in very ſmall needles, ſome of which are orange†, others of a lemon colour. If evaporated quickly, nothing but a yellow powder is obtained.

Two ſulphates.

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50. The increaſe of weight they aſcribe to oxigen abſorbed by the iron and the cerium.

[*] Firſt blood-red, then, as the heat decreaſes, green, yellowiſh, and finally colourleſs. If it be kept in the middle of the flame it continues as clear and colourleſs as glaſs. Theſe phenomena are more evident, if a phoſphoric ſalt be employed. If two colourleſs tranſparent globules, one formed with borax the other with a phoſphoric ſalt, be fuſed together, a tranſparent compound is produced, which on cooling becomes opake, and of a pearl colour. Meſſrs. H. and B.

With a phoſphoric ſalt.

[†] Theſe Meſſrs. H. and B. conſider as an acidulous ſulphate of cerium at a maximum of oxidation.

The ſulphate of cerium is ſoluble in water only by means of an exceſs of acid. Its taſte is ſaccharine and acid.

With the white oxide.

Sulphuric acid eaſily combines with the white oxide, particularly in the ſtate of carbonate. The ſolution is colourleſs, or with a ſlight roſy tinge; of a ſaccharine taſte without any perceptible acidity; and readily affords white cryſtals.

Nitric acid with the red oxide.

Nitric acid does not readily diſſolve the red oxide unleſs aſſiſted by heat. If the acid be ſuperabundant, the ſolution yields white deliqueſcent cryſtals: if not, no cryſtals are formed, but a yellowiſh ſalt is formed by deſiccation, of which alcohol at 38° will diſſolve half its weight. The nitrate of cerium is decompoſable by heat, and leaves a brick-coloured oxide.

With the white.

The white oxide unites more readily with nitric acid, but this ſalt is not more eaſily cryſtallizable. Its taſte is at firſt pungent, afterward very ſugary.

Muriatic acid.

Muriatic acid diſſolves the red oxide with efferveſcence. The ſolution cryſtallizes confuſedly. The ſalt is deliqueſcent, ſoluble in an equal weight of cold water, and in three or four times its weight in alcohol. The flame of this ſolution acquires no colour from the ſalt, but if agitated, white, red, and purple points appear in it *.

Oxigenated muriatic acid.

Oxigenated muriatic acid has no action on the red oxide, but diſſolves the white, without yielding to it any of its oxigen.

Carbonic acid.

The oxide of cerium unites eaſily with carbonic acid. The moſt ſimple and ready method of forming this compound is to decompoſe a ſolution of the nitrate or muriate of the white oxide by ſaturated carbonate of potaſh, when a very white precipitate will be formed with efferveſcence, which is very light, and on drying aſſumes a ſhining ſilvery appearance.

Hidro-ſulphures ſeparate iron from cerium.

Sulphurated hidrogen does not combine with cerium: but hidroſulphures may be employed ſucceſſively to ſeparate any iron that may be mixed with it; for, when this is the caſe, the firſt portions of hidroſulphure will throw down from the ſolution of cerium a greeniſh precipitate till no more iron remains.

Tartarous acid.

The white oxide will unite directly with tartarous acid, but requires an exceſs of the acid to render it ſoluble †.

Mr.

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* When this ſolution is concentrated it burns with a yellow ſparkling flame. Meſſrs. H. and B.

† Meſſrs. H. and B. have obſerved, as well as Mr. V. that, if the

Reduction of the metal.

Mr. Vauquelin made ſeveral unſucceſsful attempts to reduce this metal; at firſt he uſed the oxalate made into a paſte with fat oil. However, having mixed tartrite of cerium with a very ſmall quantity of oil and lamp-black, he put it into a crucible of charcoal bedded in ſand in an earthen crucible, and heated it for an hour and half in a forge furnace. A metallic globule ſcarcely as large as a pin's head was now left in the coal, but no other trace of cerium could he diſcovered, though the ſand was examined with the utmoſt care.

The globule examined.

None of the ſimple acids acted on this globule, but it diſſolved, though with extreme difficulty, in aqua regia, after being triturated. The ſolution was reddiſh, and exhibited unequivocal marks of iron: but it likewiſe gave evident ſigns of the exiſtence of cerium, both by its ſaccharine taſte, and by the white precipitates which tartrite of potaſh and oxalate of ammonia threw down. The metallic globule too was harder, much more fragile, more ſcaly in its fracture, and more white than pure caſt iron.

Volatile, but attempt to ſublime it fruitleſs.

As from theſe experiments cerium appears to be volatile, a ſimilar mixture with the addition of borax was heated in a porcelain retort, to the neck of which a porcelain tube was adapted. Whether from the inſufficiency of the heat however, or from the metal being volatilized without adhering to the neck of the retort, no trace of ſublimate was found. But ſeveral very ſmall metallic globules remained in the retort, adhering to a black varniſh formed by the borax. There were ſome of theſe globules about the upper part of the veſſel, to which apparently they had been ſublimed by the force of the fire; but all theſe globules together would not have amounted to a fiftieth part of the cerium employed.

Simple and ready method of freeing cerium from iron, and obtaining the oxide pure.

the ſalts of cerium, decompoſed by tartrite of potaſh ſtill contain traces of iron, the iron remains diſſolved in the liquor, particularly if a ſlight exceſs of tartrite be employed. Accordingly they have propoſed this method as the beſt and ſimpleſt for freeing the cerium from iron. The proceſs they recommend for obtaining pure oxide of cerium is, to diſſolve in nitro-muriatic acid any quantity of cerite, carefully ſelected and thoroughly calcined. To filter the ſolution, neutralize it by cauſtic potaſh, and then precipitate by tartrite and potaſh. The precipitate well waſhed, and afterward calcined, is pure oxide of cerium.

IX.

Abſtract of a Memoir, entitled Conſiderations on Colours, and ſeveral of their ſingular Appearances; read at the Claſs of Mathematical and Phyſical Sciences of the National Inſtitute, March, 1805, *by* C. A. PRIEUR [*].

Object of the memoir.

OUR author here endeavours to account for ſeveral phenomena, which appear to him never yet to have been properly explained: or rather it is his object to exhibit a general theory, by means of which all caſes of coloured appearances, even the moſt extraordinary, may be referred to certain principles.

Begins with the colours reſulting from a mixture of rays.

He ſets out from the known opinions concerning the various ſpecies of luminous rays, the colours reſulting from a mixture of ſeveral of theſe rays taken at different parts of the ſolar ſpectrum, and among others the very remarkable caſe, where the rays are ſo choſen, that their union produces on the organ of ſight the ſenſation of whiteneſs, even if two ſorts of rays only be employed.

For which we are indebted to Newton.

For theſe ideas we are indebted to the diſcoveries of the immortal Newton, and they flow immediately from the method he has propoſed for determining what colour would be obtained from a mixture of certain quantities of other given colours.

Preliminary requiſites.

If we would thoroughly comprehend what paſſes in the ſeeing of colours, it is indiſpenſable in the firſt place to obtain a familiar acquaintance with the ſhades compoſed of ſeveral ſimple rays; to acquire preciſe ideas of *black* and of *white*, and of the complication theſe introduce into coloured appearances; and more eſpecially to underſtand the relation of colours, which, taken two and two in a certain order, are capable of forming by their union white, or if you pleaſe any other compound tint.

Complementary colours.

Two colours having this kind of relation to each other are reciprocally termed *complementary colours*: one of theſe being given, the other may be determined with more or leſs preciſion by various modes of experiment, calculation, or ſimple reaſoning; and the conſideration of them applies very uſefully to a great number of caſes, as will be ſeen farther on.

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[*] Tranſlated from the *Annales de Chimie*, Vol. LIV. p. 5, April, 1805.

We here paſs over many particulars, which perſons verſed in the ſcience of optics, or habituated to the practical application of colours, will eaſily ſupply. Beſides, the ſubſequent part of the memoir, of which we have undertaken to give an account, will furniſh an opportunity of repeating what is moſt neceſſary for underſtanding theſe ſubjects.

Contraſts.

After theſe preliminaries the author proceeds to obſervations on *contraſts*. He employs this word to characterize the effect of the ſimultaneous viſion of two ſubſtances differently coloured, when brought near together under certain circumſtances. Contraſt then is here a compariſon, from which reſults the ſentiment of a certain difference, great or ſmall. It is pretty generally known, and painters in particular are well aware, that a coloured ſubſtance occupying a ſpace of little extent, and placed near or ſurrounded by a given colour, has not the ſame appearance as in the neighbourhood of another colour: but whence ariſes this difference?

Before we attempt to anſwer this queſtion, let us make an eſſential diſtinction. The colours in queſtion muſt be either homogeneal, that is formed of one ſort of rays only; or compound, that is formed of a mixture of different rays.

Contraſts of ſimple colours not yet examined.

In the firſt caſe, it muſt be confeſſed, we are ignorant, whether the approximation of different ſimple colours would produce any alteration in their reſpective appearance. As we ſeldom have an opportunity of ſeeing exhibitions of colour of this kind, and it is not eaſy to arrange ſuch at will, no experiments have yet been made on their contraſts. The ſubject, however, is well worth ſtudying.

Its effect produced by abſtracting from a colour the rays analogous to that contraſted with it.

As to compound colours, and ſuch are almoſt all thoſe of natural or artificial ſubſtances, as our author ſhews in the courſe of his paper, the new colours exhibited by contraſt are always conformable to the tint that would be obtained by abſtracting from the colour proper to one of the ſubſtances the rays analogous to the colour of the other.

Orange on red appears yellow; on yellow, red; on violet, yellowiſh; on green, another red:

Thus if we place on red paper a ſlip painted orange-colour, the latter will appear nearly yellow: on the contrary, the ſame ſtrip placed on yellow paper will appear nearly red. If we place it on violet paper it will reſume a yellowiſh tint, but different from the former; and laſtly, on green paper it will appear red, but in a different degree.

becauſe orange conſiſts of all rays but blue.

The explanation of theſe inſtances by the rule propoſed is eaſy, if we ſuppoſe the orange-colour of the little ſtrip to be compounded of all the rays except blue, which is commonly the caſe.

Contraſt modified by circumſtances.

A multitude of combinations of colours thus placed upon one another, bring out the colour of contraſt indicated by the rule above laid down; but there are ſeveral circumſtances that render the effect more ſtriking, or modify the reſult.

Degree of light.

Many contraſts at once.

Sometimes it depends on the degree of light by which the colours are obſerved. They may be illumined uniformly, or ſome more than others. The quantity of light entering ſimultaneouſly into the eye from the whole field of view, has likewiſe its influence. If the colours form ſeveral ſurrounding borders to each other, as a ſeries of circles decreaſing in ſize and placed one upon another would do, they will act reciprocally on each other. At every junction there will be on each ſide a border coloured by the contraſt of the adjacent tint. Theſe borders will be of greater or leſs extent in proportion to the brightneſs of the colour. The effect of a ſingle one may be ſufficient to deaden or annihilate all the reſt.

Effect increaſed by ſlight fatigue of the eye.

But not by exceſſive.

The colours of contraſt will appear likewiſe with greater vividneſs after having obſerved them a few moments, or if the coloured ſubſtances be ſhaken a little, ſo that they may paſs ſlowly over the retina. It ſeems as if a certain fatigue of the eye, either inſtantaneouſly with regard to the intenſity of the light, or more ſlowly by a prolonged viſion, concurred to produce the appearances in queſtion. But an exceſſive fatigue of the organ would produce a degeneration of the colours belonging to another mode.

Colour on the retina after expoſure to ſtrong light, not from contraſt.

We ought not therefore to refer to contraſt thoſe impreſſions mentioned by Æpinus, which are propagated in the eye with a certain duration, and a particular period of tints, when we have looked ſtedfaſtly on a very brilliant light, as that of the ſun.

Buffon's accidental colours are

But the colours termed by Buffon *accidental,* on which Scherfer has written an intereſting eſſay, belong to the claſs of contraſts, or at leaſt conſtantly obſerve the ſame law.

coloured ſhadows of the ſame nature;

Coloured ſhadows are another phenomenon of the ſame kind. Count Rumford has eſtabliſhed this fact beyond queſtion in two eſſays, where he has treated the ſubject in a very pleaſing manner.*

Mr. Prieur thinks, that thoſe appearances of the ſolar light received through a hole in a coloured curtain, which General Meuſnier had remarked on account of their ſingularity, are alſo to be aſcribed to contraſt. With this too he aſſimilates ſeveral caſes of colours diſplayed by opals, or, to ſpeak more generally, by bodies including perceptible opake parts diſſeminated through a tranſparent ſubſtance. In the ſame way he explains the colours under which the grayiſh duſt collected by age on papers, or on coloured ſtuffs, appears; and he draws the ſame inferences with reſpect to the blueiſh appearance of the veins of the human body.

alſo light through hole in coloured curtain; opals; old duſt on paper and cloth, and blueneſs of the veins.

He likewiſe propoſes a new method of rendering the colours of contraſt very ſenſible, more ſo than even by the known proceſs of accidental colours, and nevertheleſs without occaſioning any extraordinary fatigue of the eye. This laſt circumſtance is of no ſmall conſequence, for every one muſt be aware, that ſo delicate an organ cannot be ſtrained by over exertion without danger.

This method conſiſts, the obſerver being in a room with a good light, in placing againſt the window the coloured papers, on which he means to obſerve the contraſts in the manner above mentioned. The coloured paper ſerving as the ground will then poſſeſs a degree of ſemitranſparency, while the little ſlip of a different colour placed upon it is more opake, and in the ſhade, on account of the double thickneſs of paper: thus the colour produced by the contraſt is rendered much more ſtriking.

Method of rendering contraſts very ſenſible.

From this arrangement too reſults the ſingularly ſtriking effect of contact of a little ſlip of white paper applied ſucceſſively on paper, glaſs, and cloth of a given colour. When the tranſparent body is red, the opake white appears blueiſh green; if the ground be orange, it is decidedly blue; on a yellow ground, a kind of violet; on a crimſon ground, green, &c.; always correſponding exactly to the *complementary colour*.

Slip of white paper on coloured paper, glaſs, &c.

On this it muſt be obſerved, that, according to the rule already mentioned, if we abſtract from white, which is a com-

Explanation.

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* See his Philoſophical Works, Vol. I. p. 319, and following.

pound of all the coloured rays, the red rays for example, the remaining pencil ought to appear a very pale blueiſh green: but, as in the experiment above the little white ſlip is in the ſhade, the black hence ariſing may be of a proper degree to deſtroy the effect of the white, and then the blueiſh green appears of a lively tint. The ſame reaſoning is applicable to the caſe of all the other colours.

Reflected light muſt be avoided.

To obtain the full effect in repeating theſe experiments, we muſt take care, while procuring a favourable light, to guard againſt the reflection of adjacent bodies, and againſt double coloured fringes. Thus when the bright light tranſmitted through the window ſurrounds the tranſparent paper, it may very ſenſibly augment the brightneſs of the colour of contraſt, or injure it by introducing another tint, according to the colour of the body under obſervation. We have it always in our power, however, to get rid of this ſupercompoſition, by taking a piece of black cloth or paſteboard to maſk the object thus incommoded, or by looking through a blackened tube ſo as to confine the field of viſion to the neceſſary extent.

How.

Uſeful in the arts.

This knowledge of contraſt may be uſefully applied to thoſe arts, which are employed on the ſubject of colours. The painter is aware, that it is not a matter of indifference what colour is placed near another: but when he is acquainted with the law, to which their action on each other is ſubjected, he will know better what to avoid, and how to diſpoſe his tints, ſo as to heighten the brilliancy of that which he wiſhes to bring forward. Contraſting them together in ſucceſſion likewiſe affords us valuable indications of their nature and compoſition. This the author himſelf has put in practice with advantage in his manufactory of colours and paper-hangings.

White appearance of a coloured body through glaſs of the ſame hue.

Theſe conſiderations on contraſts led him to the examination of a very ſingular caſe, which Mr. Monge has mentioned and treated with his uſual ſagacity [*]. This caſe is the white appearance, which a coloured body ſometimes exhibits when viewed through a glaſs of the ſame hue. There remained ſome uncertainty reſpecting the circumſtances actually neceſſary for producing this effect: theſe our author determines by particular experiments, and he enumerates thoſe which have a favourable influence or the contrary. His concluſion is, that, when we have the perception of whiteneſs in theſe caſes, it is owing ſolely to the action of contraſts, by which the impreſſion of the colour is deadened or annihilated; while that of a certain degree of brightneſs ſtill ſubſiſts, and is noticed from the oppoſition of a greater degree of obſcurity. This manner of conſidering the ſubject leads to a new definition of whiteneſs which has certainly nothing in it inconſiſtent: *white is with reſpect to us the ſenſation of light, when no particular colour predominates in it, or is perceived in it.*

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[*] *Annales de Chimie*, Vol. III.

New definition of whiteneſs.

In the ſubſequent part of his memoir our author particularly conſiders the colouring of different opake and tranſparent bodies; that is to ſay, he inquires what are the luminous rays which a given coloured body is really capable of reflecting or tranſmitting.

Further ſubject of inquiry.

His method of making his experiments is ſimple. If the ſubſtance be opake, he places it on a piece of black cloth, and obſerves it with the priſm. If it cannot be cut ſo as to reduce it to a rectangular figure, he covers it with a piece of blackened paſteboard, in which there is a rectangular aperture. Under theſe circumſtances the coloured fringes diſplayed on two oppoſite ſides indicate the kind of rays reflected, and conſequently thoſe abſorbed when we know the nature of the illuminating pencil. On which we have farther to remark, that, as the fringes are themſelves compound tints, the ſimple tints that compoſe them muſt be diſcriminated. Their inſpection ſuffices an experienced perſon for this; but the habit is to be acquired, and its place ſupplied, by taking for a guide papers repreſenting each ſpecies of rays, placing them in their order one upon another, and drawing them back in gradation conformably to their difference of refrangibility: or we may uſe a table conſtructed after Newton's method for determining the compound tints of ſeveral elementary colours.

Method of making his experiments.

Compound tints to be diſcriminated.

How.

If the body to be examined be tranſparent, the aperture in the paſteboard juſt mentioned will be well adapted to cover it when placed againſt the light, ſo that the priſm may exhibit fringes on it. Or, if the obſerver place himſelf in the dark, a light, as that of a candle, will exhibit through the tranſparent ſubſtance, by the aſſiſtance of the priſm, a ſeries of coloured images correſponding to the rays tranſmitted.

Method of examining tranſparent bodies.

Making his experiments in this manner, our author diſcovered that ſeveral opake ſubſtances which happened to be at hand,

Colours of opake bodies owing to abſorption.

Laws.

hand, of various natures and of all colours, whether yellow, orange, red, or green, blue and violet, owed their coloured appearance to the following laws: 1ſt, each of the bodies always abſorbed the rays that were complementary to the predominant colour: 2dly, in ſome the abſorption included, beſide the complementary ſpecies, others collateral to this ſpecies, and more or leſs numerous: 3dly, the deeper a colour is, the fewer ſpecies of rays it reflects.

Relates to chemical compounds, not mechanical mixtures.

It is to be underſtood that mixed colours are not here ſpoken of, but only thoſe that form a homogeneal compound, or a true *combination*, in the ſenſe in which chemiſts uſe this word. Nor muſt the colour reflected from the interior of the molecules, ſuſceptible of light or deep tints, be confounded with the light reflected from the anterior ſurface of bodies: and though this mixes more or leſs with the proper colour, it is eaſy to diminiſh its effects, and diſcriminate them in the experiments.

Predominant colour.

Another remark proper to be made, is, that the expreſſion *predominant colour* muſt not be ſuppoſed to imply, that the rays of this colour are more abundant than the reſt, which would be a miſtake. Several ſpecies of rays may exiſt together in the pencil producing the colour, without any one ſpecies being for this reaſon more abundant. Strictly ſpeaking, all the elements of the pencil are diſſimilar; and conſequently no one exiſts in it in greater quantity. But the general tone of colour remains analogous to that of the rays ſtyled predominant; for which reaſon it is well to retain the term, provided it be not taken in an exaggerated ſenſe.

Tranſparent bodies follow the ſame law of abſorption.

The author has likewiſe obſerved tranſparent bodies, ſuch as coloured glaſs of different ſorts, and liquors contained in a bottle with two broad parallel ſides. For theſe he found the ſame law of abſorption as for opake bodies, but ſtill more marked, and free from all doubt.

Its modifications.

This law is conſtant and regular. It depends on the nature of the body receiving the light, its denſity, and its thickneſs. It is likewiſe modified by the intenſity of the light of the illuminating body, and the kind of rays that compoſe this light.

Progreſs of the abſorption of rays.

The abſorption always commences with the rays moſt oppoſite to the predominant colour of the body illumined. It goes on to thoſe which come next in the ſpectrum; and thus proceeds regularly from one order of rays to the next in ſucceſſion,

ceſſion, never by fits, till it reaches the laſt. In conſequence the body grows darker and darker, and always finiſhes with becoming black. Sometimes it extends only on one ſide from the rays firſt abſorbed; at other times on both ſides at once, and either with equal pace, or more rapidly on one ſide than on the other.

Change of circumſtances varies the reſults.

If we vary each particular that affects the experiment ſeparately, we ſhall have a diſtinct progreſſion of reſults. That depending on the denſity of the ſubſtance is not always ſimilar to that ariſing from change of thickneſs. In receiving light of different kinds too on the ſame ſubſtance, the progreſs of abſorption is differently modified, and conſequently the colours changed.

Our author adduces inſtances of all theſe caſes. He takes them from the numerous experiments he has made with coloured glaſs, acid, or alcaline ſolutions of metals, and fluids tinged by the infuſion or decoction of vegetable ſubſtances. Theſe exhibit curious particularities, but we ſhall not here relate them, both for the ſake of brevity, and becauſe it is eaſy for any perſon to obſerve them, when once the track is pointed out.

May lead to the cauſe of permanent colours.

From all theſe obſervations taken together, many very important conſequences reſpecting the reciprocal action of bodies and light on each other are drawn; and perhaps at ſome future period they will tend to elucidate the grand queſtion concerning the cauſe to which their permanent colours are to be aſcribed.

Colour of bodies at different degrees of incandeſcence.

Lamps ſeen through miſt. Sun through ſmoaked glaſs.

After theſe hints, the author dedicates a concluding paragraph to the examination of ſeveral phenomena of different kinds. He points out the modifications that coals heated to different degrees of incandeſcence undergo in their colours. His remarks apply to other ſubſtances likewiſe, as iron in the ſtate of ignition, a long row of lamps with reverberators ſeen through a fog, or a white light ſeen through a glaſs blackened by progreſſive applications of ſmoke. In all theſe caſes the colours neceſſarily paſs through a ſeries of tints from white to yellow, orange, and red of a deeper and deeper ſhade, the reaſon of which he gives.

Colours of metallic oxides proportionate to the oxigen.

Metallic oxides too have a gradation of tints, according to their proportion of oxigen. A certain continued change in

vegetation

Of flowers, &c. vegetation produces the ſame effect on ſome parts of plants. The arts and chemical proceſſes exhibit the ſame in a multitude of circumſtances.

Uſe to the manufacturer. Hence the manufacturer may derive with advantage indications either of the progreſs of combinations, or of the proper inſtant for executing certain parts of his operations.

Coloured clouds. Our author next enters more particularly into the appearance of coloured clouds, particularly thoſe we ſee about the riſing and ſetting of the ſun. This phenomenon ſo generally known, had hitherto remained without explanation, though this had been attempted by natural philoſophers of the firſt rank.

Owing to abſorption of light, not to refraction. It is not owing to the refraction of the ſolar rays, but to the ſucceſſive abſorption of them, when they ſtrike on the inferior parts of the atmoſphere, which are more loaded with vapour.

This abſorption follows laws analogous to thoſe already mentioned. The quantity of vapours, and even their nature not being the ſame every day, produce correſponding differences in their effects.

Order of abſorption. Commonly the firſt rays attacked by theſe vapours are the blue adjacent to the violet. Soon after they attack the contiguous rays, gaining with more rapidity the blue properly ſo called; then the green, the yellow, and thus proceeding to the red. Hence the yellowiſh, orange, and red colours exhibited by the clouds. This period of tints, the evening for example, diſplays itſelf gradually as the ſun approaches the horizon. The ſame hues tinge terreſtrial objects, the part of the air neareſt the ſun, and this luminary itſelf. Accordingly when we can receive its rays on a priſm, we perceive that the rays actually abſorbed correſpond to the general tint of the moment.

Sun-ſet.

From the ſucceſſive increaſe of the vapours traverſed by the light in thickneſs and denſity, it follows likewiſe, that at the ſame inſtant clouds differently placed muſt be clothed in different hues. The higheſt may be white, while others not ſo high are yellow, and others ſtill lower proportionately more red. At equal elevations thoſe furtheſt from the point where the ſun ſets will incline to red, and thoſe neareſt it to yellow.

Blue and green ſhadows, owing to contraſt. We may then ſee blue or green ſhadows on bodies naturally white, as Buffon and other philoſophers have obſerved. Theſe, as has been ſaid, are nothing more than the effect of contraſt between the actual colour of the part enlightened, and that of the part in ſhade.

Contraſts affect the colour of the clouds.

Contraſts may likewiſe render the colour of the clouds complicated, as for inſtance, when a great portion of the ſky diſplays its blue tint. There are ſome clouds, the colour of which ariſes ſolely from this cauſe; and ſuch may be ſeen at times in the middle of the day, when we have a lofty mountain at our back, or are in any other ſituation where the eye is defended from the too powerful action of the ſolar light, either direct or reflected; but in this caſe the clouds have only a yellowiſh tinge, preciſely the complementary colour of ſky blue.

The Moon.

Sometimes we ſee the moon of a ſimilar colour, when it is very high, a little before or after the ſun paſſes the the horizon: farther it appears thus, or even completely white, when clouds variouſly coloured by the vapors of ſunſet or ſun-riſe exiſt in the air at the ſame time. From this concurrence of circumſtances we have a new proof of the difference of cauſes to which theſe colours are owing.

Red clouds over head at ſun-ſet.

Laſtly let us remark, that from the irregularity of the earth's ſurface, and of the ſtate of the atmoſphere, the phenomena are liable to be concealed or ſubjected to various interruptions. In our climate the colouring of the clouds ſeldom reaches its laſt ſtage. On ſome evenings however, when the ſky is very clear toward the part where the Sun ſets, while light clouds float very high over our heads, we ſhall ſee theſe at a ſubſequent period appearing of a very light red, heightened by the diminution of light on the earth, ſoon after obſcured, and at length becoming extinct in ſhade.

Concluſion.

The theory of colours imperfect.

Notwithſtanding the many beautiful diſcoveries already made reſpecting light, the theory of the production of colours has not yet attained a degree of generalization that renders it applicable to all caſes, or that ſimplicity of principles to which we are almoſt always led when we have diſcovered the real laws of nature. Many phenomena have eluded explanation, and that given of ſeveral requires correction. Our author has propoſed to eſtabliſh alterations in the theory, the neceſſity of which he points out. He ſupports his principles partly by the doctrine and facts generally admitted; partly by others leſs commonly known, though of ancient date; and laſtly by obſervations of his own. He is far from flattering himſelf, however, that a ſketch like the preſent exhibits the matter in a ſuitable light; and was ſoon aware that a ſubject ſo extenſive and ſo complicated required maturer labours.

The author intends to purſue the ſubject.

To fill up many gaps, unfold various points, and correct and extend others by farther reſearches, new experiments, and profound reflections, is an ample field of improvement; and this he will attempt, if his powers and his leiſure will permit.

It would likewiſe be uſeful, as well as juſt, to give at the ſame time an abſtract of what we owe to the genius of the great Newton, who opened the career in ſuch an admirable manner, and to thoſe philoſophers who have diſcovered new facts, or removed difficulties. Greater preciſion alſo ſhould be introduced into the language which we employ reſpecting colours, proportionate to the increaſe of our knowledge, and the actual ſtate of the arts and ſciences. Laſtly, in a ſubject like the preſent, it would not be too much to add the reſources of algebra and geometry to the treaſures of experiment, and if poſſible to the advantages of a better method.

X.

Report made by the Phyſical and Mathematical Claſs of the Inſtitute in Anſwer to the Queſtion, whether thoſe Manufactories, from which a diſagreeable Smell ariſes, may prove injurious to Health. Read in the Sitting of January, 1805, by Meſſrs. GUYTON-MORVEAU and CHAPTAL.*

THE miniſter of the home-department has conſulted the claſs on a queſtion, the ſolution of which is of eſſential import to our manufacturers.

Queſtion.

The object is to determine, whether the vicinity of certain manufactories can be injurious to health.

Its importance.

The ſolution of this problem muſt appear of the more conſequence, as, from the confidence which the deciſions of the Inſtitute naturally merit, it may hereafter form the baſis of deciſions in a court of juſtice, when ſentence is to be pronounced between the fate of a manufactory and the health of our fellow-citizens.

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* Tranſlated from the *Annales de Chimie*, vol. LIV. p. 86, for April, 1805.

The ſolution is ſo much the more important, it is become ſo much the more neceſſary, as the fate of the moſt uſeful eſtabliſhments, I will ſay more, the exiſtence of many arts, has depended hitherto on ſimple regulations of police; and that ſome, driven to a diſtance from materials, from workmen, or from conſumers, by prejudice, ignorance, or jealouſy, continue to maintain a diſadvantageous ſtruggle againſt innumerable obſtacles, by which their growth is oppoſed.

Manufactories objected to.

Thus we have ſeen manufactories of acids, of ſal ammoniac, of Pruſſian blue, of beer, and of leather, ſucceſſively baniſhed from cities; and we daily ſee appeals to authority againſt theſe eſtabliſhments made by troubleſome neighbours or jealous rivals.

Diſadvantage of having no fixed rules.

As long as the fate of theſe manufactories is inſecure, as long as an arbitrary legiſlation poſſeſſes a right to interrupt, ſuſpend, or fetter the hands of a manufacture; in a word, as long as a ſimple magiſtrate of police has at his nod the fortune or ruin of a manufacturer, how can we conceive, that he will be ſo imprudent as to engage in undertakings of ſuch a nature? How could it be expected, that manufacturing induſtry ſhould eſtabliſh itſelf on ſuch a frail baſis? This ſtate of uncertainty, this continual conteſt between the manufacturer and his neighbours, this perpetual doubt reſpecting the fate of an eſtabliſhment, paralyſe and confine the efforts of the manufacturer, and gradually extinguiſh both his courage and his powers.

It is an object of primary neceſſity therefore to the proſperity of the arts, that lines ſhould be drawn, ſo as no longer to leave any thing at the arbitrary will of the magiſtrate; to point out to the manufacturer the circle in which he may exert his induſtry with freedom and ſecurity, and to aſſure the neighbouring proprietor, that he has nothing to fear for his health, or for the produce of his fields.

To arrive at the ſolution of this important problem, it appeared to us indiſpenſable, that we ſhould take a view of each of the arts, againſt which the moſt clamour has been raiſed.

Claſſification of objectionable trades.

With this view we ſhall divide them into two claſſes. The firſt will compriſe all thoſe, the proceſſes of which allow aeriform emanations to eſcape from them into the atmoſphere, either in conſequence of putrefaction or fermentation, which may be deemed nuiſances from their ſmell, or dangerous from their effects.

The ſecond claſs will include all thoſe, in which the artiſt, operating by the aid of fire, developes and evolves in air or vapour various principles, which are more or leſs diſagreeable to reſpire, and reputed more or leſs injurious to health.

1ſt claſs. In the firſt claſs we may advert to the ſteeping of flax and hemp, the making of catgut, ſlaughter-houſes, ſtarch-manufactories, tanneries, breweries, &c.

2d claſs. In the ſecond, the diſtillation of acids, of ſpirits, and of animal ſubſtances; gilding on metals, preparations of lead, copper, and mercury, &c.

1ſt claſs injurious to health. The arts of the firſt claſs, conſidered in relation to the health of the public, merit particular attention, becauſe the emanations that proceed from fermentation or putrefaction are really injurious to health in ſome caſes, and under certain circumſtances: Steeping hemp. the ſteeping of flax and hemp for inſtance, which is performed in ponds or ſtill waters, infects the air and kills fiſhes; and the diſeaſes to which it gives riſe are all known and deſcribed: Accordingly wiſe regulations have almoſt every where enjoined, that this operation ſhould be carried on without the precincts of towns, at a certain diſtance from every dwelling, and in waters, the fiſh of which conſtitute no reſource for the public. Theſe regulations unqueſtionably ought to be continued; but as the execution of them is attended with ſome inconvenience, it is to be wiſhed, that Brale's method recommended. the proceſs of Mr. Brale, the ſuperiority of which has been confirmed by Meſſrs. Mongez, Berthollet, Teſſier, and Molard, ſhould ſoon become known and adopted.

Beer, vegetable colours, ſtarch, paper, &c. Other operations on vegetables, or certain products of vegetation, to obtain fermented liquors, as in breweries; to extract colours, as in the manufactures of litmus, archil, and indigo; or to diveſt them of ſome of their principles, as in manufactories of ſtarch, paper, &c. do not appear to us of ſuch a nature as to be capable of exciting any diſquietude in the mind of the magiſtrate. At all events the emanations ariſing from theſe ſubſtances in a ſtate of fermentation can prove dangerous only near the veſſels and apparatus in which they are confined, ceaſing to be ſo the moment they are mingled with the open air; ſo that a little prudence only is required, to avoid all danger from them. Beſides, the danger affects only the manufacturers themſelves, and by no means the inhabitants of the neighbouring houſes, ſo that a regulation enjoining theſe manufactories to be removed out of towns, and to a diſtance from any dwelling-houſe, would be an act of authority both unjuſt, vexatious, and injurious to the progreſs of manufactures, and in no reſpect a remedy for the evils attending the operation.

Catgut.

Some preparations extracted from animal ſubſtances require the putrefaction of theſe ſubſtances, as in the fabrication of catgut; but it is more frequently the caſe, that animal ſubſtances employed in manufactures are liable to putrefaction from being kept too long, or expoſed to too great warmth, as we particularly find in dyeing cotton red, a proceſs in which a large quantity of blood is employed. The miaſmata exhaled by theſe putrid matters ſpread far round, and form a very diſagreeable atmoſphere for all the neighbourhood to breathe; it is the part of a good government, therefore, to cauſe theſe ſubſtances to be renewed ſo as to prevent putrefaction, and the manufactory to be kept ſo far clean, that no refuſe of the animal ſubſtances employed ſhall be left to rot in them.

Dyeing cotton red.

Slaughter-houſes.

In this laſt point of view ſlaughter-houſes exhibit ſome inconveniencies; but they are not of ſufficient importance to require them to be placed without the precincts of towns, and aſſembled together in one ſpot, as ſpeculative men are daily propoſing to government. A little attention on the part of the magiſtrate, to prevent butchers from throwing out the blood and refuſe of the beaſts they kill, would be ſufficient to remedy completely every thing diſguſting or unhealthy ariſing from ſlaughter-houſes.

Poudrette.

The fabrication of *poudrette* (night-ſoil dried) begins to be eſtabliſhed in all the large towns of France, and the operation by which excrementitious ſubſtances are reduced to this ſtate, neceſſarily occaſions a very diſagreeable ſmell for a long time. Eſtabliſhments of this kind therefore ought to be confined to airy places, remote from any habitation; not that we conſider the aeriform exhalations from them as injurious to health; but no one can deny, that they are incommoding, noiſome, diſagreeable, and difficult to breathe, on all which accounts they ought to be removed to a diſtance from the dwellings of men.

Animal putrefaction dangerous only in proportion to its humidity.

There is a very important obſervation to be made on the ſpontaneous decompoſition of animal ſubſtances, which is, that the emanations from them appear to be ſo much the leſs dangerous, as the ſubſtances which undergo putrefaction are leſs humid: in the latter caſe, a conſiderable quantity of carbonate of ammonia is evolved, which imparts its predominant character to the other matters volatiliſed, and corrects the bad effects of ſuch as are deleterious. Thus the decompoſition of ſtercoraceous matters in the open air, and in places the ſituation and declivity of which allow the fluids to drain off, and that of the refuſe of the cocoons of the ſilk-worm evolve a vaſt quantity of carbonate of ammonia, which corrects the virus of ſome other emanations; while the very ſame ſubſtances, decompoſed in water or drenched with this fluid, exhale ſweetiſh and nauſeous miaſmata, the reſpiration of which is very dangerous.

2d claſs. The numerous arts in which the manufacturer produces and diffuſes in the air, in conſequence of his proceſſes and by the help of fire, vapours more or leſs diſagreeable to breathe, conſtitute the ſecond claſs of thoſe we have to examine.

Theſe, more intereſting than the former, and much more intimately connected with the proſperity of our national induſtry, are ſtill oftener the ſubject of complaints brought before the magiſtrate for deciſion, and on this account have appeared to us to require more particular attention.

We will begin our examination with the manufacture of acids.

Acids. The acids that may excite complaints of the neighbours againſt their preparation are the ſulphuric, nitric, muriatic and acetous.

Sulphuric acid. The ſulphuric acid is obtained by the combuſtion of a mixture of ſulphur and nitre. It is very difficult in this proceſs to prevent a more or leſs obſervable ſmell of ſulphurous acid from being diffuſed around the apparatus, in which the combuſtion is performed; but in manufactories ſkilfully conducted this ſmell is ſcarcely perceptible within the building itſelf, is not dangerous to the workmen who reſpire it daily, and can give no reaſonable foundation for complaint to the neighbours. When the art of making ſulphuric acid was introduced into France, the public opinion was ſtrongly expreſſed againſt the firſt eſtabliſhments for the purpoſe; the ſmell of the match with which we kindle our fires contributed not a little to exaggerate the effect that muſt be produced by the rapid combuſtion of ſeveral hundred weight of brimſtone; but men's fears on this head are now ſo much allayed, that we ſee ſeveral of theſe manufactories proſper in peace in the midſt of our cities.

Aqua fortis and ſpirit of ſalt.

The diſtillation of aqua fortis and ſpirit of ſalt, in other words, of the nitric and muriatic acids, are not more dangerous than that of ſulphuric acid. The whole of the proceſs is performed in an apparatus of glaſs or earthen-ware, and it is unqueſtionably the great intereſt of the manufacturer to diminiſh the volatilization or loſs of the acid as much as poſſible. Yet, let him pay whatever attention he will to this, the air breathed in the manufactory is always impregnated with the ſmell peculiar to each of theſe acids; but you may reſpire there freely and ſafely, the men who work in it daily are not at all incommoded by it, and the neighbours would be very much in the wrong to complain.

Vinegar.

Since the manufactories of white lead, of verdigris, and of ſugar of lead have increaſed in France, the demand for vinegar has been enlarged.

Diſtilled vinegar.

When this acid is diſtilled, to fit it for ſome of the purpoſes for which it is uſed, it diffuſes to a diſtance a very ſtrong ſmell of vinegar, in which there is no danger; but when a ſolution of lead in this acid is evaporated, the vapours aſſume a ſweetiſh character, and produce in thoſe who reſpire them conſtantly all the effects peculiar to the emanations of lead itſelf. Happily theſe effects are confined to the people who work in the manufactory, and are unfelt by thoſe who dwell in the vicinity.

Mineral preparations, and gilding.

The preparations of mercury and of lead, thoſe of copper, antimony, and arſenic, and the proceſſes of gilding on metals, are none of them without ſome danger to the perſons who reſide in thoſe manufactories, and are concerned in the operations; but their effects are bounded by the walls within which they are carried on, and are dangerous only to the perſons concerned in the manufactories. It is an object well worthy the attention of chemiſts, to inveſtigate the means of preventing theſe injurious effects, and indeed many of the inconveniencies have already been prevented by the help of chimneys, which convey the vapours into the air out of the reach of reſpiration; and at preſent the whole attention of adminiſtration [illegible] ought to be confined to directing ſcience toward the means of improvement of which theſe proceſſes are ſuſceptible with regard to health.

Pruſſian blue, and ſal ammoniac.

The fabrication of Pruſſian blue, and the extraction of carbonate of ammonia by the diſtillation of animal ſubſtances in the new manufactories of ſal ammoniac, produce a large quantity of fetid vapours or exhalations. Theſe exhalations, it is true, are not injurious to health; but as it is not ſufficient to conſtitute a good neighbour, not to be a dangerous one merely, but not even to be a diſagreeable one, they who undertake ſuch manufactures, when they have to ſeek a ſituation for them, ſhould prefer one remote from any dwelling-houſe. But when ſuch a manufactory is already eſtabliſhed, we would be far from adviſing the magiſtrate to order its removal: it would be ſufficient in ſuch caſes, to oblige the manufacturer to build very high chimneys, that the diſagreeable vapours produced in theſe operations may be diſſipated in the air. This is particularly practicable for the fabrication of Pruſſian blue, and by adopting it one of our number has continued to retain in the midſt of Paris one of the moſt important manufactories of this kind we have, againſt which the neighbours had already leagued.

Few injurious to health.

In the report we lay before the claſs we have thought it our duty to attend only to the principal manufactories, againſt which violent clamours have been raiſed at divers times and places. It is eaſy to ſee, from what has been ſaid, that there are but few the vicinity of which is injurious to health.

Caution to magiſtrates.

Hence we cannot too ſtrongly exhort thoſe magiſtrates, who have the health and ſafety of the public committed to their charge, to diſregard the unfounded complaints, which, too frequently brought againſt different eſtabliſhments, daily threaten the proſperity of the honeſt manufacturer, check the progreſs of induſtry, and endanger the fate of art itſelf.

They ſhould not liſten too readily to complaints.

The magiſtrate ought to be on his guard againſt the proceedings of a reſtleſs or jealous neighbour; he ſhould carefully diſtinguiſh what is only diſagreeable or inconvenient from what is dangerous or injurious; he ſhould recollect that the uſe of pit-coal was long preſcribed, under the frivolous pretence that it was injurious to health; in ſhort, he ſhould be fully aware of this truth, that, by liſtening to complaints of this nature, not only would the eſtabliſhment of ſeveral uſeful arts in France be prevented, but we ſhould inſenſibly drive out of our cities the farriers, carpenters, joiners, braſiers, coopers, founders, weavers, and all whoſe occupation is more or leſs diſagreeable to their neighbours. For certainly the employments juſt named are more unpleaſant to live near than the manufactories mentioned above, and the only advantage they enjoy is that of ancient practice. This right of toleration has been eſtabliſhed by time and neceſſity; let us not doubt therefore, but our manufactures, when grown older and better known, will peaceably enjoy the ſame advantage in ſociety; in the mean time we are of opinion, that the claſs ought to avail itſelf of this circumſtance, to put them in a particular manner under the protection of government, and declare publicly, that the manufactures of acids, ſal ammoniac, Pruſſian blue, ſugar of lead, white lead, ſtarch, beer, and leather, as well as ſlaughter-houſes, are not injurious to the health of the vicinity, when they are properly conducted.

Diſagreeable occupations ſanctioned by time.

Manufactures not injurious to health.

We cannot ſay as much for the ſteeping of hemp, making catgut, layſtalls, and in general eſtabliſhments where a large quantity of animal or vegetable matter is ſubjected to humid putrefaction. In all theſe caſes, beſide the diſagreeable ſmell they exhale, miaſmata, more or leſs deleterious, are evolved.

Injurious manufactures.

We muſt add, that, though the manufactories of which we have already ſpoken, and which we have conſidered as not injurious to the health of the neighbourhood, ought not to be removed, yet adminiſtration ſhould be requeſted to watch over them ſtrictly, and conſult with well-informed perſons for preſcribing to the conductors the moſt proper meaſures for preventing their ſmoke and ſmell from being diffuſed in the vicinity. This end may be attained by improving the proceſſes of the manufactures, raiſing the outer walls, ſo that the vapours may not be diffuſed among the neighbours; improving the management of the fires, which may be done to ſuch a point, that all the ſmoke ſhall be burnt in the fire-place, or depoſited in the tunnels of long chimneys; and maintaining the utmoſt cleanlineſs in the manufactories, ſo that nothing ſhall be left to putrify in them, and all the refuſe capable of fermentation be loſt in deep wells, and prevented from any way incommoding the neighbours.

Manufactures not injurious require ſome reſtrictions.

We ſhall obſerve too, that when new manufactories of Pruſſian blue, ſal ammoniac, leather, ſtarch, or any other ar-

New manufactories.

ticle

ticle by which vapours very inconvenient to the neighbours, or danger of fire or exploſions are to be eſtabliſhed, it would be wiſe, juſt, and prudent, to lay down as a principle, that they are not to be admitted into cities, or near dwellings, without ſpecial authority; and that, if perſons neglect to comply with this indiſpenſable condition, their manufactories may be ordered to be removed without any indemnification.

Summary. It follows from our report, 1ſt, that catgut manufactories, layſtalls, ſteeping of hemp, and every eſtabliſhment in which animal or vegetable matters are heaped together to putrify in large quantities, are injurious to health, and ought to be remote from towns and every dwelling houſe: 2dly, that manufactories where diſagreeable ſmells are occaſioned through the action of fire, as in the making of acids, Pruſſian blue, and ſal ammoniac, are dangerous to the neighbours only from want of due precautions, and that the care of government ſhould extend only to an active and enlightened ſuperintendance, having for its objects the improvement of their proceſſes and of the management of the fire, and the maintenance of cleanlineſs: 3dly, that it would be worthy a good and wiſe government, to make regulations prohibiting the future eſtabliſhment of any manufacture, the vicinity of which is attended with any eſſential inconvenience or danger, in towns or near dwelling-houſes, without ſpecial authority previouſly obtained. In this claſs may be compriſed the manufactories of poudrette, leather, and ſtarch; foundries, melting houſes for tallow, ſlaughter houſes, rag warehouſes, manufactories of Pruſſian blue, varniſh, glue and ſal ammoniac, potteries, &c.

Such are the concluſions which we have the honour to lay before the claſs,* and addreſſed to government, with invitation to make it the baſe of its deciſions.

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* Theſe concluſions were adopted by the Inſtitute.

XI.

Facts relative to the Torpid State of the North American Alligator.

By BENJAMIN SMITH BARTON, M. D.*.

IT has not, I think, been remarked by the generality of the writers on natural hiſtory, that the North American Alligator paſſes during the prevalence of cold weather, into the torpid ſtate. This however, is unqueſtionably the caſe in ſome parts of the continent.

Account by by Boſſu.

Mr. Boſſu, a French writer, after telling us that theſe animals are numerous in the Red River, one of the weſtern branches of the Miſſiſſippi, ſays, " they are torpid during the cold weather, and lie in the mud with their mouths open, into which the fiſh enter as into a funnel, and neither advance nor go back. The Indians then get upon their backs, and kill them by ſtriking their heads with hatchets, and this is a kind of diverſion for them †.

Dr. Foſter, the tranſlator of the work, obſerves in the preceding paſſage, " that the circumſtance of the alligator's being torpid during winter is quite new, and very remarkable for natural hiſtory." It ſeems (he adds) almoſt all the claſs of animals called *amphibia*, by Dr. Linnæus, when found in cold climates grow torpid during winter.

Another account by Mr. Graham.

In addition to the authority of Mr. Boſſu, I may here mention the following fact, which was communicated to me about the year 1785, by a Mr. Graham, at that time a very intelligent ſtudent of medicine in the Univerſity of Pennſylvania.

The alligator ſwallows pine-knots previous to becoming torpid.

" The alligator having previouſly ſwallowed a number of pine-knots, retires to his hole, where he remains in a torpid ſtate, during the ſeverity of winter. If killed at this ſeaſon, theſe knots are found highly poliſhed by their trituration one againſt the other in the animal's ſtomach, as I have more than once heard from men of undoubted veracity, who had

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* From " the Philadelphia Medical and Phyſical Journal". Collected and arranged by Ben. Smith Barton, M. D. It is publiſhed in half yearly Numbers, the firſt of which appeared in November, 1804.

† Travels through that part of North America formerly called Louiſiana. Engliſh Tranſlation, Vol. I. p. 367. London 1771.

been concerned in the fact. Indeed this is ſo notorious in thoſe parts in which theſe creatures abound, that the digeſtion of the alligator's ſtomach is proverbial amongſt the multitude, who deride its inſipidity in the choice of ſuch food, though, I preſume, this it does inſtinctively, for ſome purpoſe unaccounted for by naturaliſts; and which, perhaps is beyond the limits of human ken."

The fact related by Mr. Graham, relates to the alligator of the Carolinas, in which parts of the United States this animal is very common. By another gentleman I have been informed, that the pine knots which the alligators ſwallow are generally ſuch as are very abundant in turpentine. I have alſo been aſſured, by my friend Mr. William Bartram, that he has ſeen a brick-bat which was taken out of the ſtomach of an alligator, and that it was worn quite round.

Local ſituation of this animal, &c.

Mr. Lawſon ſays, that the alligator is not ſeen to the North of North-Carolina. They are very common at Cape-Fear in latitude 34. One twelve feet in length has been ſeen at this place. On the Atlantic ſide of the United States I am not able to trace them farther than the "Alligator Diſmal Swamp," which is between Edenton and Newbern in North-Carolina. The mouth of the Red River in latitude 31.

Within the tract of country juſt mentioned, the alligator obeying the impulſe of the climate, paſſes into the torpid ſtate. In North-Carolina this takes place about the middle of November, ſooner or later, according to the ſtate of the ſeaſon. Whether the animal becomes torpid in more Southern parts of the Continent, I have not been able to learn. On the river St. John in Eaſt Florida, they have been ſeen awake even in the middle of winter, but it was remarked that they ſeemed dull and ſtupid. It has alſo been obſerved, that they are accuſtomed to frequent the warm ſprings which are ſo abundant in this part of the Continent; and that they are fond of lying in theſe ſprings. Perhaps the heat of theſe ſprings may be ſufficient to prevent them from becoming torpid. But it muſt be obſerved, that a deficiency of heat is not the only cauſe of the torpid condition of animals.

Conjectures reſpecting their ſwallowing the knots of the pine.

It may not perhaps be an eaſy taſk to aſſign a ſatisfactory cauſe for the ſingular inſtinctive appetite, which leads the alligator, before going into the torpid ſtate, to ſwallow pine-knots, and other ſomewhat ſimilar ſubſtances. But I apprehend that theſe ſubſtances, when taken in by the animal, act in ſome meaſure by keeping up a certain degree of action in its ſtomach, and conſequently in every part of the ſyſtem, and thereby prevents the death of the animal, which might otherwiſe be deſtroyed by the long continued application of cold. Some facts mentioned by Dr. Pallas, though they reſpect a very different family of animals, render this conjecture not a little plauſible[*].

This ſubject is worthy of more attention. In particular, it will be well to enquire, whether the alligator does ſwallow pine-knots, ſtones, &c. in thoſe parts of America in which it does not paſs into the torpid ſtate.

XII.

Obſervations and Experiments on the conducting Power of Fluids.

By T. S. Traill, *M. D. From the Author.*

To Mr. NICHOLSON.

SIR, *Liverpool, Sept.* 10, 1805.

IF you think the following obſervations and experiments worthy of a place in your excellent Journal, your inſerting them will oblige,

Your obedient ſervant,

J. S. TRAILL, *M. D.*

Doctrine of Count Rumford, that fluids are non-conductors of heat, controverted.

Count Rumford was the firſt who maintained that fluids are abſolute non-conductors of caloric. This concluſion he drew from the intereſting fact he had diſcovered, of the extreme ſlowneſs with which ice melted when a ſtratum of cold water was interpoſed between it and the heated body. He imagines that it was always melted in ſuch circumſtances, either by *currents* produced, in ſome of them by changes in ſpecific gravity, or by the tranſmiſſion of caloric through the ſides of the containing veſſel. The experiments of this illuſtrious philoſopher have rouſed the attention of the learned, and to the united labours of yourſelf, of Thomſon, of Dalton, and of Murray, we are indebted for an inveſtigation of the Count's opinions, the reſult of which ſeems to be, that fluids are not abſolute *non-conductors* of caloric.

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[*] Hiſtoria Glirium, &c.

Experiments of Dalton, Thomſon, Nicholſon, and Murray, conſidered by the Count.

The experiments of Dalton and Thomſon have proved, that the appearances of currents, ſuch as deſcribed by Rumford, may be often illuſory; and from thoſe of Nicholſon, and from Murray's firſt experiments, we have ſtrong reaſons for ſuppoſing, that the temperature was affected by the conducting power of the fluids employed; but in my opinion the experiments of Murray with a cylinder of ice, are the moſt complete demonſtration of this conteſted point. In a late paper, inſerted in the Tranſactions of the Royal Society of London, Count Rumford endeavours to obviate theſe objections to his hypotheſis, in his uſual ingenious manner.

It is not apprehended that the experiment of Murray could be affected by currents:

Even admitting that in your experiments the caloric was tranſmitted ſolely by the containing veſſel (an opinion by no means probable), and that *currents*, ſuch as Rumford deſcribes, have all the effect he attributes to them in certain caſes; ſtill the experiments of Mr. Murray appear to me incontrovertible. It was not, therefore, without ſurpriſe, that I obſerved him uſe the following argument to invalidate their reſults: "When that veſſel is conſtructed of ice, the flowing "down of the water, reſulting from the thawing of the ice, "will cauſe motions in the liquid, and conſequently inaccu"racies of ſtill greater moment;" viz. than thoſe produced by the conducting powers of the ſides of the veſſel. Now the melting of the ice could affect the thermometer only by being itſelf heated, and then trickling down the ſides of the cylinder of ice. But I apprehend, the water reſulting from the melting of the ice could not gain a higher temperature than 32° F. while it remained in contact with ice. If we mix even equal parts of water at 172° and ice, we do not find that the temperature of the mixture is above 32°. If ſuch a large quantity of water cannot maintain its temperature in contact with ice, can we ſuppoſe that ſuch a ſmall quantity as was formed could riſe to a higher while trickling down the ſides of a thick cylinder of ice.

and certainly not that with mercury in a veſſel of ice.

But even this explanation of the phenomenon advanced by Count Rumford, is entirely inapplicable to the experiment with mercury; for the drops of water formed could not poſſibly ſink in a fluid ſo much more denſe, nor throw it into currents which could reach the thermometer.

Proofs that fluids are proper conductors.

Beſides thoſe moſt ingenious experiments deviſed by Murray, we have other proofs of the conducting power of liquids in ſeveral well known facts.

1. They take a common temperature on mixing.

1ſt. If the *non-conducting* power of liquids have any meaning, it muſt ſignify that their particles are incapable of communicating to each other the temperature they have acquired by phyſical contact with ſome other body, whoſe temperature was elevated. If this were true, how ſhall we account for the mean temperature produced by mixing equal quantities of hot and cold water? Rumford, if I recollect aright, has endeavoured to obviate this objection to his hypotheſis by ſuppoſing, that it is only an intimate mixture of hot and cold particles which takes place in ſuch caſes. If this were true, we ſhould expect, from the rapid motion he ſuppoſes the currents to have in liquids that are heated, that they would ſoon ſeparate into warmer and colder ſtrata, from the difference in their ſpecific gravities: This however is not the caſe: The whole acquires a uniform temperature.

2. More particularly water and mercury.

2d. When mercury and water at different temperatures are mixed, an interchange of caloric takes place. From the very great difference of their ſpecific gravities we cannot ſuppoſe that every particle of the one has been in contact with every particle of the other; yet they ſoon acquire a common temperature, which though not a mean, has always a conſtant relation to the temperature of the two fluids before mixture. Does not this indicate a conſiderable conducting power in thoſe liquids? Indeed, I cannot conceive that any interchange of temperature could take place in ſuch caſes, if the particles of the liquids were incapable of communicating their caloric to the next particles.

3. The veſſel is too bad a conductor to account for the effects urged againſt the doctrine of C. R.

3rd. The beautiful experiment deviſed by Rumford, in which water, in a glaſs tube, was made to boil over a cake of ice, by the application of a heated body to the upper part of the containing tube, without, for a very long time, affecting the ice, is a ſufficient proof of the ſlowneſs with which glaſs tranſmits caloric, and clearly indicates that the ſides of the veſſel in ſeveral of the experiments of the above-mentioned philoſophers, could not be the ſole conducting medium.

4. Hot water poured on cold does not raiſe its temperature by currents.

4th. The ſixteenth experiment in Rumford's ſeventh eſſay, affords another argument againſt his opinion. He poured boiling water on a ſtratum of cold water, which reſted on a cake of ice in the bottom of a jar; he found that near the ſurface of the ice the temperature was 40°, at the diſtance of three inches it was 159°, but at the diſtance of ſeven inches it was only 160°. Had the cold water acquired its elevation of temperature by the currents produced, or by the ſides of the veſſel, we ought, I apprehend, to have found the temperature ſpreading more uniformly: but though the firſt four inches only differ by one degree, we find the next three differing by 119 degrees.

5. Heat applied to fluids downwards.

5th. If liquids were abſolute non-conductors of caloric, it would neceſſarily follow, that when caloric was applied to the upper ſurface of different liquids, other circumſtances remaining unchanged, and provided the liquid did not increaſe in ſpecific gravity by cooling, *equal increments of temperature would take place in equal times.*

Apparatus for experiments of tranſmitting heat downwards through fluids.

From ſeveral experiments it is probable, that ſome liquids conduct caloric more rapidly than others. The following were undertaken with a view to aſcertain more accurately this point: How far I have ſucceeded I leave you to judge:

A cylindrical veſſel was turned out of wood, having its ſides 0.5 inch thick; its height four inches, and its diameter two. It has a moveable wooden top or cover perforated with a hole in its centre a little more than an inch in diameter, into which an iron cylinder of one inch in diameter could be eaſily introduced. This cylinder is ſupported by a ſlight flanch or ſhoulder-piece, and can be taken up by means of a ſtring attached to its top. When the iron bar is in its place, its flat lower extremity is 0.5 inch diſtant from the bulb of a delicate mercurial thermometer D E, which is fixed by wax, in a hole perforating the cylinder near its bottom. This thermometer, which was made by the late Ramſden, has a tube as fine as a human hair, and is bended to a right angle, ſo that its bulb and part of its ſtem lie in the axis of the wooden cylinder. This ſhape was preferred, becauſe the ſtem could be little affected by the caloric tranſmitted by the ſides of the veſſel, till after the bulb was acted on by the caloric of the iron bar.

Into various liquids in ſucceſſion at 67° F. a cylinder of metal at 212°

A variety of experiments were performed with this apparatus in the following manner: The temperature of the room being ſteadily 67° F. during the trials, a kettle of water was kept boiling over the fire: Its temperature was between 211° and 212°, and into this the cylinder of iron was ſuffered to remain, and the riſe of thermometer 0.5 inch beneath, was obſerved in three minutes.

main, at each experiment, for 15 minutes. The liquid to be examined, and all the apparatus (but the iron bar), were, at each experiment, aſcertained to be at 67°. The liquid was poured into the wooden veſſel, till it could riſe 0.1 inch on the ſide of the iron cylinder when in its place: The wooden top was put on, and the iron was drawn out of the kettle of boiling water by means of the attached ſtring, and inſtantly let down through the hole of the cover. The time the thermometer took to riſe through three degrees (to 70°) was accurately marked by means of a ſtop-watch, and the reſults of my experiments on ſeveral fluids are exhibited in the following

Table of reſults.

TABLE.

	Liquids.	Minutes.	Seconds.
1.	Water, - - - -	7	5
2.	Milk of a Cow, - -	8	25
3.	Proof Spirit, - - -	8 nearly	
4.	Alkohol. London Pharm.	10	45
5.	Tranſparent Olive Oil, -	9	50
6.	Mercury, - - -	0	15
7.	Solution of Sulphate of Iron, one part of Salt to five of Water, - - - -	8	0
8.	Saturated Solution of Sulphate of Alumine, -	9	40
9.	Ditto Solution of Sulphate of Soda, - - -	6	30
10.	Aqua Potaſs. Puræ. Lond. Pharm. - - -	8	15
11.	Saturated Solution of Sulphate of Soda, but the Liquid not touching the Iron Cylinder by 0.1 Inch, or *nearly ſo*, -	19	20

The temperature was always too high to produce a deſcending current in water by heating.

As the water in the firſt experiment was employed at a temperature above 42°, it could not affect the thermometer by any change of denſity; it may therefore ſerve as a ſtandard to compare the other liquids. With regard to the differences of a few ſeconds, we need not inſiſt on it as indicating any material difference between the conducting power of the different ſubſtances; becauſe the eye may not be able to mark it inſtantaneouſly: but where this difference amounts to nearly a quarter a quarter of a minute, much more when to ſeveral minutes, we may fairly conclude, that there is a difference in conducting power.

In all theſe experiments the ſides of the apparatus ſhould have produced equal increments, had this been the cauſe of the riſe of the thermometer; and it is evident that currents downwards could not affect it. That the ſides of the veſſel could not communicate the temperature to the thermometer, nor even the radiant caloric affect it in the manner obſerved, the eleventh experiment (which by the way aroſe from an error in the mode of conducting the trial with ſulphate of ſoda) ſufficiently demonſtrates. From an inſpection of the table, it will be ſeen, that the aqueous ſolutions of different ſalts differ materially from each other in the celerity with which caloric is propagated through them.

I attempted to meaſure the conducting powers of ſeveral of the weaker acids, but I was ſoon convinced that their action on the iron might invalidate the accuracy of the reſults.

The fluids are proper conductors.

It will be unneceſſary to obſerve that if we find the thermometer requiring different times for its elevation, in ſuch caſes, we muſt aſcribe it to the conducting powers of the medium between it and the heated body.

If I am not deceived, we may conclude from what I have above adduced, that liquids as well as ſolids are conductors of caloric; that the tranſmiſſion of it through them follows a particular law depending on the properties of the particular liquid, but which is not in the exact ratio of any of their mechanical properties, though nearer that of their *denſity* than any other.

The Count's facts may be as well explained by the ſlow conducting energy of fluids as by its negation.

Very ſlow currents will explain his fact of the Glaciere of Chamouni.

Such, Sir, are the principal arguments that ſeem to militate againſt Count Rumford's hypotheſis, which he has, with that ingenuity which diſtinguiſhes his reſearches, applied to the ſolution of many important phenomena of nature. Theſe, however, may be equally well explained by ſuppoſing liquids very bad conductors of caloric; and, if the currents cauſed in liquids by changes in temperature, have even a very inferior velocity to what he ſuppoſes, we may, I think, account ſufficiently well for the appearance he obſerved on the Glaciere of Chamouni, which he propoſes as a teſt of his opinions, by the decreaſe in denſity of water while its temperature deſcends from 42° to 32°, (a fact which the Count's late experiments confirm) without aſſenting to his opinions with regard to the non-conducting power of fluids. An examination of this would, however, extend farther this already too long letter; but if you deem ſuch an enquiry intereſting, it may be the ſubject of a future communication.

I am, Sir,
Your's with reſpect,
T. S. TRAILL.

*** As this letter did not come to hand till above a fortnight after its date, and the verbal deſcription is very clear, it was not thought neceſſary to poſtpone it for engraving the author's ſketch.—N.

XIII.

Indian Account of a remarkably ſtrong and ferocious Beaſt, which (they ſay) exiſted in the northern Parts of the State of New York about two hundred Years ago. Collected and communicated[*] *by Mr.* JOHN HECKEWELDER.

Account of the large animal called jagiſho by the American Indians.

THE jagiſho[†] (or naked animal, or bear, as ſome of the Indians call it) was an animal much ſuperior in ſize to the largeſt bear. It was remarkably long-bodied, broad down its ſhoulders, but thin, or narrow at its hind legs, or juſt at the termination of the body. It had a large head and a frightful look. Its legs were ſhort and thick. Its paws (the toes of which were furniſhed with long nails or claws, nearly as long as an Indian's finger) ſpread very wide. Except the head, the neck, and the hinder parts of its legs, in all which places the hair was very long, the jagiſho was almoſt naked of hair; on which account the Indians gave it the name of "naked."

Several of theſe animals had before this time been deſtroyed by the Indians, but this particular one had, from time to time, deſtroyed many of the Indians, particularly women and children, when they were out in the woods getting nuts, digging roots, &c. or when they were working in the fields. Hunters when faſt purſued by this animal, had no means of eſcaping

from

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[*] To the Editor of the Philadelphia Medical and Phyſical Journal, whence this is taken.

[†] The Indian name of this beaſt or animal.

Account of the large animal called jagiſho by the American Indians.

from it except where a river or lake was at hand, by plunging into the waters, and ſwimming *out*, or *down* the ſtream to a great diſtance, they effected their eſcape. When this was the caſe, and the beaſt was not able to purſue his intended prey any further, he would ſet up ſuch a roaring noiſe, that every Indian who heard it trembled with fear.

This animal preyed upon every beaſt it could lay hold of. It would catch and kill the largeſt bear, and devour it. While the bears were plentiful the Indians had not ſo much cauſe to dread the jagiſho; but when this was not the caſe, he would run about in the woods, ſearching for the track or ſcent of the hunters, and follow them up. The women became ſo much afraid of going out to work, that the men aſſembled to deliberate on a plan for killing him.

This beaſt had its reſidence at or near a lake, from which the water flows in two different ways (or has two different outletts), one northerly and the other ſoutherly. The Indians being well informed of this circumſtance, a reſolute party of them, well armed with bows, arrows, and ſpears, made towards the lake. They ſtationed themſelves on a high perpendicular rock, climbing up the ſame by means of Indian ladders, and then drawing theſe ladders up after them.

After being well fixed, and having taken up with them a number of ſtones, the Indians began to imitate the voices and cries of the various beaſts of the woods, and even thoſe of children, in order to decoy the jagiſho thither. Having ſpent ſome days in this place without ſucceſs, a detached party took an excurſion to ſome diſtance from the rock. Before they had reached the rock again, the beaſt had gotten the ſcent of them, and was in full purſuit of them. They, however, regained that poſition before he arrived, When he came to the rock, he was in great anger, ſprang againſt it with his mouth wide open, grinning and ſeizing upon it, as though he would tear it to pieces * * * *. During this time, numbers of arrows and ſtones were diſcharged at him, until at length he dropped down and expired.

His head was cut off, and was carried in triumph by the Indians to their villages or ſettlements on the North River, and was there fixed upon a pole that it might be ſeen. As the report of the death of the animal ſpread among the neighbouring tribes, numbers of them came to view the head and to praiſe the victorious Indians for their warlike deed.

Account of the large animal called jagiſho by the American Indians.

N. B. The Mahicanni claim the honour of this act.

REMARKS BY THE EDITOR.

The preceding traditional accounts of the Indians, concerning the "naked beaſt," are in ſome reſpects, ſo very extravagant, that they may perhaps be deemed altogether unworthy of any attention. I muſt confeſs, however, that I cannot but conſider ſuch traditions, though imperfectly handed down to us, and evidently disfigured by fable, as entitled to the notice of the naturaliſt and philoſopher.

That ſuch an animal as the Jagiſho is deſcribed to have been, has ever exiſted in the ſtate of New York, may perhaps admit of a rational doubt; but that the Indian tradition relates to *ſome remarkable* animal that is no longer to be ſeen in the country which it is ſaid to have inhabited, I think there is good reaſon to believe. What this animal was, at what period it ceaſed to be ſeen, and what was the more pure account of the Indians concerning it one hundred years ago, I do not pretend to determine.

Poſſibly the Indian tradition refers to the large animal, (I mean an individul of the ſame ſpecies,) ſome of whoſe bones have been found in a cavern in the back parts of Virginia; the animal of which mention is made in the firſt part of this Journal*. Is is true indeed that the Indian accounts of the activity of the New York animal are not very favourable to the idea, that the animal was Mr. Jefferſon's Megalonyx, which I have ſuppoſed belonged to the order of Tadigrada, comprehending the Sloth, the Armadillo, and others. What is ſaid of the claws of the Jagiſho may be thought to favour the notion that this was really the Megalonyx, or Megatherium. But I would not be underſtood to place any dependance upon the minute or deſcriptive circumſtances which are mentioned in the Indian tradition. Nor indeed do I think it at all probable that the Megalonyx (as it is called) or any of the ſpecies of elephants whoſe exuviæ abound in various parts of North America, have been ſeen in a living ſtate in this Continent, within the period of two, or even twice two hundred years.

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* Section Third, p. 152, 154.

SCIENTIFIC

SCIENTIFIC NEWS, &c.

Death of Profeſſor Claproth.

Klaproth.

SOME of the foreign Journals have announced the death of the celebrated Klaproth of Berlin, who, for the benefit of the Sciences, continues in good health in his ſixty-ſecond year. Mr. Juſtus Claproth, Profeſſor of Juriſprudence in the Univerſity of Gottingen, well known for ſeveral learned works on that ſubject, died on the 10th February laſt, in his ſeventy-ſeventh year.

Aſtronomical Prize.

Lalande's prize given to Harding.

The medal founded by de Lalande for the beſt aſtronomical work, has been adjudged by the National Inſtitute in its ſitting of April laſt, to Mr. Harding, for his diſcovery of the laſt new planet. That able aſtronomer has been appointed to the direction of the Obſervatory at Gottingen.

New Muſical Inſtrument.

New muſical inſtrument.

A Poliſh clock-maker named Maſlouſky, arrived at Berlin, at the beginning of the preſent year, with the intention of exhibiting a new ſtringed inſtrument, of his invention. Notwithſtanding a variety of advertiſements, he did not ſucceed in attracting the public notice; and he determined to exhibit the inſtrument at a concert previous to his departure. About 300 auditors attracted by the names of Himmel and Seidlen, who were to perform, attended, and towards the end of the concert nearly half the number retired. The artiſt proceeded to exhibit his Kœliſon, which is the name he gives the inſtrument. It conſiſts of a ſound-board, on which the uſual ſyſtem of wires of the piano are fixed. Between theſe wires are ſmall wooden cylinders, which being put into motion, communicate their vibrations to the wires. The tones are ſo ſoft and enchanting that the harmonica cannot equal them, the forte and piano are given in every imaginable gradation, and the whole effect was no leſs ſurpriſing than unexpected, and the maker Huhn received orders for a number of the inſtruments.

The preſent article is taken from Millin's *Magaſin Encyclopedique*, who does not ſay whether the wooden cylinders were moved in rotation or otherwiſe, nor how they were applied and preſſed againſt the ſtrings. The leading fact of this notice ſeems to be, that there are certain kinds of wood, and perhaps certain reſinous or other matters to be applied to them, that will produce the effect of a bow upon wire ſtrings in a ſuperior manner. It is indeed probable, that we do not yet poſſeſs much knowledge of the art of producing tones by the powerful expedient of bowing, or light friction; and mechanics have ſtill an ample field for applying this method with force, preciſion, and rapidity to the more compounded inſtruments.

Saverien.

Death of Saverien.

On the 28th of May laſt died Alexander Saverien engineer of the French marine; who has been ſixty years known to the ſcientific world, for his writings on navigation and the theory of building, rigging and manœuvering ſhips. He has written accounts of the inſtruments for making obſervations at ſea; a marine dictionary; a dictionary of the mathematics; a dictionary of architecture; an hiſtory of modern philoſophers, and an hiſtory of the progreſs of the human underſtanding. His works indicate a conſiderable ſhare of talent and very extenſive knowledge. For many of the laſt years of his life he was poor and infirm, and was much indebted to the cares of a ſervant who continued with him from motives of attachment. He died at the age of eighty-five, leaving behind him a widow likewiſe very aged and in want.

Pure and beautiful Ceruſe.

Pure and beautiful ceruſe.

Mr. Van Mons informs us, that, if lead aſhes be diſſolved in a ſufficient quantity of dilute nitric acid by the help of a gentle heat, filtered, and precipitated by chalk in impalpable powder, the precipitate, when waſhed and dried, will be the pureſt and moſt beautiful ceruſe poſſible. The queſtion which offers itſelf on this occaſion, is whether it could be afforded at a reaſonable price.

Chromate of Lead and of Silver.

Chromate of lead and of ſilver diſſolved in nitric acid.

Count Mouſſin Pouſchkin has diſſolved both the red lead ſpar and chromate of ſilver in nitric acid, by adding a little ſugar the moment the acid is poured on, and promoting the action by gentle heat. The ſpar then requires only five or ſix parts of acid, the chromate of ſilver ſtill leſs. Nitrous acid gas is evolved, and the ſolution of the former is an amethyſt colour, of the latter a garnet red, without the leaſt trace of green, either by reflection or refraction.

Putrefaction prevented.

Putrefaction prevented by red precipitate,

Dr. Valli having left a pound of ſoup in which were twelve or fifteen grains of red precipitate, expoſed to the open air for four months, found it exhibited no ſign of putrefaction, and did not even ſeem to have undergone any alteration. He then repeated the experiment for a month in the height of ſummer, with the ſame effect. The oxide in the mean time had neither diminiſhed in weight nor altered its colour.

without loſs of weight or colour;
and by the combination of exigen.

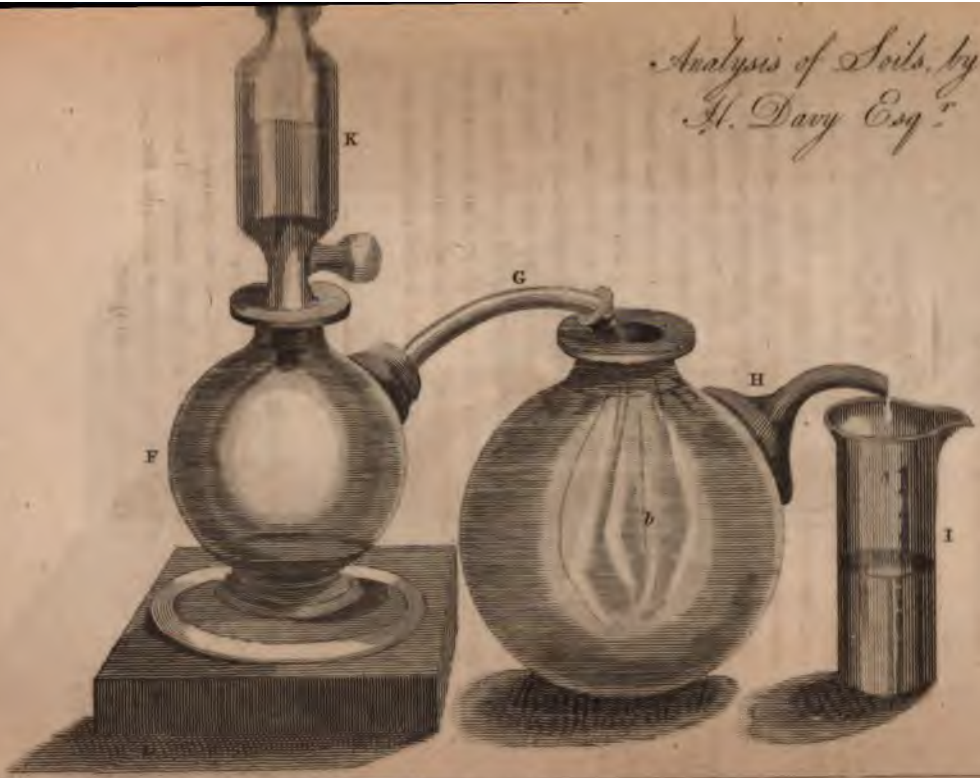
On this Mr. Van Mons obſerves, that he has found broth keep for years by means of a few grains of mercury in the ſtate of oxide and citrate: Nitrate of ſilver has long been conſidered as the moſt powerful of antiſeptics, and he has found thoſe of gold and of mercury equally ſo. Oxigenated muriate of potaſh retarded the putrefaction of ſtrong ſoup ſeveral days, and ultimately put a ſtop to it at a certain point. Very dilute nitric acid, and oxigenated muriatic acid in the ſtate uſed for bleaching, preſerved a moderate ſtrong ſoup for ſeveral months.

Leverian Muſeum.

Sale of the Leverian Muſeum.

The Leverian Muſeum which has been near 40 years in collecting at an expence of near 50,000*l.* will be ſold by public auction in May 1806, unleſs an acceptable offer for the purchaſe of the whole be previouſly made. The ſale will take place in the building which now contains the Muſeum. Catalogues are preparing with all ſpeed.

Analysis of Soils, by H. Davy Esq.r



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Analyses of Soils, by H. Davy Esq.r

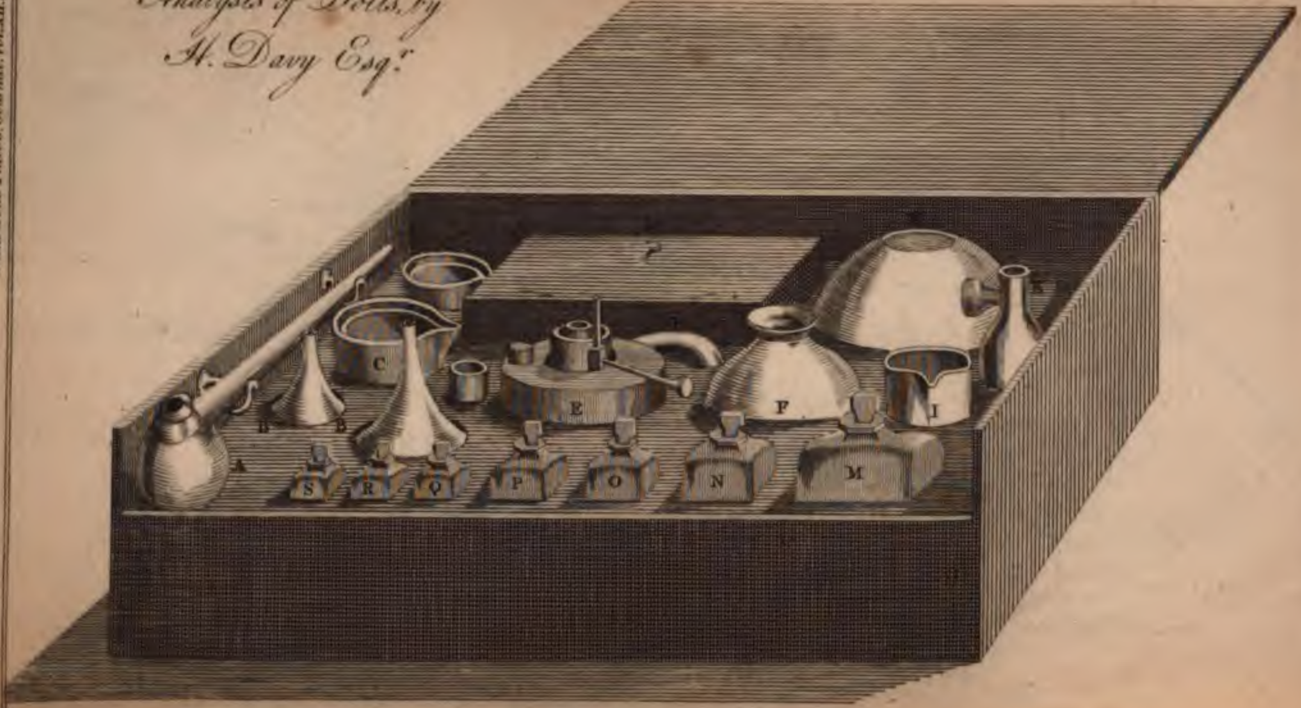
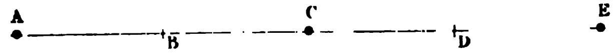


Fig. 1.



Count Rumford's Investigations concerning heat.

Fig. 2.

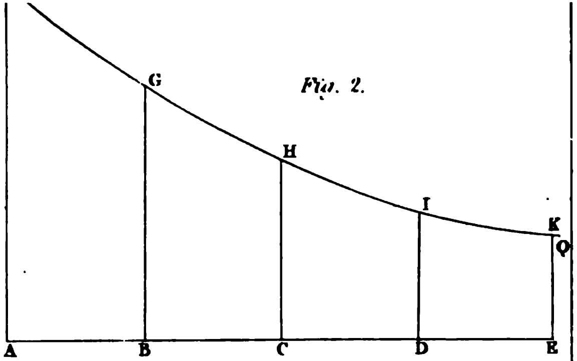


Fig. 3.

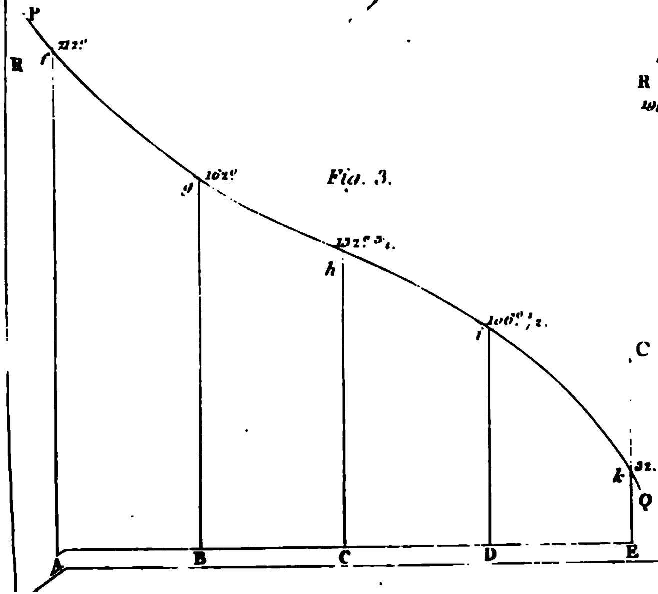
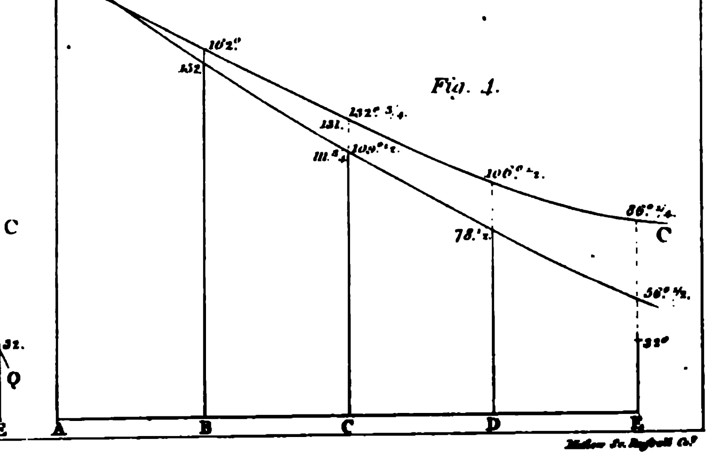


Fig. 4.



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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

NOVEMBER, 1805.

ARTICLE I.

Facts and Obſervations relating to the Blight, and other Diſeaſes of Corn. In a Letter from G. CUMBERLAND, *Eſq.*

To MR: NICHOLSON,

SIR, *Weſton-ſupra-Mare*, 10*th October*, 1805.

Reference to the Pamphlet of Sir Joſeph Banks on the blight in corn.

ALTHOUGH not much in the habit of taking any thing on truſt, I muſt confeſs, that when early in the ſpring the pamphlet of Sir Joſeph Banks, called, *A ſhort Account of the Cauſe of the Diſeaſe in Corn, called by the Farmers, the Blight, the Mildew, and the Ruſt*[*], came to my knowledge, I felt very much inclined on the credit of his extenſive fame to receive with a favourable prejudice, what was ſo poſitively announced as information, on a ſubject that we may all, I ſuppoſe, be allowed to have ſomething to do with.

Corn ſowed near a barberry buſh.

Having therefore much leiſure, and being placed in a favourable ſituation to make obſervations, I began doing all that could then be done, viz. planting ſome wheat near a barberry buſh, and ſearching carefully for the yellow ruſt on the early leaves.

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[*] Inſerted intire in our Journal X. p. 225.

Diſeaſed ſtraw being examined, it was ſeen that Mr. Bauer's drawings are very correct.

I next examined the ſtraw by the microſcope, making many diſſections with great attention. The reſult of which was, a conviction that the deſigners and engravers part of the fine plates that accompany this eſſay, has been excellently and faithfully managed, as far as the then ſeaſon would permit me to compare them, and ſome old ſtraws of the laſt year rendered it very probable, that the whole was correctly drawn.

Doubt whether this diſeaſe affects the grain.

But here I began to ſuſpect, that the *ruſt* was not ſo guilty as has been repreſented, for, admitting for a moment, that it does intercept the ſap by plugging the apertures ſaid to be deſtined in wet weather to receive the humidity of the atmoſphere, yet, as it is *not yet aſcertained* that it ſtrikes root, into the cellular texture beyond the back, for I could not admit of ſaying there is *no doubt* of a thing that *has not yet been traced*, ſee page 11 (or 227 of Journal) and as the plates, if they prove any thing *prove the contrary, vide* fig. 7. I thought we muſt wait awhile before we could charge to this cauſe the diminution of our flour. There was yet another motive in my humble opinion for doubt: I ſaw, even by examining the ſtraws of laſt year, that it was ſcarcely poſſible to find among hundreds of *ruſted ſtraws* that had *blighted heads*, one that any way partook of the ruſt except at the upper joint, and that partially only, while the ſheaths that nature has kindly given to ward off injuries, were compleatly conſumed with it. Now I believe, no one will pretend to aſſert, that this injury done to the ſheaths of the ſtraws could in any way affect the riſing of the *ſap* to the ear; we muſt therefore, I ſoon ſaw, confine the cauſe of injury, *if this be any cauſe*, to the quantity of fungi that more immediately attacks the upper bare joint of the ſtraw.

And even here it appeared at this early ſtage of my doubts to be very uncertain; for if we reflect firſt of all that it is by no means pretended to be proved, that theſe fungi do penetrate the cellular texture; and next, that if they do, it will remain to be proved, that by ſo doing they materially intercept the ſap; and laſtly (which I conceive to be no extravagant conjecture) that if they did, yet as far as the cellular texture of the ſtraw is concerned in conveying it, the interception could only be very partial.—Taking all theſe reflections together, I think I was grounded in entertaining doubts of the true *cauſe* having been exhibited, as is ſet forth in this pamphlet, p. 13.

P. 13. (or 228 Journal.) Where the preſident ſays, "though diligent enquiry was made during the laſt autumn, no information of importance relative to the origin or the progreſs of the *blight* could be obtained! this is not to be wondered at, for as no one of the perſons applied to had any knowledge of the *real cauſe* of the malady, none of them could direct their curioſity to the real channel. *Now that its nature and cauſe have been explained,* we may reaſonably expect that a few years will produce an intereſting collection of facts and obſervations, and we may hope that ſome progreſs will be made towards the very deſirable attainment of either a preventive or a cure." page 14.

Country names of the diſeaſes of wheat.

Having thus advanced in the examination as far as the ſeaſon would afford, I thought it would be beſt to aſcertain in the county in which I happened to be placed, the terms (intirely overlooked in the pamphlet) there applied to the different diſeaſes of corn; and here it ſoon appeared that the terms are not univerſal.

Firſt blight; or mildew, by which the embrio grain is turned to powder.

The firſt blight (for there are many) is that early appearance of intirely decayed ears, of plants apparently in a healthy ſtate, but which, in the embrio which lays within the upper ſheath, before the ear is develloped, has turned to a brown puce-coloured powder. This by ſome has been ſuppoſed to originate from defective ſeed, but ſurely improperly; for many of theſe ears are found to be the fineſt and largeſt in the field in their embrio ſtate; and to me it ſeems evident, that they are imperfect from a really defective parturition, owing to ſome accidental circumſtance; or poſſibly (for they generally ſtand below the others) from the want of ſun to uncloſe the upper ſheath at a critical moment.

The ears are uſually crooked at bottom, &c.

Theſe ears are alſo in general found to be crooked at the root of the ear ſtalk, owing to the effort to raiſe the ear acting within the ſheath on a decayed and mouldy ear; but it appears that they are all at length ejected from the ſheath, and the brown duſt blown compleatly away, ſo that the ſtalk at laſt remains without a ſingle grain on it, ſtanding up like a bare and barren pole. In this manner many ears at firſt periſh, but the quantity is ſeldom any object to the farmer.

Appearance of the powder; it produces no infection.

Of the puce-coloured powder a quantity was collected: it had no ſmell, but felt ſoft to the touch, like whiting, though more greaſy. An attempt was made to innoculate a number of other ſound ears with it; by rubbing it on the leaves, by giving friction to the ſtraw itſelf with it, by inſerting it into the ears, by placing it beneath the ſheaths, and laſtly, by introducing it into the pith of ſeveral ſtraws,—each of theſe in wet and dry weather, but nothing took any effect. It fell harmleſs:—but one diſcovery aroſe from examining the ears in which it was produced, viz. that corn is capable of being completely blighted without any external diſeaſe or application. For all the ſtraws were without blemiſh as well as the leaves, and conſequently we have no occaſion to recur to external cauſes for this internal decompoſition.

The firſt blight does not ariſe from an external cauſe.

Wheat in this ſtate I drew very accurately, and ſent ſpecimens of it to town in this condition to Mr. Nicholſon; and thus ended the firſt blight as it is called in Somerſetſhire, but which might poſſibly be with more propriety termed the *mildew.*

Second diſeaſe. Smut. Full grains, containing only a dark powder of a fiſhy ſmell.

The next diſeaſe appears in thoſe ears which ſtand erect and ſtaring, indicating their lightneſs by their attitude. Although on gathering they appear full of corn, they turn out in effect to be full of a dark powder that has the ſmell of ſtale lobſters or ſhrimps, when preſſed between the fingers. This powder on examination by the microſcope ſhews ſome ſaccharine concretions among it, but it has none of the actual properties of wheat. Upon carefully examining theſe grains the outer ſkin was diſcovered to be *intire, unperforated, perfectly green, and perfectly full*;—yet ſtrange to tell, if it be not really the work of inſects, one half of an ear was often found to be thus ſmutty while the other half was ſound.

Mechanical injury did not produce this diſeaſe.

Among other conjectures, it was thought that this ſmutty diſeaſe might ariſe from the juices of the ſtraw being intercepted by accidents. In order therefore to try what could be effected by injuries done to the ſap, I bound ſome ears, wounded others by preſſure, divided ſome with a knife near the ſtem, marking each by cutting off the beards with ſciſſars; yet I never found any ſuch effect produced, as either ſmut, or even decay, and all the ears thus injured came very well to maturity. Here were real injuries, and committed at a time (June) when the ears were by no means far advanced, and this led me ſtill leſs to expect any great effect from a little partial moſs adhering to the ſtem, the accidental effects of the ſeaſon at a later period.

At length on the 1ſt of Auguſt, I ſaw in a low field the firſt appearance of yellow ruſt, but collected one with difficulty; and now I found that it was univerſally agreed by our farmers, that this fungus, as our plate deſcribes it, was conſidered as the effect of fogs or great humidity, which firſt attacks the leaf or ſheath of the ſtraw, but ultimately penetrates, and vegetates on the upper joint of the ſtraw itſelf, where it is uncovered juſt before the corn is ripe, ſo that what our plate exhibits is by all agreed to be called the *ruſt* in all ſtages; but that the ruſt cauſes the *latter blight*, or ears with ſhrivelled grains does not ſeem to be ſo generally agreed.

Third diſeaſe; the ruſt conſidered as effect of humidity.

To prevent the *ſmut*, our farmers ſteep their wheat in ſalt water, in order to ſeparate the ſound grains from the light or blighted better than by freſh water, becauſe ſalt water naturally floats all but the heavieſt. In the *Venetian* ſtate I remember they added ſaltpetre to the ſteep on the ſame principle, a very different ſyſtem from that of Sir Joſeph, who recommends, I think very dangerouſly, the uſe even of tailings as ſeed, and this on mere hot houſe experience, ſee p. 25 (or 232 Journal.)

The proceſs of ſteeping grain is uſed to ſeparate the heavieſt for ſeed.

An old and good farmer* laſt year at *Weſton*, bought good wheat for ſeed becauſe his own was *ſmutty*, but not having quite enough, he ſowed about three pecks of his own ſmutty wheat to finiſh, and it turned out quite as good wheat as ſome of that which was bought as the beſt; yet this does not confirm the doctrine, I think.

Smutty grain not uſed for ſeed, yet ſome ſucceeded.

Having thus aſcertained what is called the early *blight or mildew*, together with that which follows, and is known by the term *ſmut*, and alſo the diſeaſe which comes next, and being ſimilar to Sir Joſeph's Banks's plate, is called *ruſt*, and laſtly the *latter blight* which is ſeen in merely ſhrivelled grains, or grains imperfectly ripened;—I ſhall now proceed to the ſpecimens which I gathered in all the ſtates, made drawings of them, and ſtill retain in the ear labelled with great care, as proofs of what is here advanced; after which I ſhall make ſome deductions.

Latter blight; or ſhrivelled grains.

On 22nd of July, 1805. I began my collection; and No. 1. contains healthy ears, clean to outward appearance, the loweſt leaf a ſpeck or two of fungus. Second joint from the head a little reddiſh. No bloom on the cane.

Accounts of ſpecimens of wheat gathered in this enquiry.

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* Mr. Oakley of Weſton-Supra-Mare.

Accounts of ſpecimens of wheat gathered in this enquiry.

No. 2. Much diſeaſed ſtraw, bloom or mould under the ſheath, and on the leaf; in other reſpects the ſtraw quite as healthy and ſound as No. 1.

No. 3. Entirely diſeaſed, yet every grain full and of its proper ſize—ſome grains evidently opened at their ſides by ſome ſmall inſect. The outſide of this ear quite green and healthy, not even a ſpot, ſmell of the ear very fiſhy, not wanting one grain. The head ſtalk not even waved at the ear, root intire, upper ſtalk a little yellow.

No, 4. A healthy ear of the bearded, thirteen rows of grains, ſtraw mouldy under the ſheath, and at joints, yet ſound, laſt or upper joint green.

No. 5. Another nearly ſimilar.

No. 6. A double ſtraw to one root, both ears perfect, both ſtraight at head ſtalk, both diſeaſed, yet full and plump; ſome grains ſound yet green, and cloſe to a black one; a rich golden coloured moſs or duſt at the back of the green coat, yet the grain coat perfectly green and uninjured, while the grain was compleatly full of the black fiſhy-ſmelling blight inſtead of flour. Straws green at the top and quite healthy throughout.

No. 7. A ſtraw that having been blighted in the ſheath by the early blight, had thrown off all its brown duſt, and grown to a ſtrong ſtraw: The ſkeleton of the ear only remaining, very crooked near the ear: The upper ſtalk evidently by its purple ſtripes diſeaſed under the ſkin, yet no moſs or fungus protruded on the cane; ſtripping back the firſt leaf from the head, I found the powder from the ear had adhered to the ſtraw under the ſheath, and that it was mouldy: The ſecond joint quite healthy under the leaf, but with the red and purple ſtreaks where uncovered to the light: The third joint the ſame: The ſheath leaves themſelves healthy.

No. 8. Spotted ear externally, ſound ſtraw, yet having black duſt at the joints, in this ear I found a maggot about the 30th part of an inch long, of the exact yellow of the powder found behind the diſeaſed grains, (that powder may be his excrement) viz. orange yellow, his form reſembled the maggot of nuts, lived an hour on the table.

No. 9. Diſeaſed ear, grains all blighted yet no yellow powder under the ſheaths of the grains, ſtraw healthy, ear fiſhy ſmell.

No. 10. Ditto with an inſect very active in the ear, yellow, ſee drawing A B. Fig. 1. Plate IX.

No. 11. Two perfectly healthy ears from the ſame root, both healthy throughout, yet on the leaf a ſpeck or two of Sir Joſeph's fungus was originated, and well grown: it was of the orange colour.

No. 12. An ear faſt ripening, ſolid in the grain, yet had loſt its firſt ſix lower grains, laſt joint green, beards yellow, a few ſpots on the ſheath of the grain, ſeemingly occaſioned by a ſmall black fly found in it.

Obſervations made on the ſame Day on Grains.

Diſeaſed grains deſcribed.

Diſeaſed grains were always found to be full and plump without any aperture; the fine ſkin that holds the flour very green, yet all black within; the external ſurface of the black matter covered with a white concretion, perhaps the ſaccharine matter of the wheat. Sound grains found in diſeaſed ears, all the diſeaſed ears ſmelt filthy.

Obſervations made on the Straw.

Diſeaſed ſtraw.

The diſeaſe always attacks the portion of the ſtraw that peeps beyond the ſheath leaf near the joint, and evidently commences at the pores, as old ſtraws will ſhew; but the fungus cannot, I think, be the cauſe of the diſeaſe; becauſe where no fungi have taken root, the corn is compleatly corrupted. Theſe fungi grow it is true, faſteſt on diſeaſed ears, probably becauſe when the ear is diſeaſed it draws leſs humidity from the ſtraw, ſo that the diſeaſed ears ſeem to aſſiſt the growth of the fungi, not the fungi the diſeaſe. In fact, I believe they live on the ſuperfluous moiſture of the ſtraw, or returning ſap.

Blighted ears, the ſtraw not ruſted.

On the 27th July laſt, 1805. I examined at leaſt twenty blighted ears of corn, and could only find one among them that had the ſmalleſt degree of *ruſt*, and that only a ſpeck or two on the lower leaf that ſheaths the third joint. The ſpots were orange colour and deep purple, and did not occupy half the diameter of a ſpace of three inches long.

Healthy plants having the ſtraw injured.

Upon again examining many *healthy plants*, I found not only their ſheath leaves, but even the ſtraw eaten away by ſome inſect, and at the ſame time diſcovered abundance of the green locuſt like inſect on the ears; of which, ſee the magnified deſign, Fig. 2. Plate IX.

Inoculated plants, and others injured by violence, took no diſeaſe.

Some plants that I had lately innoculated were ſtill ſound: others that I had pinched and bruiſed in the upper joint ſhewed no alteration, but in all reſpects reſembled the moſt healthy.

Plants gathered which have the ſtraw and the grain not in the ſame (healthy or diſeaſed) ſtate.

Aug 3rd, 1805, I went to a field of Mr. Oakley's, o Weſton, where the wheat had a general good aſpect; and ſelected and labelled, of ears green yet full.

No. 1. Sound ears with diſeaſed ſtraws.

No. 2. Compleatly ſmutty ears with ſound ſtraws in every reſpect.

No. 3. A ſmutty and a ſound ear from the ſame root. The ſound ear had a ſpeck of fungus.

No. 4. Ears with crooked tops, others twiſted by ſpiders; others with crooked beards, ſhort ſtalks and long; yet all of them full of grain, green and ſound.

No. 5. Ears half ſmutty, viz. on one ſide all the way up.

No. 6. Ears half ſtagged, (ſtagged means here thoſe that ſhew only the ſkeleton of the ear and the crooked upper ſtalk from early blight, all but the bare poles being blown away) theſe ears were half *ſtagged* and half covered with grains.

No. 7. Bare *ſtaggs*, but with quite fine ſound ſtraw.

Other inſtances of ſound wheat with ruſty ſtraw; and ſhrivelled wheat with ſound ſtraw.

Laſtly; in Auguſt and September, 1805, at Allcombe near Minehead, I collected out of a field juſt reaped, two bundles which with all the others I ſtill keep as proofs of my aſſertions: One all of ſound wheat with all their upper ſtalks very much covered with the ruſt of Sir Joſeph Bank's deſcription, and the other all of black and ſhrivelled ears, yet all ſound in the upper ſtalk.

The ſhrivelled corn was never fairly ripened.

Theſe latter mentioned ſtalks, I think, throw great light on my ultimate conjectures drawn from every obſervation through the whole ſeaſon; viz. they preſent ſhrivelled, blighted grains, and exhibit ſhort ears, becauſe on examination they were evidently never ſufficiently expoſed to light and heat: for all their ſtraws though clean were green, not yellow as thoſe of ripe wheat ought to be, and their ſmoaky miſerable appearance (not having the leaſt ſmell of the fiſhy ſmutt) could only ariſe from their humble ſituation below the other ears, where air, ſun, and light was deficient; in fact, they never ripened properly. The ſtraw remained green, and the ſap probably returned inſtead of being intercepted by the drying of the upper joint; and to me it now appears to be a fair conjecture, that what is generally called blighted corn, or thoſe ears that produce ſo many ſhrivelled grains, and which we are called upon to ſeek a remedy for, is nothing more than the effect of a practice of late much recommended, viz. *to reap early*, a practice not only promoted by the *Miller*, who is eager for the new corn to come to market, but by the avarice of the *Farmer*, who fears that by letting it ſtand too long the grain may fall in price, and reduce his profits; and, what is ſtill more unfortunate, by ſome agricultural writers of great reputation, who recommend it as ſtopping the progreſs of the ruſt, forgetting that the ſun only can effectually deſtroy that ſuppoſed evil, by well drying the ſtraw.

What is called blighted corn ſeems to be corn prematurely reaped, from avarice or ignorance.

Far be it from the writer of theſe few remarks to diſcourage any attempt at ſaving in a remarkably wet ſeaſon, or in remarkably wet ſituations, wheat that has paſſed the period uſually productive of ripe corn. He knows that in caſes of laid-wheat in ſhady ſituations, by reaping it early, we may accelerate the ripening of that which otherwiſe would not have ripened at all, by the operation of turning and expoſing the ſheaves to the ſun, and ſo make good ſaving crops; but what he wiſhes to guard againſt is, that eagerneſs for putting in the ſickle originating in the motives before alledged; for, reaſoning from analogy do we not always find, that in all other ſeeds that are to be gathered, thoſe alone are plump, ſound, and full of their proper flour, that are ſuffered to receive the utmoſt influence of the ſun while on the ſtalk; and he always thought many years ago, that we were in the habit of being too fearful of the latter ſeaſons; it is true that late harveſts are expenſive in collecting, but they are generally well matured, and the inſtance of *barley* that may be well ſaved (for colour can have little to do with the intrinſic value of grain) even as late as November, proves the juſtice of the obſervation; fruit gathered too ſoon, diſappoints all views of profit or pleaſure, and we might, he thinks, as well attribute the ſhrivelling of our apples early gathered to the influence of the apple-tree moſs, as the ſhrivelling of our grains of wheat to a ſuppoſed blight originating in the funguſes, that have of late ſo much alarmed the theoretical agriculturiſts, and œconomiſts of our day.

It is often adviſeable to reap early.

Argument in favour of late reaping.

On

Concluſion.

On the facts expoſed I could greatly enlarge, but I think on all accounts it is beſt at preſent to offer them in their preſent form to the reaſoning faculties of your readers. At any rate they may ſerve as data, and if they ſhould fail to bring others to my opinion, may act, I truſt, as uſeful ſtimulants to the further inveſtigation of a very curious ſubject of enquiry, as to what are the nature of the enemies to the perfection of our wheat harveſt.

With reſpect and eſteem,

I am, Sir,

Your moſt obedient humble Servant,

G. CUMBERLAND.

The vicinity of the barberry does not ſeem to affect grain.

P. S. I ought to obſerve, that on the grains of wheat ſown near the barberry, I had no opportunity of making obſervations; but that I have a dried root of wheat now by me, on which there are above 100 ſtraws that are all clean and ſound, though it grew a few years ago in a garden where barberry buſhes were.

II.

Experimental Inveſtigations concerning Heat. By BENJAMIN COUNT OF RUMFORD, *V. P. R. S. &c.*

(Concluded from Page 75.)

SECT. III. *Experiments tending to ſhew that Heat is communicated through ſolid Bodies, by a Law which is the ſame as that which would enſue from Radiation between the Particles.*

Object of inquiry; the laws of the propagation of heat in ſolids.

HAVING made a conſiderable number of experiments on the paſſage of heat through fluids, and through different ſubſtances in the ſtate of powder, I was curious to aſcertain the laws of its propagation through ſolid bodies, particularly metals.

I hoped this diſcovery would furniſh ſome additional data, to confirm or refute the opinions I had adopted concerning heat and its manner of acting; and it will be ſeen by the reſults, that my expectations were not fruſtrated.

Having

Deſcription of an inſtrument: Two cylindrical tin veſſels were connected by a bar of copper.

Having procured two cylindrical veſſels of tin, each ſix inches in diameter and ſix inches high, I faſtened them together by means of a ſolid cylinder of copper ſix inches long and an inch and half in diameter, which was fixed horizontally between the two tin veſſels. The extremities of the cylinder paſſed through two holes an inch and half in diameter, made for the purpoſe in the ſides of the veſſels, midway between the bottom and top, and were ſoldered faſt in them.

Each of the veſſels was made flat on the ſide where the copper cylinder was faſtened, ſo that the extremity of the cylinder did not project into the veſſel, but was level with the flattened part.

This inſtrument was ſupported at the height of eight inches and half above the table on which it ſtood, by means of three feet, two fixed to one of the veſſels, and one to the other.

The veſſels filled with water, one at 212°, the other at 32°.

One of theſe veſſels being filled with boiling water, the other with water at the freezing point; as the two extremities of the cylinder were placed in immediate contact with theſe two maſſes of fluid, a change of temperature muſt neceſſarily take place by degrees in all the interior parts of the cylinder. For the purpoſe of obſerving this change, three vertical holes were made in the cylinder, into which were introduced the bulbs of three ſmall mercurial thermometers. One of the holes was in the middle of the cylinder; the others midway between the centre and either extremity.

The changes marked by three thermometers at equal diſtances.

Each of theſe holes is four lines in diameter, and eleven lines and half deep; ſo that the bulbs of the thermometers, which are three lines in diameter, were all in the axis of the cylinder.

When the thermometers were put in their places, the holes were filled with mercury, in order to facilitate the communication of heat from the metal to the bulb of the thermometer.

The one water kept boiling by a lamp; and the other cold by addition of ice.

To keep the hot water conſtantly boiling, a ſpirit lamp was placed beneath the veſſel containing it; and to keep the cold water conſtantly at the temperature of melting ice, freſh portions of ice were added to it from time to time.

The thermometers are graduated to Fahrenheit's ſcale, the freezing point being marked 32°, and that of boiling water 212°.

The thermometers not noticed till nearly ſtationary.

As the firſt and moſt important object I had in view was, to learn at what temperature the three thermometers would become

come ſtationary, I did not very carefully notice the progreſs of the thermometers toward this point; but as ſoon as they appeared nearly ſtationary, I obſerved them with the greateſt attention for near half an hour.

The thermometers diſtinguiſhed.

To diſtinguiſh the three thermometers I ſhall call that neareſt the boiling water B, that in the centre C, and that neareſt the cold water D.

Experiment.

The following are the progreſs and reſults of an experiment made the 28th of April, 1804, the temperature of the air being 78° of Fahrenheit.

Tabulated reſults.

Time.			Temperature of the hot water.	Temperature marked by the thermometer B.	Temperature marked by the thermometer C.	Temperature marked by the thermometer D.	Temperature of the cold water.
H.	m.	s.	Degrees.	Degrees.	Degrees.	Degrees.	Degrees.
1	52	15	212	160	130	105	32
—	53	30	—	160½	131	105½	—
—	55		—	161	131¼	106	—
—	56	30	—	161¾	132	106½	—
—	58		—	162	132½	107	—
2	0	0	—	162	132¾	107½	—
—	1	30	—	162	133	107½	—
—	4		—	162	132¼	106½	—
—	6		—	162	132	106	—
—	9		—	162	132¼	106½	—
—	11		—	162	132¼	106½	—
—	28		—	162	132¼	106½	—

Account of the reſults that would have followed the hypotheſis of heat being propagated through bodies by radiation from particle to particle.

Before I proceed to examine more minutely the reſults of this experiment, I will endeavour to ſhow thoſe reſults which it ought to have exhibited, on the ſuppoſition that heat is propagated, even in the interior of ſolid bodies, by *radiations* emanating from the ſurfaces of the particles compoſing theſe bodies.

The particles of ſolid bodies are diſtant from each other.

On this ſuppoſition we muſt neceſſarily conſider the particles that compoſe bodies as being *ſeparate from each other*, and even to pretty conſiderable diſtances compared with the diameters of theſe particles; but there is nothing repugnant to the admiſſion of this ſuppoſition; on the contrary, there are many phenomena which apparently indicate that all the ſolid bodies with which we are acquainted are thus formed.

To ſee now by what law heat would be propagated in a ſolid cylinder, let us repreſent the axis of this cylinder by a right line A E, *Plate* VII. *Fig.* 1; (ſee our laſt nomber) and let us begin with ſuppoſing that the cylinder conſiſts of three particles of matter only, A C E, placed at equal diſtances in that line.

If the ſolid cylinder conſiſt of three equi-diſtant particles,

Let us farther ſuppoſe, that the extremity A of the cylinder is conſtantly at the temperature of boiling water, while its other extremity, E, remains invariably at the freezing point.

—and the extremes at the boiling and freezing temperatures.

By an experiment, of which I have already given an account to the claſs*, I found that when two equal bodies, A B, one hotter than the other, are iſolated and placed oppoſite each other, the intenſities of their radiations are ſuch, that a third body, C, placed in the middle of the ſpace that ſeparates them, will acquire a temperature by the ſimultaneous action of theſe radiations, which will be an arithmetical mean between thoſe of the two bodies A and B.

Then the middle particle would have the arith. mean temperature: A body midway between two others acquires the mean heat by radiation,

From the reſult of this experiment we have ground to conclude, that if the cylinder were compoſed of three particles of matter only, A, C, E, the particle C, which is in the middle of the cylinder, muſt neceſſarily have the arithmetical mean temperature between that of A and that of E, which are at the two extremities of the cylinder; that is to ſay, between 212° and 32° of Fahrenheit, which is 122°.

—or 122°.

Now let us interpoſe between the particles A, C, and E, two other particles B D, and ſee whether the introduction of theſe two particles will make any change in the temperature of the particle C that occupies the middle of the cylinder.

Suppoſe two more particles to be interpoſed.

If the particle B be placed in the middle of the ſpace compriſed between the extremity A of the cylinder and its middle, C, it ought to acquire a mean temperature between that of the extremity A of the cylinder, and that of the point C, namely that of 167°, the mean between 212° and 122°; and if the particle D be placed in the midſt of the ſpace compriſed between the middle of the cylinder and its other extremity, E, this particle ought to acquire a mean temperature between that of the middle of the cylinder and that of its extremity E; it ought then to have the temperature of 77°.

Theſe would each take the mean temperature between the middle particle and the neareſt end particle,

From this new arrangement, the particle C, ſituate in the middle of the cylinder, will find for its neighbours on one ſide

—and therefore they would not alter the temperature, the particle B, at the temperature of 167°, and on the other the particle D, at that of 77°. The point in queſtion is, whether the preſence of theſe two particles will make any change in the temperature of the particle C, or not.

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* See our Journal, IX. 193.

—of that middle particle.

In the firſt place it is evident, that if the calorific influences of the particle B on the particle C be as efficacious in heating it, as the frigorific influences of the particle D be in cooling it, the temperature of the particle C ought not to be changed. But experience has ſhewn, that, at equal diſtances and equal intervals of temperature, the calorific influences of hot bodies, and the frigorific influences of cold bodies, are exactly equal; and as the diſtance from B to C is equal to the diſtance from D to C, while the interval of temperature between B and C= 45°, is the ſame as that between D and C=45°; it is evident that the temperature of the particle C, which is in the middle of the cylinder, can be no way affected by the introduction of the intermediate particles B and D.

And by the ſame reaſon it would not be changed by other particles interpoſed.

By the ſame way of reaſoning may be proved, that the introduction of an indefinite number of intermediate particles would produce no change in the temperature of the middle of the axis of the cylinder, or in any part of it; and if the introduction of an indefinite number of intermediate particles make no change in the ſtate of a thermometer placed in the middle of the axis of the cylinder, we may conclude that the thermometer would remain equally ſtationary, if the number of intermediate particles were increaſed till they had that proximity to each other which is neceſſary to conſtitute a ſolid body. If, inſtead of a ſingle row of particles in a right line, there were a bundle compoſed of an indefinite number of ſuch rows placed ſide by ſide, forming a ſolid cylinder, the temperature in the different parts of the line A E would remain the ſame.

But the temperature of a continued ſolid ſhould decreaſe from one particle to another in arithmetical progreſſion.

From this reaſoning we may infer, that the temperatures of the different parts of the cylinder ſhould decreaſe in arithmetical progreſſion from one extremity of the cylinder to the other.

This is true only when the ſolid is remote from other bodies.

But it is evident, that this law of decrement of temperature could take place only in the ſingle caſe of the ſurface of the cylinder being completely iſolated, ſo as to be no way affected by the action of ſurrounding bodies, which is abſolutely impoſſible.

Our experiments are always thus influenced.

The circumſtances under which the experiments were made are very different from thoſe here taken for granted. The bodies we ſubject to experiment are conſtantly ſurrounded on all ſides by the air and other bodies which act on air inſtruments continually, and often in a very perceptible manner; and we can never hope to iſolate a cylinder ſo completely that the apparent progreſs of heat in its interior ſhall perceptibly obey the law we have juſt diſcovered. In common caſes it deviates widely from this law.

Appreciation of the effect.

As the cauſes of this deviation are well known, we will ſee whether there be no means of appreciating their effects.

The atmoſphere will affect the cylinder,

The ſurface of the cylinder being ſurrounded by the atmoſpheric air and other bodies, all which are of a known and ſenſibly conſtant temperature, we may determine the comparative effects of theſe bodies on the different parts of the ſurface of the cylinder.

—by cooling its hot part and heating its cold part.

In thoſe parts of the cylinder which are hotter than the air and other ſurrounding bodies, the ſurface of the cylinder will be cooled by the action of theſe bodies; but if one of the extremities of the cylinder be colder than the atmoſpheric air, thoſe parts of the cylinder which are colder than the circumambient fluid will be *heated* by its influence and that of the ſurrounding bodies.

If one extremity of the ſolid be at the temperature of the air, the other hotter,

We will begin with examining the caſe where the coldeſt extremity of the cylinder is at the ſame temperature as the ſurrounding air. Let us ſuppoſe then, that the experiment with boiling water at the one end and freezing at the other be made when the temperature of the air is at the freezing point, or 32° of Fahrenheit.

—the ſurface will be every where cooled,

In this caſe it is evident that the ſurface of the cylinder muſt every where be *cooled* by the influence of the ſurrounding atmoſphere. The queſtion then is to determine the comparative effects, or the relative quantities of refrigeration or loſs of heat, that muſt take place *in the different parts of the cylinder:* and in the firſt place it is clear, that the hotter a given part of the cylinder is, the more heat it muſt loſe in a given time, by the influence of the ſurrounding cold bodies; whence we may conclude, that the refrigeration of the ſurface of the cylinder by the influence of the air and other ſurrounding cold bodies muſt neceſſarily diminiſh from the extremity of the cylinder A, which is in contact with the hot water, to its extremity E, which is in contact with the cold.

—but moſt ſo where the heat is greateſt.

The change is in proportion to the difference of temperature.

From reaſoning which appears incontrovertible, and which the reſults of a great number of experiments appear to confirm, it has been concluded that the celerity with which a hot body placed in a cold medium is cooled, is always proportional to the difference between the temperature of the hot body and that of the medium. Conſidering this concluſion as eſtabliſhed, we may determine *a priori* what ought to be the gradation of temperatures in the interior of a given ſolid cylinder ſurrounded by air, one extremity of which is in contact with a conſiderable body of boiling water, while the other is ſimilarly in contact with cold.

We have ſeen that, if the ſurface of the cylinder were perfectly iſolated, the decreaſe of temperature from the hotteſt extremity of the cylinder A to its other extremity E, which is in contact with cold water would be *in arithmetical progreſſion*, and it has juſt been ſhewn, that the decreaſe muſt neceſſarily be accelerated by the action of the air and other ſurrounding cold bodies.

Whence the temperatures will be in geometrical progreſſion,

But the acceleration of the decreaſe of temperature in thoſe parts of the cylinder which are toward the cold extremity, depending on the action of the air and ſurrounding bodies, muſt be continually diminiſhing in proportion as the temperature of the ſurface of the cylinder approaches nearer and nearer that of the air; and hence we may conclude, that if a given number of points at equal diſtances from each other, be taken in the axis of the cylinder, the temperatures correſponding with theſe points will be *in geometrical progreſſion*.

We may repreſent the progreſs of the decreaſe of temperature by *Fig*. 2. *Pl.* VII.

—and may be repreſented by the logarithmic curve. Figure conſtructed.

In a right line A E, repreſenting the axis of the cylinder, if we take the three points B, C and D, ſo that the diſtances A B, B C, C D, and D E ſhall be equal; and, erecting the perpendiculars A F, B G, C H, D I, E K, take A F = the temperature of the cylinder at its extremity A, B G = its temperature at the point B, and ſo of the reſt; the ordinates A F, B G, &c. will be in geometrical progreſſion, while their correſponding abſciſſes are in arithmetical progreſſion; conſequently the curve P Q, which touches the extremities of all theſe ordinates muſt neceſſarily be the *logarithmic* curve.

Compariſon of the theory with the experiment.

We will now ſee, whether the reſults of experiment agree with the theory here exhibited or not.

To form our judgment with eaſe, and as it were at a ſingle glance, of the agreement of our theory with the reſults of the experiment, of which I gave an account at the beginning of this memoir, we have only to repreſent theſe reſults by a figure in the following manner.

Conſtruction of a figure exhibiting by a curve, and its ordinates the temperatures actually obſerved.

On the horizontal line A E, *Fig.* 3. repreſenting the axis of the cylinder employed in the experiment, we will take three points, B, C and D; one, C, in the middle of the axis, being the ſituation of the central thermometer, the other two, B and D, at the intermediate points which the other two thermometers occupied between the middle of the axis and its two extremities.

Erecting the perpendiculars A f, B g, C h, D i, and E k, on the points A, B, C, D and E; and taking the ordinate A f= 212, the temperature of boiling water; B g=162, the temperature indicated by the thermometer B; C h=$132\frac{3}{4}$, the temperature indicated by the thermometer C; D i=$106\frac{1}{2}$, the temperature given by the thermometer D; and laſtly, E k= 32, the temperature of water mixed with pounded ice; a curve, P Q, paſſing through the points f, g, h, i, k, ought to be the *logarithmic*; that is, ſuppoſing the temperature of the ſurrounding air to be conſtantly at the temperature of melting ice during the experiment.

The curve has a point of contrary flexure.

But the experiment in queſtion was made when the temperature of the air was at 78° F. conſequently, reckoning from a certain point, taken in the length of the cylinder, where the temperature was at 78°, to the extremity E, the influence of the ſurrounding air, inſtead of *cooling* the ſurface of the cylinder, *heated* it; and it is evident, that the curve P Q muſt neceſſarily in this caſe have a point of inflexion.

It is likewiſe irregular.

In fact it appears on a ſimple inſpection of the figure, that the curve P Q has a point of inflexion; but we ſee likewiſe, that this curve is not regular. That branch which is concave toward the axis of the cylinder is not ſimilar to the adjoining portion of the curve, of equal length, which is convex toward that axis; as it ought to be according to our theory; and even the part of the curve which is convex toward the axis A E, differs ſenſibly from the logarithmic, particularly toward its extremity P.

The deviation is too much to be owing to the defects of thermometers.

It ought neceſſarily to differ from this curve, *as far as the diviſions of our thermometers are defective*; but the deviation between the ordinates A *f* and B *g*, indicated by the reſults of the experiment in queſtion, appears to me much too conſiderable to be aſcribed to the imperfection of our thermometers.

It differs greatly from the logarithmic.

To ſee how far the curve P Q differs from the logarithmic, we have only to draw a logarithmic curve R S through the points g and *i*, and we ſhall find, that the ordinates correſponding to the points

	A,	B,	C,	D,	E.
Inſtead of being	212°	162°	132°¼	106½	32°
Will be -	199.55	162	131	106½	86.55
Difference -	−2·45	0	−1¼	0	+54·55

Aſcribed to water being a bad conductor of heat,

The very great difference that exiſts between the temperature of cold water, and that indicated by the reſults of the experiment for the extremity of the cylinder which was in contact with this water, led me to ſuſpect, that it was owing to the quality poſſeſſed by water in common with other fluids, which renders it *a very bad conductor of heat*.

—and the currents in the cold water being inconſiderable.

If it be true, as I believe I have elſewhere proved, that there is no ſenſible communication of heat between the adjacent particles of a fluid, from one to another; and that heat is propagated through fluids only in conſequence of a motion of their particles, reſulting from a change in their ſpecific gravity, occaſioned by their being heated or cooled: as the ſpecific gravity of water is very little altered by an inconſiderable change of temperature when this fluid is near the freezing point, it might have been foreſeen, that a ſolid body a little heated, and plunged into cold water, would be very ſlowly cooled.

Experiment to prove this.

The reſult of the following experiment, which I made with a view to elucidate this point, will put the fact out of a doubt.

When the cold water was briſkly ſtirred the thermometers were all greatly depreſſed.

The three thermometers being ſtationary, one, B, at 162°, the ſecond, C, at 132¼°, and the third, D, at 106½° the water in contact with one of the extremities of the cylin being ſtill boiling, while the water mixed with pounded which was in contact with the other extremity, was conſt at the temperature of melting ice, I began to ſtir this m of ice and water pretty briſkly with a little ſtick, and tinued to ſtir it uninterruptedly, and with the ſame ve, for two and twenty minutes.

I had ſcarcely begun this operation, when I had that my conjectures were well founded. The mercury in the three thermometers immediately began to deſcend, and did not ſtop till it had fallen very conſiderably.

Quantities of depreſſion.

The thermometer B fell from 162° to 152°; C from 132¾° to 111½°; and D from 106½° to 78½°.

On comparing theſe numbers we find, that, in conſequence of the agitation of the cold water for two and twenty minutes, the thermometer B fell 10° of Fahrenheit's ſcale, the thermometer C 21°, and the thermometer D 28°.

As ſoon as I had ceaſed to ſtir the cold water, the three thermometers began to riſe, and at the end of a quarter of an hour they had all reached the points from which they ſet out at the beginning of this operation.

Diagram to repreſent theſe effects.

To facilitate the compariſon of the reſults of theſe two experiments, one made with cold water at reſt, the other with the ſame water in a ſtate of conſtant agitation, I have repreſented them in *Fig.* 4.

Obſervations upon this experiment.

In the firſt place we ſhall learn ſeveral very intereſting facts by ſimple inſpection of this figure; we ſhall ſee, 1ſt. that the progreſs of refrigeration, or, to ſpeak more properly, *the decreaſe of temperature*, was every where much more rapid, when the cold water in contact with the extremity of the cylinder E was agitated when it was at reſt.

2dly. That the extremity of the cylinder in contact with this water was conſtantly near 30° colder in the firſt caſe than in the ſecond.

3dly. We ſhall ſee, that the progreſs of refrigeration was every where, and in both the experiments, ſuch nearly as our theory points out.

The decreaſe of temperature toward the middle of the cylinder was ſo regular, that it is more than probable the apparent irregularities toward the two extremities were occaſioned ſolely by the difficulty which a body of water finds in communicating its mean temperature to a ſolid, with which it is in ontact.

Agitation increaſed the effect of the boiling water likewiſe.

The boiling water being in continual motion owing to its llition, it had a great advantage over the cold water, which at reſt, in communicating its temperature to the extrey of the cylinder it touched; but I have found, notwithding this, that by agitating the boiling water ſtrongly with ill, and particularly when with the quill I made a rapid friction againſt the end of the cylinder immerſed in the boiling water, I occaſioned all the thermometers to riſe ſeveral degrees.

The difference between the experiment and the theory confirm its truth;

It may perhaps be imagined, at firſt ſight of the reſults of the experiment, that, as the three thermometers, which occupied the parts about the middle of the axis of the cylinder, did not indicate a decreaſe perfectly agreeing with the theory, the theory itſelf cannot be true: but a moment's reflection will ſhow, that this inference would be too haſty, and that the difference between the theory and the reſults of our experiments, far from proving any thing adverſe to the theory, ſerve on the contrary to render it more probable.

becauſe the ſcales of our thermometers are defective.

The reſults of ſuch experiments can never agree with the theory, except the diviſions of our thermometers be perfectly accurate: but it is well known to every one, who has any knowledge of natural philoſophy, that the diviſions of our thermometers are defective.

To improve this inſtrument is an object of importance.

One of the objects I had in view in the experiments, of which I have juſt given an account to the claſs, and in ſeveral others, which I intend to make without delay, is to improve the diviſion of the ſcale of the thermometer, in order to render this valuable inſtrument of greater utility in the delicate inveſtigations of natural philoſophy.

The air thermometer deſerves to be attended to.

It appears certain, that the increaſe of the elaſticity of air by heat is much more nearly proportionate to the increaſe of temperature, than the dilatation of mercury or any known fluid; conſequently it is the air thermometer we ought to endeavour to improve, and which muſt ultimately afford us the moſt accurate meaſure of heat, that it is poſſible for us to procure.

SECT. IV. *The Heat produced in a Body by a given Quantity of ſolar Light is the ſame whether the Rays be denſer or rarer, convergent, parallel, or divergent.*

Whether the quantity of heat generated by the ſolar rays be proportional to the light abſorbed.

In all caſes where the rays of the ſun ſtrike on the ſurface of an opake body without being reflected, heat is generated, and the temperature of the body is increaſed: but is the *quantity* of heat thus excited always in proportion to the quantity of light that has diſappeared? This is a very intereſting queſtion, and has not hitherto found a deciſive ſolution.

Whe

It does not follow from reaſoning that the *abſolute* power of the rays to produce heat can be increaſed by condenſing them.

When we conſider the prodigious intenſity of the heat excited in the focus of a burning mirror or a lens, we are tempted to believe, that the concentration and condenſation of the ſolar rays increaſe their power of exciting heat; but, if we examine the matter more cloſely, we are obliged to confeſs, that ſuch an augmentation would be inexplicable. It would be equally ſo on both the hypotheſes, which natural philoſophers have formed of the nature of light: for, as it has been proved both by calculation and experiment, that two undulations in an elaſtic fluid may approach and even croſs each other, without deranging either their reſpective directions or velocities, if light be analogous to ſound, we do not ſee how the concentration or condenſation of theſe undulations can increaſe their force of impulſe: and if light be a real emanation, as its velocity is not altered, either by the change of direction it undergoes in paſſing through a lens, or by its reflection from the ſurface of a poliſhed body, it ſeems to me, that the power of each of theſe particles to excite or impart heat, muſt neceſſarily be the ſame after refraction or reflection as before; and conſequently, that the heat communicated or excited muſt be, in all caſes, as the quantity of light abſorbed.

Experimental inveſtigation.

I have juſt made ſome experiments, which appear to me to eſtabliſh this fact beyond queſtion.

Two convex lenſes perfectly ſimilar were uſed,

Having procured from the optician Lerebours two lenſes perfectly equal, and of the ſame kind of glaſs, four inches in diameter, and of eleven and a half focus, I expoſed them at the ſame time to the ſun, ſide by ſide, about noon, when the ſky was very clear; and by means of two thermometers, or reſervoirs of heat, of a peculiar conſtruction, I determined the relative quantities of heat, that were excited in given times by the ſolar rays at different diſtances from the foci of the lenſes.

to throw the ſun's light upon flat tin boxes containing water and blackened on their ſurfaces.

The two reſervoirs of heat are a ſort of flat boxes of braſs filled with water. Each of theſe reſervoirs is three inches ten lines and a half in diameter, and ſix lines thick, well poliſhed externally on all ſides except one of its two flat faces, which was blackened by the ſmoke of a candle. On this face the ſolar rays were received in the experiments.

Each of theſe reſervoirs of heat weighs when empty 6850 grains, *poids de marc*, (near a pound troy), and contains 1210 grains of water (about 2 oz. 2 dwts.

Taking the capacity of braſs for heat to be to that of water as 0,11 to 1, it appears, that the capacity of the metallic box, weighing 6850 grains, is equal to the capacity of 622 grains of water; and adding this quantity of water to that contained in the box, we ſhall have the capacity of the reſervoir prepared for the experiments equal to that of 1932 grains of water.

The temperature of the water in each was ſhewn by a thermometer.

Each reſervoir is kept in its place by a cylinder of dry wood, one of the extremities of the cylinder being fixed in a ſocket in the center of the interior face of the reſervoir; and each reſervoir has a little neck, through which it is filled with water, and which after receives the bulb of a cylindrical thermometer, that reaches completely acroſs the inſide of the box in the direction of its diameter.

The two reſervoirs of heat, with their two lenſes, are firmly fixed in an open frame, which being moveable in all directions by means of a pivot and a hinge, the apparatus is eaſily directed toward the ſun, and made to follow its motion regularly, ſo as to keep the ſolar ſpectra conſtantly in the centers of the blackened faces of the reſervoirs.

Light admitted through equal apertures.

In order that the quantities of light paſſing through the two lenſes ſhould be perfectly equal, a circular plate of well poliſhed braſs, in the centre of which is a circular hole three inches and a half in diameter, is placed immediately before each of the lenſes.

When the reſervoirs of heat are placed at different diſtances from the focuſes of their reſpective lenſes, the diameters of the ſolar ſpectra, which are formed on the blackened faces of the reſervoirs, are neceſſarily different; and as the quantities of light are equal, its denſity at the ſurface of each reſervoir is inverſely as the ſquare of the diameter of the ſpectrum formed on that ſurface.

Experiment I.

Experiment. With equal apertures the ſolar ſpots from the lenſes were of 6 and of 24 lines diam.

In this experiment the reſervoir A was placed ſo near the focus of the lens, between the lens and the focus, that the diameter of the ſolar ſpectrum falling on it was only ½ an inch, or 6 lines, while the reſervoir B was advanced ſo far before the focus, that the ſpectrum was two inches in diameter, or 24 lines.

As the quantities of light falling on both were equal, the denſity of the light at the ſurface of the reſervoir A was to the denſity of that at the ſurface of the reſervoir B, as the ſquare of 24 to the ſquare of 6, or as 16 to 1.

The denſities of theſe equal quantities of light were therefore as 16 to 1;

I imagined, that, if the quantity of heat, which a given quantity of light is capable of exciting, depended any way on its denſity, as the denſities were ſo different in this experiment, I could not fail to diſcover the fact by the difference of time, which it would require to raiſe the two thermometers the ſame number of degrees.

Having continued the experiment more than an hour, on a very fine day, when the ſun was near the meridian and ſhone extremely bright, I did not find, that one of the reſervoirs was heated perceptibly quicker than the other.

but both the veſſels were heated in equal times.

Erperiment II.

I placed the reſervoir of heat A ſtill nearer the focus of the lens, in a ſituation where the ſolar ſpectrum was only 4¼ lines in diameter, and where blackened paper caught fire in two or three ſeconds; and I removed the reſervoir B ſtill farther from the focus, advancing it forward till the diameter of the ſpectrum was two inches three lines.

Experiment wherein the diameters of the ſpots were as 4¼ to 27.

The denſities of the light at the ſurfaces of the reſervoirs in this experiment were as 32 to 1.

The denſities of the light were as 32 to 1.

The temperature of the reſervoirs, as well as that of the atmoſphere, at the beginning of the experiment, was 54° F. = $9^\circ \frac{7}{9}$ R.

The reſervoir A, after having been expoſed to the action of very intenſe light near the focus of the lens for twenty-four minutes forty ſeconds, was raiſed to the temperature of 80° F. = $21^\circ \frac{1}{3}$ R.

The denſeſt light afforded rather leſs heat.

The reſervoir B, which was much farther from the focus of its lens, was raiſed to the ſame temperature, 80° F. a little more quickly, or in twenty-three minutes forty ſeconds.

To raiſe the temperature of the reſervoir A to 100° F. = $30^\circ \frac{2}{9}$ R. it was neceſſary to continue the experiment for one hour fifteen minutes ten ſeconds, reckoning from the commencement of it; but the reſervoir B reached the ſame temperature in one hour twelve minutes ten ſeconds.

The progreſs of this experiment from the beginning to the end is exhibited in the following table:

The general reſults tabulated.

Increaſes of Temperature.	Time taken By A.	Time taken By B.
From 54° to 80° F.	24′. 40″	23′. 40″.
80 85	7 45	7 30.
85 90	9 55	9 0
90 95	13 30	13 0
95 100	19 20	19 0
54 100	75 10	72 10

Time of the experiment.

This experiment was begun at 7 minutes 30 ſeconds after 11, and finiſhed at 22 minutes 40 ſeconds after 12, the ſky being perfectly clear during the time.

Hence light does not give more heat abſolutely by being condenſed.

On comparing all the reſults of this experiment, we ſee, that the reſervoir A, which was placed very near the focus, was more ſlowly heated than the reſervoir B, which was at a conſiderable diſtance from it *. The differences of time however taken to heat them an equal number of degrees were very trifling, and I think may be eaſily explained, without ſuppoſing the condenſation of light to increaſe (*qu.* diminiſh?) its faculty of exciting heat.

The rays were convergent in the preceding experiments.

In both the preceding experiments the ſolar rays ſtriking on the reſervoirs of heat were *convergent*, and they were even equally ſo on both ſides. To determine whether *parallel* rays have the ſame power of exciting heat as convergent rays, I made the following experiment.

Experiment III.

When one veſſel was expoſed to the parallel rays of the ſun without interception,

Having removed the lens from before the reſervoir B, I ſuffered the direct rays of the ſun to fall on the blackened face of the reſervoir, through the circular hole three inches and half in diameter in the round braſs plate, which had been conſtantly placed before that lens in the preceding experiments.

The reſervoir A was placed behind its lens as in the former experiments, and at the place where the ſolar ſpectrum had ſix lines diameter.

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* Did not the elevated temperature of the ſmaller ſurface ſuſtain its power of abſorbing heat, conformably to the known laws of heated bodies?—N.

Having

It was heated more quickly than the other by a ſpectrum of 6 lines or one-ſeventh part diam.

Having expoſed this apparatus to the ſun, I found, that the reſervoir B, on which the direct rays fell, was heated ſenſibly quicker than the reſervoir A, which was expoſed to the action of the concentrated rays near the focus of the lens.

The temperature of the apparatus and of the atmoſphere at the beginning of the experiment being 53° F. = 9° $\frac{1}{3}$ R. the reſervoir A required twenty-three minutes thirty ſeconds to raiſe it to the temperature of 80° F. = 21° $\frac{3}{9}$ R; but the reſervoir B, which was expoſed to the direct rays of the ſun, acquired the ſame temperature in eighteen minutes thirty ſeconds.

To reach the temperature of 100° F. = 30° $\frac{2}{9}$ R. took the reſervoir A one hour and three minutes, but the reſervoir B forty-ſeven minutes fifteen ſeconds only.

The following table will ſhow the progreſs of this experiment from the beginning to the end.

General reſults.

Increaſes of Temperature.		Time taken			
		By A.		By B.	
From 53°	to 65° F.	8′	26″	7′	0″
65	70	4	10	3	15
70	75	5	10	3	45
75	80	5	40	4	30
80	85	7	0	4	45
85	90	7	30	5	45
90	95	10	30	8	0
95	100	13	10	10	15
100	105	20	0	14	45
53	105	81	36	62	30

This difference aſcribed to light being loſt in paſſing through the lens.

As a conſiderable part of the light that fell on the lens before the reſervoir A, was loſt in paſſing through it, it is evident, that the quantity received by this reſervoir was leſs than that received by the reſervoir B, which was expoſed to the direct rays of the ſun; and we have ſeen, that the latter was heated more rapidly than the former.

This experiment is not deciſive: but

As we know not exactly how much light was loſt in paſſing through the lens, we cannot determine from the reſults of this experiment, whether convergent rays be more or leſs efficacious in exciting heat than parallel rays; but the difference in the times of heating was not greater, as it appears to me, than we might have expected to find it, ſuppoſing it to be occaſioned ſolely by the difference between the quantities of light acting on the reſervoirs.

The reſult of the following experiment will eſtabliſh this point beyond doubt.

Experiment IV.

Exp. 4 was made with equal apertures and ſpectra; but the one being *within* the focus was formed by convergent rays, and the other *without* by divergent rays.

Having replaced the lens belonging to the reſervoir B, I adjuſted this reſervoir to ſuch a diſtance between the lens and its focus, that the ſolar ſpectrum was one inch in diameter; and I placed the reſervoir A at the ſame diſtance beyond its focus.

As the quantities of light directed toward both were equal; and the diameters of the ſpectra, conſequently the denſities of the light that formed them, were alſo equal; there could be no difference between the reſults of the experiments with the two reſervoirs, except what was occaſioned by the difference in the *direction* of the rays that formed the ſpectra. On one hand theſe rays were *convergent*, and on the other *divergent*; and I had inferred, that if parallel rays were in reality leſs efficacious in exciting heat than convergent rays, as ſome philoſophers have ſuppoſed, *divergent* rays muſt be ſtill leſs efficacious than parallel rays, and conſequently much leſs than convergent rays.

No ſenſible difference occurred.

Having made the experiment with all poſſible care, I found no ſenſible difference between the quantities of heat excited in a given time by divergent and convergent rays.

The following are the particulars of the progreſs and reſults of this experiment:

General reſults of this laſt experiment.

Increaſes of Heat.	Time taken By A, with divergent Rays.	Time taken By B, with convergent Rays.
From 60° to 65° F.	4′ 50″	4′ 50″
65 70	4 55	5 0
70 75	5 27	5 25
75 80	6 13	6 15
60 80	21 25	21 30

From

Concluſion. The quantity of heat is always as the light abſorbed.

From the reſults of all the experiments, of which I have juſt given an account to the claſs, we may conclude, that the quantity of heat excited or communicated by the ſolar rays is always, and under all circumſtances, as the quantity of light that diſappears.

III.

Obſervations on blaſting Rocks; with an Account of an Improvement, whereby the Danger of accidental Exploſion is in a great Meaſure obviated. By Mr. William Close. *From the Author.*

To Mr. NICHOLSON.

SIR, *Dalton, Oct.* 14, 1805.

Practice of blaſting with ſand in Furneſs,

THE method of confining the force of gunpowder by a column of ſand in blaſting rocks, has been ſeveral years uſed in this part of Furneſs: At one time it was a very favourite practice; but at preſent, from the prejudices or indifference of workmen, or on account of the little danger attendant on working lime-ſtone in the common manner, it is leſs in repute.

noticed by the author elſewhere.

About two years ago, ſuppoſing this method not to be generally known, I drew up a ſhort account of it, and ſhould have ſent it to the Philoſophical Journal, had it not been connected with other miſcellaneous matter, which I had given to Mr. G. Aſhburner, the printer and proprietor of a new edition of Weſt's Antiquities of Furneſs, in which work the proceſs is deſcribed and recommended*.

Though

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* The paſſage alluded to is as follows, p. 393. "In breaking up the looſe rocks upon Baycliff Haggs, after the encloſure of that common, a method of employing ſea-ſand, for the purpoſe of confining the force of gunpowder in blaſting, was uſed, which does not appear to be generally known, though it was undoubtedly in uſe in other parts before it was adopted in Furneſs. The method is briefly this: After the excavation is made in the uſual manner with a borer, the charge of powder is poured in; and a priming-ſtraw of a proper length, filled with powder, is placed in the hole, having one of its ſides near the lower end ſo cut or thinned, that the

Improvement by Mr. Fiſher announced.

Though this method is undoubtedly worthy of much attention, and may often be employed with advantage; yet, when a ſtrong charge is required, the common mode of ſtemming muſt be frequently adopted: And as the danger in blaſting ſome kinds of rocks in this manner is very conſiderable, I am happy to notice an eaſy method of obviating one principal cauſe of accidental exploſion, which was communicated to me in converſation, a few days ago, by Mr. Thomas Fiſher, a reſpectable ſlate merchant in this town, who aſſures me it is infallible.

Cauſes of accidental exploſion. The principal is from the friction of the iron pricker in drawing.

The principal danger attendant on blaſting, does not conſiſt in ſtemming upon the charge of powder, but in the ſubſequent operation of drawing the iron rod, called the pricker, which makes the channel for the priming-ſtraw. For although the colliſion of the firſt fragments of ſtemming ſometimes produces an exploſion; yet this may be prevented by previouſly ramming a thick cap of paper, &c. upon the powder; by beating lightly upon the firſt pieces of ſtone that are thrown into the hole; or by uſing thoſe materials for ſtemming which are leaſt liable to give fire, ſuch as rotten ſtone, pieces of broken pots, or burnt clay. The pricker being hard preſſed againſt the rock, and in cloſe contact with the ſtemming, cannot be drawn out by hand, but muſt be ſtruck out by the hammer, a ſtrong piece of iron called a jumper being firſt placed in an eye or loop in the higheſt part of the rod, to receive the blows which are given in a proper direction to bring it out of its place. Now it frequently happens, that the friction of the loweſt part of the pricker againſt the rock fires the powder at the firſt or ſecond blow. When the exploſion happens at the commencement of ſtemming, the workman generally ſuſtains only a partial injury; but when in this part of the operation, when the powder exerts its whole force, and

the charge may partially communicate with the ſmall aſcending column contained in the ſtraw. After this, the remainder of the excavation is filled, by pouring in dry ſea-ſand; and the exploſion is given, by firing the priming-ſtraw in any of the various ways which are in common uſe.

"This method has been found to be equally effectual as ſtemming with any of the common materials; and where it can be uſed is certainly preferable: it is ſafer, ſimpler, and more expeditious."

diſperſes pieces of the ſhattered rock in various directions, his life is in the utmoſt danger, and his ſituation is truly terrible to contemplate.

Mr. F.'s expedient is a pricker made of copper, which is not liable to fire the powder.

Mr. Fiſher's improvement is to obviate this danger; and conſiſts in the uſe of a copper rod, or pricker, for making the hole that receives the priming-ſtraw, inſtead of one of iron, which before was every where employed in this part of the kingdom.

In our converſation Mr. F. obſerved, that ſome years ago, three exploſions happened on drawing the pricker, in the courſe of a fortnight, at his quarry in Kirkby Ireleth, and that one man being killed and two wounded, ſeveral of the workmen were ſo intimidated, that they reſolved to abandon a place which they conſidered as deſtined to daily misfortunes. It therefore became highly requiſite, on ſeveral accounts, to attempt ſome innovation for the ſecurity and encouragement of the workmen.

In meditating on the cauſe of theſe accidents, it appeared moſt rational to attribute them to the iron pricker giving fire by its friction againſt the rock, which was a hard blue rag, or whinſtone: and from this view of the cauſe it was inferred, that ſafety would accrue from the uſe of prickers conſtructed of thoſe metals which are leaſt diſpoſed to give fire with ſtone. Mr. Fiſher, therefore, determined to make trial of copper, and having procured ſome implements of this kind, found them to anſwer the purpoſe completely. It is now upwards of three years ſince this improvement was adopted, and as no exploſion has happened at the end of ſtemming in that period, at an extenſive work where accidents were frequent before, Mr. F. conſiders the means as almoſt infallible; and is happy to think that many ſad misfortunes have been thereby prevented.

Ample experience has proved its utility.

There are eleven ſlate quarries in Kirkby Ireleth, at ſeveral of which copper rods are now uſed; but at others they are not. At one of theſe a fatal accident happened a few months ago, from an exploſion upon drawing a rod of iron.

Prickers, ſuch as uſed by Mr. Fiſher, are eaſily conſtructed: A piece of copper being forged to the proper length, ſhape, and thickneſs for the body of the tool, is rivetted to an iron head or loop ſimilar to that of the common pricker. Theſe implements, when carefully uſed, are nearly as durable as thoſe of iron.

Advantage of ſtrong charges in firm rock.

Sand has not hitherto been uſed in blaſting at the ſlate quarries in Kirkby Ireleth. The maſters do not think it would ſucceed well in their work. I have frequently ſeen Mr. Fiſher uſe it in limeſtone rock near this town: He ſays it anſwers the beſt in deep holes, but thinks that ſand is more liable to be blown out than ſtemming. He alſo conſiders it as the moſt advantageous method of working, in driving levels, and blaſting in firm rock, to uſe ſtrong charges of powder, that the ſtone may be ſufficiently broken by the exploſion to be removed without much aſſiſtance from the hammer, the pick, or the lever: For thus the expedition of the work amply compenſates for the ſmall addition which is requiſite to a common charge of powder.

I am, Sir,

Your's reſpectfully,

WILLIAM CLOSE.

IV.

Deſcription of a portable Steam-engine, invented by Mr. SAMUEL CLEGG[*], *David Street, Mancheſter. Communicated by Mr.* DALTON, *Lecturer at the Royal Inſtitution, &c.*

Deſcription of a ſteam-engine.

THIS engine is worked by four copper valves in the uſual manner, but the mechaniſm for lifting them is very different from any hitherto made: there are no levers employed for opening the valves, and there is no hand gear. The ſteam and exhauſtion valves are on the ſame horizontal plane; thoſe which are vertical to each other are not like thoſe hitherto uſed, both expoſed to the ſteam or both to a vacuum; but by a ſimple contrivance in the conſtruction of the nozzles, the one is expoſed to the ſteam while the other has a communication with the condenſing veſſel. From what has been ſaid it may eaſily be perceived, if the two valves be connected together by a ſtraight rod, that when this rod is lifted, the preſſure is given to the piſton, and the machine is put into motion; and if the other two valves be connected in the ſame manner and lifted at an appointed time, the engine is kept in motion. The outſide appearance of theſe nozzles may be ſeen at *Fig.* 1, *c c*, (*Plate* IX.) The rods which come out of the bottom of the nozzles are kept tight by vertical ſtuffing-boxes, the whole of which is hid in the drawing by the frame.

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[*] Late apprentice to Meſſrs. Boulton and Watts, of Birmingham.

Deſcription of a ſteam-engine.

The next is a new contrivance for producing a rotative motion from a reciprocating one, which not only ſimplifies the machine very much, but exceeds the power of the common crank by nearly one-third, in conſequence of its acting always perpendicular to the radius of the wheel, which is done by a rack and wheel, as repreſented by *Fig.* 2 and 3; and as this plan of connection diſtributes the power uniformly, of courſe a much lighter fly-wheel is required, which diminiſhes friction, &c.

Explanation of the Plate.

Fig. 1. is a repreſentation of the engine: one of the corner columns A A, which ſupports the frame, ſerves likewiſe for an eduction-pipe and condenſing-veſſel: the air-pump E is joined to the condenſing veſſel by the pipe D; *e* is the piſton-rod, and though it works out at the bottom of the cylinder, it is as eaſily kept tight as if it worked out at the top; *b* is a ſimilar rod which keeps the rack perpendicular; *a a* are the two radius bars on which the braſſes are fixed that ſupport the ſhaft; by this contrivance the wheel C eaſily moves from one ſide to the other of the rack F.

Fig. 2. is a view of the rack on a larger ſcale, where C repreſents the wheel and D the ſhaft; E E, a ſliding-bar, on which is fixed the ſmall roller *o*, ſerving as a connecting link to keep the wheel C always in gear; for, when the wheel is in gear on the oppoſite ſide of the rack, the roller *o* is on the other ſide of the plate *a a*; but it will perhaps be more clearly underſtood by the plan, *Fig.* 3. where the letters repreſent the ſame movement as in the elevation, *Fig.* 2: This deſcription may be eaſily underſtood by thoſe who already poſſeſs a little knowledge of a ſteam-engine.

Mancheſter, Oct. 5, 1805.

V.

Letter from Mr. J. C. HORNBLOWER, *deſcribing the Framed Work by which the Roof of Clapham Church was raiſed to its original Situation, without diſturbing the Interior of the Building, &c.*

To Mr. NICHOLSON.

DEAR SIR,

Framed truſs by which the roof of Clapham church was raiſed, &c.

IT will be a pleaſure to you I know to record the productions of genius or fancy in your valuable Work, and therefore I have no heſitation in preſenting the incloſed for that purpoſe.

It is the invention of Mr. Watkin Bloore, one of the partners of Fothergal and Co. carpenters at Clapham, and was invented to raiſe the ſunk roof of Clapham church; and its application to the purpoſe intended, exhibits at once the means and the *end* that was to be accompliſhed; as by it the roof was *raiſed* and *ſecured* in the ſame proceſs, without incommoding the building with floors and ſcaffolds, which muſt have occaſioned conſiderable damage to the furniture of the church.

The ſhaded part of the drawing, *Plate* X. ſhews the truſs, and the lines behind it the conſtruction of the roof. The middle piece in the truſs marked A, is joggled into the king-poſt of the roof, and the two ſcrews put into action raiſe it up, and with it the whole of the middle or ſunk part of the roof; all which is eaſily comprehended by the drawing.

The drawing, *Fig.* 3, ſhews an improved mode of conſtructing the truſs, by the riders A A A A being framed over the principals B B, by which the raiſing ſcrews are more firmly ſupported in elevating the queen-poſts C C in the roof.

This muſt be a valuable experiment in the art of carpentry, which, conſidering how little ſcience of it falls to the lot of its poſſeſſors, cannot be too much regarded.

I am, Dear Sir,

Your very obedient ſervant,

J. C. HORNBLOWER.

VI.

Experiments on draining Land, by JOHN CHRISTIAN CURWEN, *Eſq. M. P. of Workington-Hall, in Cumberland, with an Engraving*[*].

DEAR SIR,

MUCH having been ſaid, in the public Papers, relative to draining, on the improved method of Mr. Elkington, I beg leave to offer you ſome obſervations reſpecting it, which have fallen under my notice, and which tend to prove it can be applied, with ſucceſs only, in ſuch parts of the kingdom, as have few, if any, interruption of the ſtrata. In order to make myſelf intelligible, it may not be improper to explain what is meant by interruptions of the ſtrata, or dykes and fiſſures, as they are denominated in mining countries. They are produced by the fracture or diſunion of the ſtrata, and conſiſt moſt commonly of the broken fragments of each ſuperior ſtrata; and towards the ſurface are of ſand, gravel, and ſtones, which ſeldom or never fail of affording conſiderable quantities of water. Theſe dykes may be approached within a few feet, and afford no water, as will be ſeen in two inſtances in the plan ſent you. No. 3 is a main drain, four feet deep, which paſſed within a few yards of A, an extreme wet place, and did not affect it. The perſon employed, ſuppoſed the water to be below him, and brought in a lower level No. 1, which likewiſe failed. No. 2 was then made ſtill lower, but with no better ſucceſs than No. 3, though with more advantage of level. As ſoon as it croſſed the dyke, I C, but before the level was brought up, not being deeper than the main drain, it got a conſiderable feeder. This proved that an interruption in the ſtrata prevented the water flowing into a drain, which was of a depth otherwiſe to have drawn it. Another example occurs in the ſame field, at letter B; which is a ſunk fence, four feet below the ſurface of the adjoining field, which was extremely wet within a few yards of the ſunk fence.

Mr. Elkington's method of draining is applicable only where the ſtrata are little interrupted.

Dykes and fiſſures.

Theſe interruptions prevent the draining off of the water.

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[*] From the twenty-ſecond volume of the Tranſactions of the Society of Arts; who awarded the gold medal to the author. The plan he refers to is at their houſe.

Experiments of drainage.

ſence. A lower level was ſuppoſed neceſſary to drain this water, and it was obtained at the dotted line. No water of any conſequence was got, till it was within a few yards of the ſunk fence, when a prodigious feeder was cut, and the head of the drain was not ſo deep at the time as the ſunk fence. Many inſtances to the ſame effect might be produced. In ſinking ſhafts in places much troubled with water, it is endeavoured, if circumſtances will permit, to get near a dyke, which ſerves as a barrier to the water; and if, in ſinking, the dyke be not croſſed, the water is kept clear off; but if otherwiſe, the water would be got at any depth, though not in ſuch quantities as when near the ſurface. The ſpot of ground, to which I have alluded, has above a dozen dykes, which may be traced from the out-burſts of water. They run in a direction of ſouth to north-weſt. I have made my drains eaſt and weſt. In one or two places, I was obliged to run a drain ſouth. This proceeds from an arm running from the dyke: but this ſeldom extends to any diſtance, and they gradually decreaſe till they end; and they rather make an interruption than a breakage of the ſtrata, as the ſtrata is the ſame on each ſide of it. In ſuch a country, Mr. Elkington could draw no more water than what lay in the uninterrupted ſtrata between any two of theſe dykes. The method of making the drain is explained by the engraving. I had twenty years ago drained this ground with ſtone drains, from 20 inches to two feet; but their direction having been moſtly from north to ſouth, and not ſufficiently deep, I had got little more than the day water. The feeder which I have now got, might be made applicable to many purpoſes. The drains are from two feet to nearly five feet deep. I have made 6000 yards in the laſt twelve months; the cutting from 14d. to 18d. per rod, filling 8d. ten and a half ſingle cart-loads of ſtones, at 9d. each, making the coſt 10s. per rod. The expence appears great; but fewer drains are required, and the work is effectually done. By reference to the plan, it will be ſeen that the direction of the drains not being able to draw the upper water, I was obliged to extend them. I would adviſe beginning at the higheſt level; for frequently that clears the whole, unleſs ſome dykes intervene in a contrary direction. I believe that all ſprings and outburſts of water proceed from dykes. The extent of theſe is various. Some may be traced for many miles, and their effects ſeen

The drains muſt be made to croſs the dykes.

Deſcription of the drains by reference to the drawing.

Springs of water proceed from dykes.

ſeen from the water that appears on the ſurface. Their origin is ſcarcely perceptible, and they thicken to many yards as they are approached. The ſtrata on both ſides have a more rapid riſe or dip, and are of a cloſer and harder texture. If theſe obſervations appear to you worthy of attention, you may make what uſe you think proper of them. I by no means wiſh to detract from Mr. Elkington's merit; but it is not generally applicable; and in counties where the ſtrata are much broken, Mr. Elkington's plan will be found to fail.

I am, Dear Sir,
Your obedient ſervant,
J. C. CURWEN.

Feb. 3, 1804.
Mr. Charles Taylor.

P. S. The higheſt drain is 120 feet above the level of No. 3.

A certificate from Mr. William Hoodleſs, farming agent, accompanied this letter, ſtating that upwards of ſix thouſand yards of drains had been cut, and completely filled, on the farm of John Chriſtian Curwen, Eſq.; that the firſt drains made according to that plan were done three years ago; and that they ſtand completely, and anſwer an admirable purpoſe.

Reference to Fig. 5, Plate XI. *of the Manner in which Mr.* CURWEN's *Drains are made.*

The loweſt part of the drain below E E is twelve inches wide.

E E 4 4 are the two ſide-ſtones of the drain, nearly four inches thick and nine inches high.

F 9 is the aperture for the water, nine inches high.

D, the flag or thin ſtone over the aperture, and which covers the ſide-ſtones of the drain.

C C, the body of the drain, filled with looſe ſtones till within nine inches of the ſurface.

B 9, the top of the drain, twenty-two inches wide and nine inches deep, filled with graſs ſod and ſoil.

VII.

Remarks on a Letter of Mr. DALTON, *concerning the Maximum Denſity of Water; with an Account of two Experiments of Dr.* HOPE, *tending to ſhew that it takes place at a Temperature above the freezing Point. In a Letter from* T. I. B.

To Mr. NICHOLSON.

SIR,

Reference to Mr. Dalton's letter.

IN No. 45 of your Journal, page 28, Mr. Dalton has publiſhed ſome remarks upon Count Rumford's experiments, relating to the maximum denſity of water, where he explains the riſing of the thermometer in the cup, by obſerving that it acquired heat by the proper conducting power of water. This, I ſhould think, is by no means probable; for the conducting power of water is not ſufficient to produce ſuch a rapid effect.

The circumſtances of the two thermometers by the ſide of the ball and cup, in the Count's two firſt experiments, I think are perfectly conſiſtent with his principle; the cup, very probably, did overflow, which might have been aſcertained by a thermometer placed below.

In the concluſion of this letter Mr. Dalton expreſſes a wiſh that Count R. or ſome one in poſſeſſion of a ſimilar apparatus, would repeat the Count's firſt experiment, with this difference, that the maſs of water ſhould be at 40° and the ball at 32°, in which caſe, he ſays, the thermometer would not be at all affected on the Count's principle; neither would it be affected (on Mr. D.'s principle) if the tenacity of the water counteracts the force of deſcent: and what concluſion could be drawn from ſuch a variation of the experiment?

Experiments of Dr. Hope.

The following experiment was made by Dr. Hope, profeſſor of chemiſtry in Edinburgh, to aſcertain the point at which water has the greateſt denſity,—and it appears to me to be perfectly deciſive.

He filled a jar with ice-cold water, and expoſed it to the air of a room at 52°: he ſuſpended in it two very delicate thermometers, one at half an inch from the bottom, and the other at the ſame diſtance from the ſurface of the water: the thermometer neareſt the bottom was firſt affected, and continued to riſe till it reached the temperature of 40°, when it became ſtationary: the thermometer at the ſurface roſe more ſlowly, but did not ſtop till it acquired the temperature of the room (52°).

Dr. Hope, to render this point ſtill clearer, expoſed water at 52° to the air of a room 32°; the reſult correſponded perfectly with the former experiment.

If you ſee no objection to the publication of this letter, by inſerting it in your Journal, you will oblige *,

Sir, your's,

T. I. B.

Edinburgh, Oct. 10, 1805.

VIII.

Obſervations and Conjectures relative to the ſuppoſed Welch Indians in the weſtern Parts of North America. Republiſhed from the "Kentucky Palladium," with additional Remarks and Conjectures, by the Editor of the Philadelphia Medical and Phyſical Journal †.

SIR,

No circumſtance relating to the hiſtory of the Weſtern Country, probably has excited, at different times, more general attention and anxious curioſity than the opinion, that a nation of white men, ſpeaking the Welch language, reſide high up on the Miſſouri. By ſome the idea is treated as nothing but the ſuggeſtion of bold impoſture and eaſy credulity; whilſt others regard it as a fact fully authenticated by Indian teſtimony and the report of various travellers worthy of credit.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

The fact is accounted for, they ſay, by recurring to a paſſage in the hiſtory of Great Britain, which relates, that ſeveral years after the diſcovery of America by Chriſtopher Columbus, a certain Welch prince embarked from his native country with a large party of emigrants; that after ſome time, a veſſel

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* A fuller account of the late experiments of Dr. Hope will be inſerted when the Edinburgh Tranſactions appear.

† Extracted from that Work, Vol. II. Part I.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

or two came back with the account that they had diſcovered a country far to the weſtward, and that they ſet ſail again with a freſh reinforcement, and never returned again any more.

The country which theſe adventurers diſcovered, it has been ſuppoſed, was the continent of North America; and it has been conjectured that they landed on the continent, ſomewhere in the Gulf of Mexico, and from thence proceeded northward, till they got out of the reach of the hoſtile natives, and ſeated themſelves in the upper country of Miſſouri.

Many accounts accordingly have been publiſhed, within the laſt thirty years, of perſons who, in conſequence either by accident or the ardour of curioſity, have made themſelves acquainted with a nation of men on the Miſſouri, poſſeſſing the complexion of Europeans and the language of Welchmen.

Could the fact be well eſtabliſhed, it would afford perhaps the moſt ſatisfactory ſolution of the difficulty occaſioned by a view of the various ancient fortifications with which the Ohio country abounds, of any that has ever been offered. Thoſe fortifications were evidently never made by the Indians. The Indian art of war preſents nothing of the kind. The probability too is that the perſons who conſtructed them were, *at that time*, acquainted with the uſe of iron: The ſituation of theſe fortifications, which are uniformly in the moſt fertile land of the country, indicates, that thoſe who made them were an agricultural people; and the remarkable care and ſkill with which they were executed, affords traits of the genius of a people, who relied more on their military ſkill than on their numbers. The growth of the trees upon them is very compatible with the idea that it is not more than three hundred years ago that they were abandoned.

Theſe hints however are thrown out rather to excite enquiry, than by way of advancing any decided opinion on the ſubject. Having never met with any of the perſons who had ſeen theſe white Americans, nor even received their teſtimony near the ſource, I have always entertained conſiderable doubts about the fact. Laſt evening, however, Mr. John Childs, of Jeſſamine County, a gentleman with whom I have been long acquainted, and who is well known to be a man of veracity, communicated a relation to me, which at all events appears to merit ſerious attention.

After he had related it in converſation, I requeſted him to repeat it, and committed it to writing. It has certainly ſome internal marks of authenticity. The country which is deſcribed was altogether unknown in Virginia when the relation was given, and probably very little known to the Shawnees Indians. Yet the account of it agrees very remarkably with later diſcoveries. On the other hand, the ſtory of the large animal, though by no means incredible, has ſomething of the air of fable, and it does not ſatisfactorily appear how the long period which the party were abſent was ſpent; though Indians are, however, ſo much accuſtomed to loiter away their time, that many weeks, and even months, may probably have been ſpent in indolent repoſe.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

Without detaining you any more with preliminary remarks, I will proceed to the narration, as I received it from Mr. Childs.

Maurice Griffith, a native of Wales, which country he left when he was about ſixteen years of age, was taken a priſoner by a party of Shawnees Indians, about forty years ago, near Voſſes Fort, on the head of Roanoke river in Virginia, and carried to the Shawnees nation. Having ſtaid there about two years and a half, he found that five young men of the tribe had a deſire of attempting to explore the ſources of the Miſſouri. He prevailed upon them to admit him as one of the party. They ſet out with ſix good rifles and with ſix pounds of powder a-piece, of which they were, of courſe, very careful.

On reaching the mouth of the Miſſouri, they were ſtruck with the extraordinary appearance occaſioned by the intermixture of the muddy waters of the Miſſouri and the clear tranſparent element of the Miſſiſſippi. They ſtaid two or three days amuſing themſelves with the view of this novel ſight: they then determined on the courſe which they ſhould purſue, which happened to be ſo nearly in the courſe of the river, that they frequently came within ſight of it as they proceeded on their journey.

After travelling about thirty days through pretty farming wood land, they came into fine open prairies, on which nothing grew but long luxuriant graſs. There was a ſucceſſion of theſe varying in ſize, ſome being eight or ten miles acroſs: but one of them ſo long that it occupied three days to travel

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

through it. In paſſing through this large prairie, they were much diſtreſſed for water and proviſions, for they ſaw neither beaſt nor bird; and, though there was an abundance of ſalt ſprings, freſh water was very ſcarce. In one of theſe prairies the ſalt ſprings ran into ſmall ponds, in which, as the weather was hot, the water had ſunk and left the edges of the ponds ſo covered with ſalt, that they fully ſupplied themſelves with that article, and might eaſily have collected buſhels of it. As they were travelling through the prairies they had likewiſe the good fortune to kill an animal, which was nine or ten feet high, and a bulk proportioned to its heighth. They had ſeen two of the ſame ſpecies before, and they ſaw four of them afterwards. They were ſwift-footed, and they had neither tuſks nor horns. After having paſſed through the long prairie, they made it a rule never to enter on one which they could not ſee acroſs, till they had ſupplied themſelves with a ſufficiency of jerked veniſon to laſt ſeveral days.

After having travelled a conſiderable time through the prairies, they came to very extenſive lead mines, where they melted the ore, and furniſhed themſelves with what lead they wanted. They afterwards came to two copper mines, one of which was three miles through; and in ſeveral places they met with rocks of copper ore as large as houſes.

When about fifteen days journey from the ſecond copper-mine, they came in ſight of white mountains, which, though it was in the heat of ſummer, appeared to them to be covered with ſnow. The ſight naturally excited conſiderable aſtoniſhment; but, on their approaching the mountains, they diſcovered that, inſtead of ſnow, they were covered with immenſe bodies of white ſand.

They had in the mean time paſſed through about ten nations of Indians, from whom they received very friendly treatment. It was the practice of the party to exerciſe the office of ſpokeſman in rotation; and when the language of any nation through which they paſſed was unknown to them, it was the duty of the ſpokeſman, a duty in which the others never interfered, to convey their meaning by appropriate ſigns.

The labour of travelling through the deep ſands of the mountains was exceſſive; but at length they relieved themſelves of this difficulty, by following the courſe of a ſhallow river, the bottom of which being level, they made their way to the top of the mountains with tolerable convenience.

After

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

After paſſing the mountains they entered a fine fertile tract of land, which having travelled through for ſeveral days, they accidentally met with three white men in the Indian dreſs. Griffith immediately underſtood their language, as it was pure Welch, though they occaſionally made uſe of a few words with which he was not acquainted. However, as it happened to be the turn of one of his Shawnees companions to act as ſpokeſman or interpreter, he preſerved a profound ſilence, and never gave them any intimation that he underſtood the language of their new companions.

After proceeding with them four or five days journey, they came to the village of theſe white men, where they found that the whole nation was of the ſame colour, having all the European complexion. The three men took them through their villages for about the ſpace of fifteen miles, when they came to the council-houſe, at which an aſſembly of the king and chief men of the nation was immediately held. The council laſted three days, and as the ſtrangers were not ſuppoſed to be acquainted with their language, they were ſuffered to be preſent at their deliberations.

The great queſtion before the council was, what conduct ſhould be obſerved towards the ſtrangers. From their fire-arms, their knives, and their tomahawks, it was concluded that they were a warlike people. It was conceived, that they were ſent to look out for a country for their nation; that if they were ſuffered to return, they might expect a body of powerful invaders; but that if theſe ſix men were put to death, nothing would be known of their country, and they would ſtill enjoy their poſſeſſions in ſecurity. It was finally determined that they ſhould be put to death.

Griffith then thought it was time for him to ſpeak. He addreſſed the council in the Welſh language. He informed them, that they had not been ſent by any nation; that they were actuated merely by private curioſity, they had no hoſtile intentions; that it was their wiſh to trace the Miſſouri to its ſource; and that they ſhould return to their country ſatisfied with the diſcoveries they had made, without any wiſh to diſturb the repoſe of their new acquaintances.

An inſtant aſtoniſhment glowed in the countenances, not only of the council, but of his Shawnees companions, who clearly ſaw that he was underſtood by the people of the country. Full confidence

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

confidence was at once given to his declarations, the king advanced and gave him his hand. They abandoned the deſign of putting him and his companions to death, and from that moment treated him with the utmoſt friendſhip. Griffith and the Shawnees continued eight months in the nation; but were deterred from proſecuting their reſearches up the Miſſouri by the advice of the people of the country, who informed them, that they had gone a twelve month's journey up the river, but found it as large there as it was in their own country.

As to the hiſtory of this people, he could learn nothing ſatisfactory. The only account they could give was, that their forefathers had come up the river from a very diſtant country. They had no books, no records, no writings. They intermixed with no other people by marriage, there was not a dark-ſkinned man in the nation. Their numbers were very conſiderable. There was a continued range of ſettlements on the river, for fifty miles, and there were within this ſpace three large water-courſes which fell into the Miſſouri, on the banks of each of which they were likewiſe ſettled. He ſuppoſed that there muſt be fifty thouſand men in the nation capable of bearing arms. Their cloathing was ſkins well dreſſed. Their houſes were made of upright poſts and the barks of trees. The only implement they had to cut them with, were ſtone tomahawks; they had no iron. Their arms were bows and arrows. They had ſome ſilver which had been hammered with ſtones into coarſe ornaments, but it did not appear to be pure. They had neither horſes, cattle, ſheep, hogs, nor any domeſtic nor tame animals. They lived by hunting. He ſaid nothing about their religion.

Griffith and his companions had ſome large iron tomahawks with them. With theſe they cut down a tree and prepared a canoe to return home in: But their tomahawks were ſo great a curioſity, and the people of the country were ſo eager to handle them, that their canoe was completed with very little labour. When this work was accompliſhed, they propoſed to leave their new friends: Griffith, however, having promiſed to viſit them again.

They deſcended the river with conſiderable ſpeed, but amidſt frequent dangers, from the rapidity of the current particularly when paſſing through the white mountains. When they reached the Shawnees nation, they had been abſent about two years and a half. Griffith ſuppoſed that when they travelled they went at the rate of about fifteen miles per day.

He ſtaid but a few months with the Indians after his return, as a favourable opportunity offered itſelf to him to reach his friends in Virginia. He came with a hunting party of Indians to the head-waters of Coal-river, which runs into New-river not far above the falls. There he left the Shawnees and eaſily reached the ſettlements on Roanoke.

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

Mr. Childs knew him before he was taken priſoner, and ſaw him a few days after his return, when he narrated to him the preceding circumſtances. Griffith was univerſally regarded as a ſteady honeſt man, and a man of ſtrict veracity. Mr. Childs has always placed the utmoſt confidence in his account of himſelf and his travels, and has no more doubt of the truth of his relation, than if he had ſeen the whole himſelf. Whether Griffith be ſtill alive or not he does not know.

Whether his ideas be correct or not, we ſhall probably have a better opportunity of judging on the return of Captains Lewis and Clark; who, though they may not penetrate as far as Griffith alledged that he had done, will probably learn enough of the country to enable us to determine whether the account given by Griffith be fiction or truth.

I am, Sir,
Your humble ſervant,
HARRY TOULMIN.

Frankford, Dec. 12, 1804.

Additional Obſervations and Conjectures by the Editor.

THE ſtory of a Welch colonization of America has excited much curioſity, both in Europe and the United States: By many it is believed, while by others it is thought unworthy of any attention. By reaſon of the preſent rapid progreſs of ſettlement in America, the time cannot be remote when the truth or falſity of this ſtory will be completely eſtabliſhed. In the meanwhile I do not heſitate to conjecture, *that no traces of the deſcendants of the Welch prince will ever be diſcovered in the weſtern parts of North America.*

It

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

It may not be improper to notice the tale upon which ſo many perſons in Europe at leaſt reſt their hopes of proving in the moſt ſatisfactory manner, that the Welch have contributed to the peopling of America.

David Powel, a Welch hiſtorian, informs us, that on the deceaſe of Owen Guyneth, king of North-Wales, a diſpute aroſe among his ſons concerning the ſucceſſion to the crown; and that Madoc or Madog, one of the ſons, "weary of this contention, betook himſelf to ſea, in queſt of a more quiet ſettlement *." We are informed, that "he ſteered due weſt, leaving Ireland to the north, and arrived in an unknown country, which appeared to him ſo deſirable, that he returned to Wales, and carried hither ſeveral of his adherents and companions. After this neither Madog nor his companions were ever heard of more. The voyage of Madog is ſaid to have been performed about the year 1170.

I have not ſeen Powel's work, but I learn, that this hiſtorian, who lived in the reign of Queen Elizabeth, and conſequently at a great diſtance of time from the event which he records, adduces no better authority in ſupport of the voyage than a quotation from a Welch poet, "which proves no more than that he (Madog) had diſtinguiſhed himſelf by ſea and land †." Some few Welch words, ſuch as *gwrando*, to hearken or liſten, &c. are very feebly or unfortunately adduced by Powel, as circumſtances favourable to the truth of the Welch emigration.

When we conſider "that the Welch were never a naval people; that the age in which Madog lived was peculiarly ignorant in navigation;" that the compaſs was then unknown; the ſtory of the voyages of the Welch prince muſt I think be conſidered as extremely improbable. I am of opinion with Mr. Pennant, that "the moſt which they could have attempted muſt have been a mere coaſting voyage."

But it may be ſaid, we muſt appeal to facts; and that independently of the verſes of the Welch poet, and the arguments of the Welch hiſtorian, it ſeems highly probable that a colony of white people who ſpeak the Welch language, does actually exiſt in the weſtern parts of North America.

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* Dr. Robertſon.

† Pennant's Artic Zoology, Introduction, p. cclxiii. &c.

I cannot,

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

I cannot, I muſt confeſs, adopt this opinion. I readily allow, that the relations publiſhed by Mr. Toulmin and many other perſons, both in Europe and in America, are extremely curious. But theſe relations are very inconſiſtent with one another, particularly in what relates to the actual ſtate of improvement of the ſuppoſed Welſhmen. By ſome we are told they are very far advanced in improvement; by others that their improvement is not at all greater than that of the Red-men or Indians of America. At one time, they are ſaid to be in poſſeſſion of manuſcripts (and even printed books) at another time nothing of this kind is found among them. It muſt be confeſſed that Maurice Griffith's relation is, in ſeveral reſpects, more plauſible than that of any preceding traveller; but it is not unincumbered with inconſiſtencies, which I do not deem it neceſſary to notice in this place. His aſſertion "that the white men of the Miſſouri ſpeak pure Welch," even though this aſſertion be qualified by the obſervation that "they occaſionally make uſe of a few words with which he was not acquainted," is to me one of the moſt improbable things that have ever been related of theſe people. His ſilence about their religion is altogether inexcuſable. One would ſuppoſe that a perſon of Griffith's inquiſitive turn of mind, would hardly have omitted to make ſome inquiries reſpecting the religious inſtitutions of a people, whom he conſidered as his countrymen. If theſe people be the deſcendants of Madog, *ſome* traces of the Chriſtian religion may be expected to be diſcerned among them; for I think it requires many centuries to entirely efface from the memory of a people all veſtiges of their religion, eſpecially from a people ſo tenacious of their language, and ſo little diſpoſed to intermix with their neighbours, as the Welch Indians are repreſented to be.

But Griffith's relation is, I think, worthy of ſome attention. I even think it not altogether improbable that future reſearches will eſtabliſh the fact, that there does exiſt in the weſtern parts of North America a race or nation of men, whoſe complexion is much fairer than that of the ſurrounding tribes of Indians, and who ſpeak a language abounding in Welch or Celtic words. But the *complete* eſtabliſhment of theſe two points would not prove the eſtabliſhment of the truth of the aſſertion, that Prince Madog had ever made a voyage to America, or that

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

that a colony of Celts had at any period prior to the *diſcovery of America by Columbus*, paſſed into this hemiſphere *from Britain*.

It may be thought, from the ſtatement publiſhed by Dr. Williams and ſome other writers on the ſubject, that the belief of the exiſtence of a race of Welch Indians in America is generally admitted by the Welch Indians and others. But this is far from being the caſe. The late Mr. M'Gilivray, a man of no inconſiderable powers of mind, and whoſe curioſity was by no means confined to his own relatives, the Muſcohge, or Creek Indians, informed me, in the year 1790, that he knew nothing of the exiſtence of any white people in the tract of country beyond the Miſſiſſippi.

The following is an extract of a letter (dated *Downing, June* 14, 1792) from my learned and excellent friend the late Mr. Thomas Pennant of Wales.

" My countrymen are wild among the Padoucas, or Welch Indians, deſcendants of Madog, now ſeated about the upper parts of the Miſſouri. I am rather in diſgrace, not having the warmeſt hopes of their diſcovery. Pray what is your opinion and that of your philoſophers ?"

In anſwer of the above I wrote a letter, of which the following is a part:

" I have heard a great deal about the Welch Indians. I very early imbibed your opinion, as delivered in your Arctic Zoology [*], and mentioned you on the ſubject in a little work [†] which I publiſhed in England at the age of * * * *. I do *not* know whether you have ſeen that work. I do not mean to hint that it is worthy of your attention. I certainly think there is ſome foundation for the ſtory; but I have no doubt but the whole affair will turn out very different from a diſcovery of Madog's deſcendants in America.

" I have ſaid, that I think there is ſome ground for the ſtory. I ſhall explain myſelf. You know that many of the firſt viſitors of the new world were ſtruck with the reſemblance which

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[*] See the introduction to the work, pages 263, 264.

[†] Obſervations on ſome parts of natural hiſtory; to which is prefixed an account of ſeveral remarkable veſtiges, of an ancient date, which have been diſcovered in different parts of North America. Part I. London, 1797.

ſubſiſts

Traditional account of inhabitants of America ſuppoſed to have originated from Wales.

ſubſiſts between ſome of the American nations and the Jews. Some Hebrew words were found in this continent, as they have been every where elſe. The Americans were now ſaid to be the deſcendants of the Jews, and Adair laboured very hard to prove the matter in a ponderous quarto which few people read, becauſe it is big with ſyſtem and extravagance, though, indeed, it contains ſome curious and accurate matter. In like manner, in the languages of ſome of the American tribes there are found ſome words which are a good deal analogous to words in the languages of the ancient Celts. Waſer, who was a very reſpectable obſerver, if we conſider his occupation in life, mentions the coincidence he found between the language of the Indians of Darien and that of the Highland Scots; and I could produce inſtances of their coincidence. Some Greek words are alſo found in certain of the American languages. I would not ſtrain a point ſo much as ſome writers have, who mention the coincidence which ſubſiſts between the Greek *Theos* and the Mexican *Teotl.* The word *Potowmack,* which is the name of one of our great rivers, is a good deal like the Greek *Potomos* ‡. Theſe words (perhaps they are accidental reſemblances) have given riſe to ſome of the numerous theories which we have had concerning the peopling of this great continent: and I doubt not that ſome * * * * or perſon who underſtood the Welch language, finding Celtic words (a language ſpoken by the Welch) among the Americans, in the fulneſs of his zeal would bring his countrymen among the Padoucas, Apaches, &c.

" Such, I believe, has been the origin of this wonderful ſtory. I preſume, that, were an ignorant Highlander to viſit the Darien Indians, or ſome other American tribes, he would fancy himſelf among his countrymen, whom painting, expoſure to the ſun, &c. he might ſuppoſe had exalted or degraded to their preſent tinge. I lately converſed with an old Highlander, who ſaid, that the Indians ſpeak the Highland language. Some Highland words were mentioned by him;

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‡ The Abbé Molina (in his Compendio de la Hiſtoria Civil del Reyno de Chile, &c. Parte Segunda, p. 334, 335.) has pointed out ſome very ſtriking inſtances of reſemblance between the Greek and Chileſe languages. He has alſo pointed out ſome reſemblance between the Latin and the Chileſe.—February 19, 1805.

**** one word **** I recollect, the word *teine*, which in the Highland language, he ſaid, ſignifies fire: now our Delaware Indians call fire *teriday*; the reſemblance in ſound is certainly not ſmall. The Celts have, undoubtedly, been very widely ſpread over the globe: I believe they exiſted in this country, and that their deſcendants are ſome of the preſent tribes[*]. That Celtic words ſhould be found among the Americans, when Celtic words are to be found almoſt every where elſe, is not I think to be wondered at."

IX.

Account of an improved Sheep-Fold, contrived and conſtructed by THOMAS PLOWMAN, *Eſq. of Broom in Norfolk, and communicated by him to the Society for the Encouragement of Arts*[†].

Advantages of the new ſheep-fold.

THE model of Mr. Plowman's Sheepfold was forwarded to the Secretary of the Society of Arts laſt year with a letter deſcribing its properties and conſtruction. It is made on an improved and very ſimple principle, combining many advantages over the old and expenſive method of folding by hurdles; and as the whole fold can be removed with eaſe at all times, it is found peculiarly uſeful in feeding off turnips on the land in froſty weather, when hurdles cannot be uſed; and, as the ſaving of labour in agriculture is a leading object, he has no doubt of ſeeing it, in a very few years, generally adopted.

Durability.

The expence, in the firſt inſtance, will exceed that of hurdles, for the ſame given quantity of ſheep; but having had one in uſe nearly three years, he is ſatisfied the ſaving will be very conſiderable: for, before he adopted this method of folding, he loſt from thirty to forty nights folding in the year, owing to the land being hard in dry ſeaſons, ſuch as the

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[*] Very conſiderable fragments of the Celtic dialects are ſtill preſerved in America; particularly, if I do not miſtake, among the Ranticokes and the Katalba or Katawbas. *February* 19, 1805.

[†] The Society awarded the gold medal for this uſeful improvement, and inſerted his account.

two laſt; which renders folding almoſt impracticable, as they never can be ſet without great labour and deſtruction of hurdles. He is alſo clearly of opinion, that the ſtock of ſheep will be greatly increaſed when this method of folding becomes more known; and that it will enable many ſmall farmers to keep from 50 to 100 ſheep, who now are deterred from it, on account of the ſmall quantity of feed they have, not anſwering to keep a man for that purpoſe only; but by this plan, they may keep a boy at 3s. or 3s. 6d. per week, who can attend on 100 or 200 ſheep, and move the fold himſelf without any aſſiſtance. In heavy gales of wind it frequently happens that hurdles are blown down, and the ſheep, of courſe, being at liberty to range over the crops, do incalculable miſchief; which cannot happen with this fold.

Saving of hurdles; and

greater profit in feeding and keeping ſheep.

It is eaſily moved, and not liable to be blown down;

In ſome counties in England, where hogs are folded, great difficulties are experienced for want of ſtowage, for them to feed off winter tares, &c. &c. as they root up every ſtake or hurdle; but from having tried the experiment, the inventor is certain his fold will keep them in, and defies their attempts to diſplace it.

reſiſts hogs.

From this drawing, which correſponds with the model, and from the deſcription, it is ſeen that an aſtoniſhing quantity of time is ſaved; for one man can remove a fold to contain 300 ſheep with eaſe in five minutes, which, by the old method, frequently takes ſome hours to accompliſh.

Certificates of gentlemen, who uſe theſe new folds, were ſent to the ſociety, among whom is that of his Grace the Duke of Bedford.

When the fold is wanted to be uſed on very hilly ground, it is beſt to begin at the top, and work it down to the bottom, for the eaſe of removing it, and then draw it up again with a horſe. This, however, the inventor has never had occaſion to do; for the land in his county is ploughed in a contrary direction, and the fold is worked in the ſame courſe as the ridges. By this mean, the inconvenience is avoided of croſſing the furrows, and they are alſo a guide to keep the fold in a ſtraight direction.

Method of placing it on hilly grounds.

With reſpect to the ſheep getting under, he does not recollect that circumſtance to have ever happened, nor does he conceive that any land, which is cultivated can be ſo uneven as to admit of it.

Deſcription with reference to the drawing.

Deſcription of the Sheepfold.

Plate XI. Fig. 1. Shows one diviſion or part of this fence twenty-one feet long, and three feet eleven inches high, compoſed of the following parts:

A. A top rail three inches deep and two inches thick. B. The upper bar, three inches deep, and three-quarters inch thick. CC, The two lower bars, four inches by three-quarters of an inch, which, with the upper bar, are morticed through the uprights. DDDD, Which uprights are oak, three inches by two inches. E, The lower bar, three inches by three. F. An upright bar, with the horizontal bars halved into it. GG, Two oak uprights, three by two inches.

Fig. 2. Shows the oak uprights GG. H, The axletree, three inches by three, and three feet between the wheels. I, An oak knee, which connects the uprights GG with the axletree, by means of two ſcrews and nuts.

Fig. 3. A plan, in which the axle H is ſhown with two arms KK at right angles to H, which are made to act as pivots to the wheels, when intended to be moved in a direction at right angles to the bars.

Fig. 4. Is a view of the ſame parts deſcribed in fig. 3. The wheels marked W, in all the figures, are of caſt iron, and coſt 3s. 6d. each.

X.

Anecdotes of an American Crow. By William Bartram*.

Anecdotes of a crow.

It is a difficult taſk to give a hiſtory of our crow. And I heſitate not to aver, that it would require the pen of a very able biographer to do juſtice to his talents.

Before I enter on this ſubject minutely, it may be neceſſary to remark, that we do not here ſpeak of the crow collectively, as giving an account of the whole race, ſince I am convinced that theſe birds differ as widely as men do from each other in point of talents and acquirements, but of a particular kind of that ſpecies, which I reared from the neſt.

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* From the Philadelphia Medical Journal, Vol. I. part I.

He

Anecdotes of a crow.

He was, for a long time, comparatively, a helpleſs dependant creature, having a very ſmall degree of activity or vivacity, every ſenſe ſeeming to be aſleep, or in embryo, until he had nearly attained his finiſhed dimenſions and figure, and the uſe of all his members. Then we were ſurpriſed and daily amuſed with the progreſſive developement of his ſenſes, expanding and naturating as the wings of the youthful phalæna, when diſengaged from its nympha ſhell.

Theſe ſenſes however, ſeemed, as in man, to be only the organs or inſtruments of his intellectual powers, and of their effects, as directed towards the accompliſhment of various deſigns and the gratification of the paſſions.

This was a bird of a happy temper and good diſpoſition. He was tractable and benevolent, docile and humble, whilſt his genius demonſtrated extraordinary acuteneſs and lively ſenſations. All theſe good qualities were greatly in his favour, for they procured him friends and patrons, even among *men* whoſe ſociety and regard contributed to illuſtrate the powers of his underſtanding. But what appeared moſt extraordinary, he ſeemed to have the wit to ſelect and treaſure up in his mind, and the ſagacity to practice, that kind of knowledge which procured him the moſt advantage and profit.

He had great talents, and a ſtrong propenſity to imitation. When I was engaged in weeding in the garden, he would often fly to me, and after very attentively obſerving me in pulling up the ſmall weeds and graſs, he would fall to work, and with his ſtrong beak pluck up the graſs; and the more ſo, when I complimented him with encouraging expreſſions. He enjoyed great pleaſure and amuſement in ſeeing me write, and would attempt to take the pen out of my hand, and my ſpectacles from my noſe. The latter article he was ſo pleaſed with, that I found it neceſſary to put them out of his reach when I had done uſing them. But one time, in particular, having left them a moment, the crow being then out of my ſight, recollecting the bird's miſchievous tricks, I returned quickly and found him upon the table, rifling my inkſtand, books, and paper. When he ſaw me coming, he took up my ſpectacles and flew off with them. I found it vain to pretend to overtake him; but ſtanding to obſerve his operations with my ſpectacles, I ſaw him ſettle down at the root of an apple-tree, where, after amuſing himſelf for awhile, I obſerved

Anecdotes of a crow.

that he was hiding them in the graſs, and covering them with ſticks and chips, often looking round about to ſee whether I was watching him. When he thought he had ſufficiently ſecreted them, he turned about, advancing towards me at my call. When he had come near me, I ran towards the tree to regain my property. But he judging of my intentions by my actions, flew, and arriving there before me, picked them up again, and flew off with them into another apple tree. I now almoſt deſpaired of ever getting them again. However I returned back to a houſe a little diſtance off, and there ſecreting myſelf, I had a full view of him, and waited to ſee the event. After ſome time had elapſed, during which I heard a great noiſe and talk from him, of which I underſtood not a word, he left the tree with my ſpectacles dangling in his mouth, and alighted with them on the ground. After ſome time, and a great deal of caution and contrivance in chooſing and rejecting different places, he hid them again, as he thought, very effectually in the graſs, carrying and placing over them chips, dry leaves, &c. and often puſhing them down with his bill. After he had finiſhed this work, he flew up into a tree hard by, and there continued a long time talking to himſelf and making much noiſe; bragging, as I ſuppoſe, of his achievements. At laſt he returned to the houſe, where not finding me, he betook himſelf to other amuſements. Having noted the place where he had hid my ſpectacles, I haſtened thither, and after ſome time recovered them.

This bird had an excellent memory. He ſoon learned the name which we had given him, which was Tom; and would commonly come when he was called, unleſs engaged in ſome favourite amuſement, or ſoon after correction; for when he had run to great lengths in miſchief, I was under the neceſſity of whipping him, which I did with a little ſwitch. He would in general bear correction with wonderful patience and humility, ſupplicating with piteous and penitent cries and actions. But ſometimes when chaſtiſement became intolerable, he would ſuddenly ſtart off, and take refuge in the next tree. Here he would conſole himſelf with chattering and adjuſting his feathers, if he was not lucky enough to carry off with him ſome of my property, ſuch as a pen knife, or a piece of of paper; in this caſe he would boaſt and brag very loudly. At other times he would ſoon return, and with every token of

penitence

Anecdotes of a crow.

penitence and ſubmiſſion approach me for forgiveneſs and reconciliation. On theſe occaſions he would ſometimes return and ſettle on the ground near my feet, and diffidently advance with ſoft ſoothing expreſſions, and a ſort of circumlocution, and ſit ſilently by me for a conſiderable time. At other times he would confidently come and ſettle upon my ſhoulder, and there ſolicit my favour and pardon with ſoothing expreſſions and careſſing geſticulations; not omitting to tickle me about the neck, ears, &c.

Tom appeared to be influenced by a lively ſenſe of domination (an attribute prevalent in the animal creation) but nevertheleſs his ambition, in this reſpect, ſeemed to be moderated by a degree of reaſon or reflection. He was certainly by no means tyrannical or cruel. It muſt be confeſſed, however, that he aimed to be maſter of every animal around him, in order to ſecure his independence and his ſelf preſervation, and for the acquiſition and defence of his natural rights. Yet in general he was peaceable and ſocial with all the animals about him.

He was the moſt troubleſome and teazing to a large dog whom he could never conquer. This old dog from natural fidelity and a particular attachment commonly lay down near me when I was at reſt, reading or writing under the ſhade of a pear-tree in the garden near the houſe. Tom (I believe from a paſſion of jealouſy) would approach me with his uſual careſſes and flattery, and after ſecuring my notice and regard, he would addreſs the dog in ſome degree of complaiſance, and by words and actions; and if he could obtain acceſs to him, would tickle him with his bill, jump upon him, and compoſe himſelf for a little while. It was evident, however, that this ſeeming ſociability was mere artifice to gain an opportunity to practice ſome miſchievous trick, for no ſooner did he perceive the old dog to be dozing, than he would be ſure to pinch his lips, and pluck his beard. At length, however, theſe bold and hazardous achievments had nearly coſt him his life, for one time the dog being highly provoked, he made ſo ſudden and fierce a ſnap, that the crow narrowly eſcaped with his head. After this Tom was wary, and uſed every caution and deliberation in his approaches, examining the dog's eyes and movements, to be ſure that he was really aſleep, and at laſt would not venture nearer than his tail, and then by ſlow, ſilent, and wary ſteps, in a ſideways or oblique manner, ſpreading his legs and reaching forward. In this poſition he would pluck the long hairs of the dog's tail. But he would always take care to place his feet in ſuch a manner to be ready to ſtart off when the dog was rouſed and ſnapped at him.

It would be needleſs (obſerves my ingenious friend in the concluſion of this entertaining account of the crow) to recount inſtances of this bird's underſtanding, cunning, and operations, which certainly exhibit inconteſtible demonſtrations of a regular combination of ideas, premeditation, reflection, and contrivance, which influenced his operations.

XI.

An Account of the Seiches of the Lake of Geneva.

By M. VAUCHER *.

Sudden and irregular riſe and fall of the lake of Geneva called ſeiches.

THE inhabitants of the banks of the lake of Geneva, deſignate by the name of ſeiches certain ſudden and irregular changes which take place in the level of the waters of the lake, and have no relation with the regular and annual increaſe produced by the melting of the ſnows. This phenomenon was deſcribed at the beginning of the laſt century. Fatio de Duilers in the 2nd vol. of Spon's Hiſtory of Geneva; and afterwards by Jalabert in the Academy of Sciences; Serre in the Journal de Savans, Bertrand, and by De Sauſſure in the 1ſt vol. of his Travels in the Alps. But though ſeveral of theſe philoſophers have attempted to explain the fact, as we ſhall hereafter remark, yet no one has conſidered it with preciſion, and as a general phenomenon. The editors of the Bulletin des Sciences, from whoſe excellent ſheet I take the preſent account, have followed Mr. Vaucher, and afterwards preſent the different explanations. The numerous obſervations of that philoſopher have led him to the following general reſults.

Particular detail of the facts; they are obſerved in other lakes.

1. The ſeiches are not peculiar to the lake of Geneva, they are alſo obſerved in the Lakes of Conſtance, Zurich, Annecy, Neuf-chatel, and in the lake Major, and there are ſtrong reaſons to think that they exiſt in moſt lakes, though they may not have been ſufficiently obſerved.

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* From the Bulletin des Science, No. 96.

2. It appears, however, to be true, that the phenomenon is more remarkable in the lake of Geneva than any where elſe that it has been obſerved. In fact, the level of the waters of Leman lake have been ſeveral times obſerved to riſe at a given place in the courſe of 15 or 20 minutes, three, four, and even five feet, and to ſubſide ſome time afterwards, whereas the ſtrongeſt ſeiches obſerved in other lakes, have been four or five inches in the lake of Conſtance, eighteen lines in that of Zurich, four or five lines in that of Annecy, and only a few lines in the lake of Neuf-Chatel and lake Major.

But moſt ſtrikingly in the lake Leman,

3. In all theſe lakes, particularly in that of Geneva, the ſeiches are moſt ſenſible in that part of the lake which is neareſt the outlet of its waters. Accordingly they are no more than one or two inches, at the diſtance of two leagues from Geneva, and at the extremity near where the lake receives its waters the ſeiches of the lake of Geneva are not ſtronger than thoſe of the other lakes here mentioned.

More conſiderable near the place of efflux,

4. In theſe different lakes they are moſt ſenſible in places where the lake is remarkably narrow.

and where the ſhores are not far aſunder;

5. The ſeiches may take place indifferently at all ſeaſons of the year, and at any hour of the day; but in all the lakes it has been obſerved, that they are more frequent in the day than in the night, and in the ſpring and autumn, than in the winter or ſummer.

they happen at all times and ſeaſons;

6. It has been obſerved in particular in the neighbourhood of Geneva, that the ſtrongeſt ſeiches take place at the end of the ſummer, that is to ſay, at the time of the greateſt elevation of its waters.

but moſt ſtrikingly when the waters are higheſt;

7. The ſeiches are extremely frequent, but they are uſually a few lines, or at moſt only a few inches, in which caſes they cannot be perceived without exact apparatus to obſerve the level of the lake. It is from a want of this obſervation that they have been ſuppoſed to be very rare, as thoſe ſeiches only could be obſerved without apparatus which varied ſeveral feet.

8. The ſeiches take place without any agitation or motion of undulation or current in the ſurface of the fluid.

attended with no agitation,

9. Their duration is very variable, ſeldom exceeding twenty or twenty-five minutes, and often much leſs.

and do not laſt long;

10. This

they ſeem to be dependent on the weight of the atmoſphere, and are thought to foretel rain.

10. This phenomenon takes place in all temperatures, but in general it reſults from very extenſive tables, that the ſeiches are more frequent, and more extreme, the more variable the ſtate of the atmoſphere. Remarkable variations of the barometer have been obſerved to correſpond with conſiderable ſeiches, and it is an opinion generally received among the fiſhermen, that the ſeiches are a ſign of change of weather. In particular, they have been obſerved to be very ſtrong when the ſun comes to ſhine very ſtrongly on a ſpot, a ſhort time before obſcured by a thick cloud.

Explanations by various authors.

After this expoſition of the phenomenon, ſome notion may be formed reſpecting the value of the different explanations. M. Fatio attributes the ſeiches to violent guſts of wind which drive the waters into the narroweſt part of the lake. Mr. Jalabert attributes them to ſome ſudden encreaſe of the Arve, which falling into the Rhone at a ſhort diſtance from the lake, and entering the river at a conſiderable angle, may in fact, ſometimes ſtop its courſe for a ſhort period, and in that manner raiſe the waters of the part of the lake neareſt Geneva; laſtly, Mr. Bertrand thinks this phenomenon to be occaſioned by electrical clouds which attract the waters of the lake, and produce oſcillations more ſenſible, the nearer its oppoſite banks may be to each other. Without dwelling on the inſufficiency of theſe three hypotheſis to account for all the different facts before mentioned; Mr. Vaucher obſerves, that the true explanation ought to be two-fold; namely, general in order to ſhew the cauſe of thoſe leſs conſiderable ſeiches which are obſerved in all the lakes, and over the whole of their ſurface; the other muſt be local, and explain why this phenomenon is much more ſenſible at the weſtern extremity of the lake of Geneva, than in any other known place.

Mr. Vaucher aſcribes them to atmoſpheric preſſure acting more ſtrongly on one part of the lake than at the place of riſe.

With reſpect to the firſt, Mr. Vaucher aſcribes it to the frequent variations which are ſenſible in the weight of different columns of the atmoſphere, and conſequently in the preſſure it exerts on different points of the ſurface of lakes *. We may eaſily conceive, that if the weight of the atmoſpheric column be ſpeedily diminiſhed in a given part of a lake, with-

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* This cauſe was before indicated conciſely by De Sauſſure, in his firſt vol. of Travels in the Alps.

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but the ſame thing happening over the reſt of the ſurface; or ſtill more if the weight ſhould be augmented upon that remaining ſurface, the water will be forced to riſe in that laſt place, and will again deſcend when the atmoſphere ſhall have reſumed its equilibrium. It is known, in fact, that theſe variations of the barometer are ſo frequent, that it can never be ſaid to be exactly ſtationary; it is known, that they can be produced by changes of temperature, and De Sauſſure has calculated that a diminution of three degrees in the column of air will account for a variation of 0,85 of a line in the barometer. It is known, that theſe variations are moſt frequent in mountainous countries in autumn and in ſpring, and previous to ſtorms, circumſtances which coincide with the greater frequency of ſeiches at thoſe times. This general cauſe tends to explain the ſlight variations of level which are common to all the lakes; it is even of ſuch a nature as to be applicable to all extended ſurfaces, and it is therefore probable, that theſe variations of level likewiſe take place in the ſea, independant of the flux and reflux, which may have hitherto prevented their being obſerved. The variations in the weight of the atmoſphere may perhaps contribute to thoſe ſudden and local elevations of the waters of the ſea, which have all been indiſtinctly conſidered as of the nature of water-ſpouts. The ſame cauſe ought likewiſe to act on rivers, but inſtead of raiſing or diminiſhing their level, it ought, according to Mr. Vaucher, to produce a momentary acceleration or retardation of their courſe; an obſervation difficult to be made, and not hitherto attempted.

—and he ſuppoſes the greater riſe in the lake of Geneva to be cauſed by its peculiar figure.

As to the ſecond part of the explanation, namely, that which ſhould account for the great intenſity of the phenomenon at the extremity of the Leman lake, near Geneva, Mr. Vaucher recurs to two circumſtances peculiar to that lake, and which are found in a leſs degree in thoſe of Zurich and Conſtance, where the ſeiches are moſt remarkable after thoſe of the lake of Geneva; namely, the contraction of a lake in a given place, and the deſcent of its waters towards the place of their diſcharge. With regard to the firſt of theſe circumſtances, it will be ſufficient, if attention be paid to a chart of the Leman lake, to ſhew that it is very remarkably contracted at its weſtern extremity, ſo that at half a league diſtance from Geneva, it has not one third of the breadth of that before Thonon. Now we may compare a lake of this form to a ſyphon full of water, of which the branches ſhould very much differ in diameter; and it will be evident that if, for example, their inequality being as fourteen to one, the ſmalleſt branch ſhould ſuddenly receive by the augmentation of the atmoſphere a ſurcharge equal to that which depreſſes the barometer one line, it would fall 14 lines, and the water which would be driven into the great branch would raiſe its ſurface only one line; whereas, on the contrary, a ſurcharge which ſhould depreſs the level of the great branch only one line, would raiſe it for a moment fourteen in the ſmaller. The effect would be double if at the ſame time the weight of the atmoſphere ſhould diminiſh on one of the branches, and encreaſe on one of the other. We may therefore admit that in lakes, the breadth of which is remarkably contracted in ſome part, the influence of the variations of the atmoſphere to produce ſeiches will be greater in the narrow than in the wide part.

And alſo by circumſtances attending the flowing off of the waters.

A like effect will take place according to Mr. Vaucher, by reaſon of the inclination obſervable in that part of the ſurface of the lake near the place where it diſcharges its water. He remarks that every particle of a liquid on a ſlope may be conſidered as ſolicited by two forces; one which tends to raiſe it to the level of the ſuperior part of the ſlope or the reſervoir, and the other which urges it in the direction of the current. If by the ſudden depreſſion of the ſuperior fluid the current be for a moment ſuppreſſed, the particle will no longer find itſelf urged but by the firſt of theſe forces, and will riſe towards its ancient level, and ſoon afterwards deſcend. Now, as we have before ſeen, all the parts of lakes which have very perceptible ſeiches have a remarkable ſlope; this ſlope is naturally more conſiderable at thoſe times of the year when the waters are higheſt, and theſe are the periods when the ſeiches are moſt ſtriking in the neighbourhood of Geneva.

Singular appearance which ſometimes occurs that the ſurface of the lake is partly ſmooth and partly agitated.

Independent of the phenomenon of the ſeiches, the lake of Geneva and moſt other lakes afford two other ſingular phenomena; the one is known by the fiſhermen of the Leman lake by the name of fontaines. This takes place when the ſurface of the lake, inſtead of being uniformly calm or uniformly agitated, is ſeen to have certain parts calm and certain parts agitated, which are often mixed among each other in a thouſand manners, and always very diſtinct. This fact ſeems to indicate the different atmoſpheric columns, though very near each other, may ſome of them be agitated and others calm. This appearance of the ſurface of the lake is conſidered by the fiſhermen as a ſign of rain.

The ſecond phenomenon of which Mr. Vaucher ſpeaks, conſiſts in certain ſonorous diſtant exploſions or noiſes which reſemble thoſe of the diſcharge of artillery, and are ſometimes heard in the fine ſummer evenings. This phenomenon is rare, but is nevertheleſs affirmed by ſeveral inhabitants near the lake of Geneva. It alſo takes place in the lake of Zurich according to Mr. Eſcher, and in that of Baikal according to that of Mr. Patrin. Mr. Eſcher aſſerts that half or three quarters of a minute after having heard one of theſe noiſes he ſaw a bubble of air about a foot in diameter riſe out of the lake of Zurich.

Another phenomenon reſembling the diſtant noiſe of artillery.

Annotations.—W. N.

It does not ſeem to me that any of the cauſes yet pointed out are ſufficient to account for the effect of the ſeiches. Sudden or ſtrong blaſts of wind could ſcarcely operate in this way ſo partially as that the exiſtence of ſuch ſqualls ſhould not at the ſame time have fixed the attention of the common people as well as of the more accurate obſervers who have noticed theſe changes. It is perhaps equally difficult to ſuppoſe ſuch unheeded variations to take place in the Arve ſufficient to account for theſe very remarkable changes in the lake. Mr. Bertrand's electrical hypotheſis refers us to a claſs of appearances too little underſtood to be admitted, otherwiſe than in the way of looſe conjecture; beſides which, it muſt be remarked that the agency of electrical clouds is much more generally directed to mountains than to the valleys in which lakes muſt neceſſarily have their ſituation. Much ingenuity is laſtly ſhewn by Mr. Vaucher in his explanation, which nevertheleſs requires us to admit of atmoſpheric columns conſiderably differing in weight and occupying very ſmall extent of ſurface. If this be even admitted as poſſible, yet ſtrong doubts may ſurely be entertained as to its probability. It appears to me that the object in queſtion admits of an eaſy ſolution upon other principles, and alſo that his explanation is grounded on poſitions not conſiſtent with the known laws of ſtatics.

Objections to the theories which have been offered reſpecting the ſeiches,

—particularly that of Mr. Vaucher's of atmoſpheric preſſure.

This ingenious author aſſumes as the conditions of his general theory that the lake ſhould conſiſt of two portions of water,

Recapitulation of his facts and deductions.

water, one much more extenſive than the other, and connected by a narrower portion or gut. He then ſtates that if the atmoſpheric preſſure be greater upon the larger ſurface than on the ſmaller, the firſt will be depreſſed and the latter will riſe, and that the difference of elevation in each ſurface occaſioned by the paſſing of any given quantity of water will be greater the ſmaller the ſurface.

No atmoſpheric change can make a greater alteration in the lake than the correſpondent riſe and fall of a water barometer, which is much leſs than really takes place.

This is very true; but it can in no caſe happen, that the difference between the level of one water and the other can amount to a greater quantity than that of a water barometer, by a like change, namely, about fourteen lines for every line of variation in the common barometer. That is to ſay, if the barometer were to riſe and fall again through half an inch, in the ſhort time of a ſeiche, which I believe ſcarcely if ever happens, the ſeiche itſelf could not riſe above ſeven inches. The whole range of variation in the barometer could only cauſe a riſe of three feet and a half inſtead of five which ſometimes happens.

Another theory offered; that the ſeiches depend altogether on the rapidity of ſupply and facility of diſcharge.

I would venture to conjecture that this phenomenon is one among the numerous oſcillatory proceſſes which take place when two variable natural powers are oppoſed to each other in the production or modification of any event. Moſt ſmall lakes are formed by the enlargement of a river, by which the lake is ſupplied at one end and evacuated at the other. The quantity of water in the lake itſelf will, in theſe circumſtances, be always more than would be ſufficient to fill its capacity, taken from the level of the loweſt point of diſcharge. How much more it may be than this quantity will depend upon the ſtreams which enter and paſs out. An increaſe in the quantity of ſupply will keep the level higher, and ſo likewiſe will any increaſe in the obſtacles to its flowing off; and on the contrary, if the ſupply be diminiſhed, or if the facility of diſemboging be encreaſed, the level will be depreſſed. Theſe effects will take place moſt ſtrikingly at firſt at that end of the lake where the efficient cauſe operates. When any change has once taken place, ſuch as that of the depreſſion, it will continue for a ſhort time after the cauſe has ceaſed to act; ſo that the depreſſion would itſelf be followed by a riſe, even if the circumſtances which cauſed it were not alſo ſubject to a like variation. Changes of this kind, on a ſmall ſcale, are obſervable in mill-dams, and even in the ſmooth places in brooks or rivulets, as may

This effect is ſeen in brooks and mill-ponds.

may be obſerved by taking notice of ſome part of the bank where a gently riſing ſand may render the changes of level more conſpicuous. The variableneſs of the weather at the ſpring and autumn, by occaſioning more frequent changes in the quantities of water, and conſequently in the ſtate of the rivers above and below the lakes in queſtion muſt render the ſeiches more frequent and extreme at theſe times. They will alſo be moſt evident at the ends of a long lake; and the other circumſtances will be modified by events that for the moſt part would require to be aſcertained by obſervations of the local circumſtances and events on the ſpot.

It muſt be more conſiderable in ſpring and autumn.

The diſtinct portions of rough and ſmooth ſurface called *fontaines*, which are obſerved on the lakes, are very ſtrikingly ſeen at ſea whenever a breeze ſprings up after a dead calm. This effect is very remarkable, and may perhaps be accounted for on the ſuppoſition that the incipient motions of the air may be attended with eddies that may act more ſtrongly on one part of the ſurface than another. This however does not ſeem reconcileable with a certain ſteadineſs of appearance with which the ſmooth and rough ſurfaces continue diſtinct from each other for certain lengths of time. I am not much ſatisfied with the conjecture which offered itſelf to me, or which may have been mentioned by ſome other perſon when I was at ſea many years ago; but it at leaſt deſerves to be noticed here. It is well known that the wind ſcarcely takes hold of water which is covered with any oily film, and from the experiments of Franklin and others we have learned that a ſingle drop of oil will rapidly ſpread over a large ſurface of water, and cauſe all the ſmall primary waves to ſubſide, rendering the ſurface extremely ſmooth. It ſeemed to me not unlikely that oily matter from animal remains might riſe to the ſurface of the ſea during a calm and ſpread itſelf irregularly over certain parts, which would continue ſmooth for a conſiderable time after the light commencing breeze had ruffled the other parts. I think from recollection that this appearance could not have laſted more than a quarter of an hour; but it is very common, and I often ſaw it. May not a ſimilar cauſe produce the appearance in the lake of Geneva.

The diſtinct patches of ſmooth and rough ſurface on the lake are very uſual at ſea after a calm.

Suppoſed to ariſe from oily matter on the ſurface of the water,

—of the lake alſo.

The ſonorous reports reſembling diſcharges of artillery ſeem very likely to ariſe from the extrication of gas at the bottom of the water, which riſes and breaks at the ſurface. I have no

The ſonorous reports reſembling artillery ſuppoſed to be made by gaſes.

no remark to make on this ſubject, but advert to it principally with a view to mention an effect not generally known, but calculated to ſhew the agitation which a ſmall quantity of aſcending air can produce in water. If a ſwimmer fill his lungs with air by inhaling as much as poſſible, and then dive down or deſcend into the water to the depth of fourteen, twenty or more feet, and when at that depth ſlowly blow the air out of his mouth, he will himſelf hear a roaring noiſe, and the ſpectators will ſee with ſurpriſe the ſurface of the water raiſed into a round or conical maſs about a yard in height, with the water flowing round on all ſides over a ſurface of ſeven or eight ſquare feet. I have little doubt but that the noiſe of this riſing column of water with the breaking of the bubbles of air would be very remarkable in one of the ſtill evenings or nights of ſummer, when the effect of noiſes is remarkably more impreſſive than when the louder ſounds of the day render them leſs obſervable, and in many inſtances altogether inaudible.

Remarkable effect of agitation in water, produced by air blown ſlowly through the lungs at the depth of ſeveral feet beneath the ſurface.

XII.

Experiments to aſcertain the beſt Colour for marking the Heads of Pieces of Cotton or Linen in the rough, which ſhall be capable of reſiſting the Operations of Bleaching, as well as the moſt complicated Proceſſes of Calico Printing, without ſpreading beyond the Limits of the Impreſſion. By Mr. HAUFFMANN.*

Properties required in a good marking colour or ink.

IN order that a colour may be proper to mark piece goods of every kind it is requiſite that it ſhould contain no ſubſtance or drug capable of ſolution in alkalies; it is equally neceſſary that its component parts ſhould not become white by oxygenation, and that they ſhould remain inſoluble in acids ſufficiently ſtrong for the bleaching proceſſes, as well as for the operations antecedent to the calico printing.

Oil colours are bad becauſe they yield to alkalies, &c.

Colours compoſed of drying oil cannot therefore, as I have found, be uſeful in theſe kind of marks, becauſe they are not only attacked by alkaline and ſoapy liquids, but likewiſe becauſe they dry ſlowly, and by ſpreading beyond the limits of impreſſion, very often occaſion ſpots.

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* Annales de Chimie, LIII. 203.

If the colours of ſpirituous varniſhes were not ſubject to the inconvenience of too ſpeedy evaporation and drying they would be inadmiſſible on another account, namely, that the turpentine and reſins are eaſily converted into ſoap. Gum copal is equally unfit for marking colours, becauſe it quits the piece by ſimple ebullition in water. But as the varniſh which I have made defends veſſels of copper or any other metal from the action of acids of a certain ſtrength as well as from that of the atmoſphere, I have thought it might not be unacceptable to deſcribe its compoſition in this place. To obtain this varniſh from copal as pale and as clear as water, this gum muſt be reduced to very fine powder and expoſed with twelve parts of the fineſt oil of turpentine for ſeveral days, or until it ſhall be completely diſſolved at a moderate heat on a ſand bath in a capſule of braſs, ſtone ware, or porcelain, taking care to ſtir it as often as poſſible with a rod of glaſs. At the moment when the conſiſtence of ſyrup begins to take place, the entire ſolution of the copal is effected by agitation, particularly if a ſmall quantity of oil of turpentine be added from time to time to ſupply the loſs by evaporation. Three fourths of the oil of turpentine which is loſt by evaporation when open veſſels are uſed, may be ſaved by performing the proceſs in a long necked mattraſs, which is to be expoſed on a ſand bath a ſufficient time to complete the ſolution of the copal, and ſhaking it very often. The varniſh obtained by either of theſe methods becomes yellowiſh if the heat be urged too ſtrongly; and as by its too glutinous conſiſtence it would be difficult in its application, it is convenient, inſtead of diluting it with oil of turpentine, to mix it with one fourth or one fifth part of its weight of alcohol, taking care not to uſe too much, becauſe an exceſs would render it of a milky white by the precipitation of part of the copal, which cannot admit in its ſolution more than a certain quantity of alcohol without precipitating. Veſſels of braſs or of any other metal may receive one, two, or three coatings of this varniſh, and muſt be each time well dried in the oven. After this treatment they may be waſhed with boiling water without injury, and may even be expoſed to a ſtill greater heat without the varniſh coming off; but theſe veſſels muſt not be rubbed with ſand or other hard bodies.

Varniſh colours are equally faulty in this reſpect.

Copal yields to boiling water.

Proceſs for making a good varniſh. Copal in powder is diſſolved by heat and agitation in oil of turpentine.

The copal varniſh is to be diluted with alcohol.

Metallic veſſels having this varniſh baked upon them may be expoſed to boiling water without injury.

By means of oil of turpentine, which evaporates and dries leſs ſpeedily than alcohol, I ſucceeded in making a black compoſition

An oily compound for marking linen and other goods.

poſition, which I expected might be uſed with advantage in marking piece goods. For this purpoſe nothing more is needful than to diſſolve ſlowly on the ſand bath, and with conſtant agitation. One fourth of its weight of aſphaltum or bitumen, judaicum well pounded, and afterwards to mix as much lamp black, or any other dark coloured mineral in fine powder, ſuch as black lead, galena, or the like. This colour may be had more or leſs thick, by due proportions of the oil of turpentine and bitumen; it prints very well without running, if the proper proportions be attended to, and a little oil of turpentine be added when it becomes too thick. This bituminous colour ſupports the action of alkalies and of oxigen very well, and reſiſts all acids of moderate ſtrength.

As I thought it unneceſſary to continue my experiments on oil colours, I made my experiments on watery compounds in the following order.

Section I.

First marking proceſs. An impreſſion is made of a ſolution of ſulphate of mangaueſe thickened with gum, and covered with lamp black, the cloth being then dipped in alkali, the mangaueſe precipitates in brown oxide which affords a mark not to be diſcharged by bleaching, or by the printing proceſſes.

I diſſolved in four ounces of water one ounce of the ſulphate of Manganeſe without its water of cryſtallization; that is to ſay, it was in the ſtate it poſſeſſes when oxigen gas is procured from the black oxide of manganeſe, by means of the ſulphuric acid, and by raiſing the heat to ignition at the end of the proceſs. This ſolution was thickened with one dram of fine gum adragan in powder, and coloured with lamp black, in order to diſtinguiſh exactly the impreſſion which may be eaſily made with this black ſaline metallic maſs, of which nevertheleſs, we cannot make effective uſe without plunging the end of the marked piece into an alkaline ley, taking care that it ſhall not firſt be wetted with water, which would carry off the ſaline matter. The ley may be made with potaſh or ſoda, in the proportion of one part alkali to nine or twelve parts water. It may be uſed in the ſtate of carbonate, or rendered cauſtic with half a part of quick lime. The precipitation of the oxide of manganeſe from the marks by either of theſe alkaline ſolutions will take place (excluſive of the ſtain from the lamp black) of a yellowiſh white colour, which will become more and more brown by attracting the oxigen of the atmoſphere. The change of theſe marks to the brown, and even to a deeper colour inclining to black, will take place very ſpeedily by bleaching with the oxigenated alkaline muriatic ley, the pieces of which the ends have been plunged for a few minutes in the alkaline as before preſcribed. Theſe marks of the brown oxide of mangaueſe reſiſt not only all the bleaching materials, and all acids of a requiſite force, but likewiſe the moſt complicated proceſs of manufacture of calico printing.

Section II.

If the acetic acid had not a much ſtronger affinity with mangaueſe than it has with iron, and if it diſengaged itſelf as readily from the acetate of mangaueſe as it does from the acetic ſolution of iron by evaporation and drying, we ſhould be able to procure indeliable marks in the moſt ſimple manner, by depoſiting the oxide of mangaueſe on piece goods by means of the acetic acid, and afterwards ſimply leaving the oxide to the attraction and ſaturation of oxigen from the air. The acetic ſolution of mangaueſe is very readily obtained by mixing a proper quantity of acetate of lead in a ſolution of ſulphate of mangaueſe. But as this acetic ſolution affords no advantage in marking piece goods beyond thoſe of the ſulphate of mangaueſe, and as it requires preciſely the ſame management as that deſcribed in the laſt ſection, and it is likewiſe more expenſive, it deſerves to be rejected.

The acetate of mangaueſe cannot be uſed without the ſame manipulation, and as it is more coſtly it muſt be rejected.

Section III.

Two ounces of ſulphate of magneſia diſſolved in eight ounces of the acetic ſolution of iron, concentrated to the point indicated by twenty degrees, afford when thickened with one fortieth part of gum adraquack, a deep yellow liquor which becomes more and more brown, when treated abſolutely in the ſame manner as deſcribed in the firſt paragraph. The acetic ſolution does not, however, afford any other advantage but that of cauſing the marks to dry a little more ſpeedily; for the oxide of iron diſſolves in acids accordingly, as it is oxigenated. I give the preference to gum-adragant for thickening colours, to other gums and to ſtarch, becauſe theſe ſubſtances weaken the colours too much, if however, there ſhould be any objection to gum-adragan in coarſe goods, ſtarch may be then uſed.

Sulphate of mangaueſe with acetate of iron treated as before. It dries more quickly.

Section IV.

Marks printed with the grey oxide of manganeſe obtained in waſhing the reſidual ſulphate, afford fixed marks by ſimple impreſſion.

If care be taken in the proceſs of diſengaging oxigen gas from a mixture of the black oxide of manganeſe and ſulphuric acid, not to carry the fire to ignition, the ſaline reſidue remains blackiſh, inſtead of becoming yellowiſh white by ſtrong heat. When this reſidue is diſſolved in water, it leaves behind it an oxide of a deep grey, which acquires a very paſty conſiſtence on the filtre. This oxide mixed with a very little water thickened with gum adragant, may be uſed to print marks of a very deep grey, which dries ſpeedily; and this colour does not waſh out with water, even though the ſubſequent dipping in an alkali be omitted. It is ſo fixed that it not only ſupports the action of all acids of the manufacturing ſtrengths, but likewiſe all the bleaching and printing proceſſes without attracting the colouring matter of any dye whatever.

Section V.

Addition of the nitro muriate of tin to the marking oxide. It affords a dye.

If there were no reaſon to fear injuring in a ſlight degree the place where the mark is made, it would be advantageous to employ equal parts of the laſt deſcribed grey paſte, and of a nitro muriatic ſolution of tin, containing one fourth part of the metal, and thickened with gum-adragath. This colour is as unalterable as that of the fourth ſection; and it has the additional advantage, that its oxide of tin being ſaturated with oxide of oxigen, attracts the colouring parts of any tincture, and acquires a puce colour by madder. I muſt obſerve on this occaſion, that by the ſame madder dye, the colours of marks from the oxide of manganeſe ſaturated with oxigen, become of a deep puce colour, inclining to black, whereas in a leſs oxigenated ſtate they acquire fainter ſhades. In all theſe circumſtances however, it is requiſite, that the quantity of metallic oxide ſhould be as great as poſſible; otherwiſe the ſhades will be various, and leſs intenſe.

Section VI.

Experiments with the precipitate of manganeſe and ſolution of iron.

As many inſoluble metallic oxides acquire the property of adhering to ſtuffs by means of acid, I did not fail to try whether the ſame would be the caſe with the precipitate of manganeſe ſaturated with oxigen. For this purpoſe I diſſolved one part

of

Experiments with the precipitate of manganeſe and ſolution of iron.

of ſulphate of manganeſe in ſix parts of water, and precipitated the metal by adding to the point of ſaturation a cauſtic alkaline ley, made with half a part of quick lime, four parts of water, and one part of calcined potaſh of the ſhops. The precipitate was yellowiſh white. To the whole aqueous maſs I then added a ſufficient quantity of oxigenated muriatic alkaline ley, until the precipitate was completely ſaturated with oxigen, and its brown colour became no deeper. I afterwards collected on a filtre the precipitate or brown oxide of manganeſe, where, by the drainage of its water, it became paſty. This brown paſte, mixed with half its weight of the moſt concentrated acetic acid no longer afforded any but a weak browniſh ſhade; it was the ſame with a ſmall addition of one or the other of the three ancient mineral acids in a ſtate of ſolution. I did not ſucceed better by mixing one part of the ſame brown paſte with an equal quantity of the acetic ſolution of iron, marking 20° of the areometer of the ſaltpetre makers and thickened with gum adraganth. This acetic ſolution of iron containing only the quantity of oxigen neceſſary for the ſolution of the metal ceaſed by a ſtronger affinity, the exceſs of oxigen of the brown oxide of manganeſe, which in its turn became diſſolved, and the mixture of the two metallic ſolutions afforded a yellow reddiſh very deep and tranſparent liquid, which confirms the fact that a metal ſaturated with oxigen requires leſs acid for its ſolution than if it were in an oppoſite ſtate, and that being then furniſhed with an exceſs of acid, the ſolution ſaturated with oxigen can admit a portion of another metal without becoming turbid. This mixed ſolution of the two metals afforded me only a ruſty yellow which was diſcharged by weak ſulphuric acid completely, in ſomewhat leſs time than was required to take out a ruſt ſpot in a leſs oxigenated ſtate. In order to obtain from the mixture of theſe two metallic ſolutions an indelible marking colour, it is neceſſary that the marks ſhould be ſteeped for ſeveral minutes in an oxigenated muriatic alkaline ley, to precipitate and ſaturate the oxigen of the oxide of manganeſe. By mixing half a part of the brown paſte of manganeſe to two parts of the ſolution of the two metals the new portion remains untouched and renders the whole turbid. This turbid mixture left only a light browniſh mark on piece goods, which had remained long in the diluted ſulphuric acid.

The muriatic ſolution of tin takes up the oxides of iron and of mangaueſe.

By means of the muriatic ſolution of tin, which has the property of taking the oxigen from many vegetable, animal, and mineral ſubſtances, and which, on this account, is very uſeful in dying, as well as in calico-printing. We may diſcolour and diſſolve inſtantly the deepeſt oxide of manganeſe and of iron, which proves the preponderating affinity of tin towards oxigen beyond that of manganeſe or of iron.

N. B. There is no reaſon to object to ſteeping the marked goods in an alkaline ley; the operation is ſpeedily made without ſenſible loſs of potaſh or of ſoda, if the operation of lixiviating be immediately proceeded upon, for which the remainder of the ley may be uſed. And generally, if the practice be uſed which has been adopted for a number of years, of rendering the alcalies cauſtic with quick-lime, the ſaving will be conſiderable and with better effect.

XIII.

Note on the Formation of Water by mere Compreſſion; with Reflections on the Nature of the Electric Spark. By M. Biot.*

That oxigen and hidrogen may unite by preſſure.

It was ſome time ago that, in converſation with M. Berthollet on the nature and properties of heat, I communicated to him the perſuaſion I had, that the combination of hidrogen and oxigen gaſes might be determined without the aid of electricity, and merely by a very rapid compreſſion. This reſult appeared to me a conſequence ſo immediately following the obſervations already made on the heat diſengaged from air by compreſſion, that I thought it needleſs to aſcertain it in any other manner. But having ſince converſed with Mr. La Place, he appeared ſo intereſted as ſtrongly to urge me to a verification. I therefore made the experiment, which completely ſucceeded. It was made in the cabinet of the Polytechnic School. I am greatly indebted to M. Haſſenfratz, profeſſor of natural philoſophy in that eſtabliſhment, for the great attention he paid in cauſing the requiſite preparations to be made, and for his perſonal aſſiſtance in repeating it.

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* Read to the National Inſtitute of France, and inſerted in the Annales de Chimie. LIII. 321.

Experiment in proof. The ſyringe of an air-gun filled up ſo as to receive the gaſes and admit of inſpection into its chamber, was firſt tried with common air,

We took the ſyringe of an air-gun, the bottom of which was cloſed by a very thick glaſs, in order that we might obſerve the light diſengaged as uſual by compreſſion. This ſyringe was of iron; it had a cock on one ſide to introduce the gaſes, and its lower extremity on the ſide of the piſton was enveloped by a cylinder of lead, ſufficiently weighty to accelerate the fall and render the compreſſion more rapid. This apparatus was firſt tried by introducing atmoſpheric air; but though the experiment was made in the dark, no perceptible light was ſeen, probably becauſe the violent motion neceſſary for the rapid compreſſion, prevented the operator from looking ſo directly through the glaſs as to perceive the tranſient light which compreſſion diſengages, and which I myſelf had ſeveral times ſeen.

and afterwards with a mixture of oxigen and hidrogen. The glaſs was broken by ſtrong luminous exploſion.

Immediately after this trial a mixture of hidrogen and oxigen gaſes was introduced into the ſyringe, and a ſtroke was given. An extremely brilliant light appeared with a loud detonation: The glaſs bottom was driven out: The copper ſcrew which retained it in its place was broken; and the perſon who held the ſyringe had his hand ſlightly burned and wounded by the force of the exploſion.

Repetition with a metallic cap. The ſyringe burſt.

The experiment was repeated, by ſubſtituting a braſs bottom of one entire piece ſcrewed on inſtead of that of glaſs, and a new mixture of the gaſes was introduced. The firſt ſtroke of the piſton produced an exploſion, which was heard like the loud crack of a whip; but a ſecond ſtroke with a new charge of the gaſes, cauſed a detonation which broke or rather tore the body of the ſyringe with a violent exploſion.

After theſe phenomena there can remain no doubt reſpecting the combination of the two gaſes; as it is known that this combination produces the detonation by the immenſe quantity of heat diſengaged when they paſs to the liquid ſtate; a heat which is ſufficient to reduce them immediately into vapour, and give them an exceſſive dilatation in that ſtate. It was not therefore thought neceſſary again to repeat this experiment, which is attended with ſome danger.

Theory, the gaſes give out heat.

The theory of theſe phenomena is extremely ſimple. A rapid compreſſion forces the gaſes to abandon a very great quantity of heat, which not being capable of immediate diſſipation, raiſes their temperature in the inſtant ſufficiently to inflame them in this ſtate of compreſſion.

Thus it is that we find in the two gaſes all the elements neceſſary for their combination, independently of the electric ſpark or external heat. We might probably in the ſame manner, and without any foreign agent, produce all the gaſeous combinations which require an increaſe of temperature.

Deduction. The electric ſpark may conſiſt merely of light driven from compreſſed air.

This identity of reſults has led me to a notion which I ſubmit to the judgment of philoſophers. It is known, and M. Berthollet has ſhewn it in his Chemical Statics, that electricity in paſſing through bodies, produces a true compreſſion of their particles. This effect is produced with the moſt extreme velocity, as may be proved by an affinity of experiments. Now electricity poſſeſſing a velocity ſo great, it is impoſſible that it ſhould not diſengage light from the air, ſince we can diſengage it by a compreſſion ſo much leſs rapid. In this way it is that we are led to a concluſion, that this reſult of the electric ſpark is the purely mechanical effect of compreſſion.

More ample explanation. The extreme velocity of electric matter will ſtrongly compreſs free air,

If we now compare what paſſes in our condenſing pump and in the eudiometer of Volta, we ſhall find that the analogy is complete. Only that in the firſt caſe we are obliged to confine the air, becauſe the velocity we can give to the piſton is limited. Whereas in employing electricity, the particles are compreſſed by a velocity ſo great that they can never withdraw themſelves with ſufficient ſpeed from its effort. Therefore the compreſſion may be equally well made in the open air, together with the diſengagement of light or the ſpark, which is its conſequence. But this effect is local; and if the gaſes be not ſuſceptible of combining together, ſhould after each exploſion return to their primitive dimenſions, they muſt immediately reſume in this dilation all the heat they had before diſengaged, ſo that there cannot be effected any laſting change in their conſtitution. This explains why no alteration has ever been ſeen in very pure unmixed gaſes, when ſubjected to the action of the electric ſpark.

and alſo the rare fluids in our vacuum.

This light which electricity diſengages from the gaſes by compreſſion, it muſt alſo diſengage from the more rarified gaſes, and on account of its extreme velocity, it muſt diſengage it even from vapours, when experiments are made under the receiver of an air-pump or in the torricelian vacuum: For we can never form a perfect vacuum with our machines, and even in the tube of the barometer mercury always exiſts in the form of vapours. Theſe vapours, though very rare, ſtill contain a large quantity of caloric, which the electricity muſt diſengage in its paſſage by compreſſion; but the inſtantaneous augmentation of electricity which reſults, cannot become ſenſible on account of the little denſity of the medium; but this increaſe is perceivable in denſer air, as we ſee in the inſtrument called Kinnerſley's thermometer.

Concluſion.

The conſiderations which I have here made, appear to me to point out with ſome probability, that the phenomenon called the electric ſpark, is owing to the light diſengaged from the air by compreſſion during the paſſage of the electricity; ſo that this phenomenon is purely mechanical, and not at all electric in itſelf. This is the notion which I ſubmit to the judgment of philoſophers: if it be true, it muſt tend conſiderably to diminiſh the number of hypotheſes which have already been made, or may be made on the nature of electricity. For this reaſon it is that I have offered it to their conſideration, requeſting that it may not be be thought that I conſider it as of greater importance than their deliberate examination may beſtow upon it.

XIV.

Account of Thermometers for regiſtering the higheſt and loweſt Temperatures in the Abſence of the Obſerver. By F. A.

To Mr. NICHOLSON.

SIR,

Thermometers and barometers for regiſtering the weather.

MANY contrivances have been propoſed and adopted for regiſtering all the ſtations of the thermometer and barometer, by means of a float or other equivalent inſtrument carrying a pencil, which marks its ſituation on a ſurface gradually moved along by means of a clock. Theſe, of which meteorologiſts know the value, are nevertheleſs expenſive, and require a degree of care and management ſufficient to render ſimpler contrivances

Six's thermometer.

contrivances acceptable. Mr. James Six communicated, about 25 years ago, to the Royal Society a thermometer, in which two ſmall indicating pieces were driven by the fluid in the tubes to ſtations where they ſtuck, and remained after the change of temperature, and ſhewed the higheſt and loweſt degrees that had occurred ſince the laſt placing of them in contact with it. As this inſtrument is ſufficiently known, and I am now to advert to a ſimpler contrivance, I will diſmiſs that ſubject and advert to this laſt.

Objections to it.

Another thermometer, which ſhews the greateſt and leaſt temperatures.

In Mr. Six's complicated thermometer the tubes were vertical, and the indexes ſtuck in the glaſs by their ſpring; beſides which, a ſmall piece of ſteel wire being expoſed to alcohol, was at length oxided and ſet faſt. The other contrivance now to be ſeen in all our London ſhops, and reſpecting which you will do an acceptable ſervice to your readers and the ſcientific world, by inſerting a ſketch in your Journal, conſiſts ſimply in two thermometers, one mercurial and the other of alcohol (*Fig.* 1, *Pl.* X.) having their ſtems horizontal; and the former has for its index a ſmall bit of magnetical ſteel wire, and the latter a minute thread of glaſs, having its two ends formed into ſmall knobs by fuſion in the flame of a candle.

Deſcription. It has a mercurial thermometer which ſhews the maximum; and a ſpirit thermometer for the minimum.

The magnetical bit of wire lies in the vacant ſpace of the mercurial thermometer, and is puſhed forward by the mercury whenever the temperature riſes and puſhes that fluid againſt it: but when the temperature falls and the fluid retires, this index is left behind, and conſequently ſhews the maximum. The other index, or bit of glaſs, lies in the tube of the ſpirit thermometer immerſed in the alcohol, and when the ſpirit retires by depreſſion of temperature, the index is carried along with it in apparent contact with its interior ſurface: but on increaſe of temperature the ſpirit goes forward and leaves the index, which therefore ſhews the minimum of temperature ſince it was ſet. As theſe indexes merely lie in the tubes, their reſiſtance to motion is altogether inconſiderable. The ſteel index is brought to the mercury by applying a magnet on the outſide of the tube, and the other is duly placed at the end of the column of alcohol by inclining the whole inſtrument.

Queſtion. Why the ſmall glaſs index always remains in the ſpirit.

I beg you will explain the motion of the glaſs index. I can eaſily underſtand from the general fact *that mercury repels ſteel*, that this fluid will drive the ſteel index before it; but I cannot make out to my ſatisfaction, how the ſpirit, by attracting glaſs, can prevent the other index from ever riſing out of its ſurface. Perhaps this thing may be already explained in elementary books; but whether it be or no, I am ſure that an account in your clear and popular way cannot be thought ſuperfluous.

I am reſpectfully,
Sir,
Your obliged
F. A.

REPLY.—W. N.

Explanation. If the ſmall glaſs piece were protruded beyond the ſpirit, the fluid would hang to it and draw it back.

When the ſurface of the column of ſpirit is viewed by a magnifier, it is ſeen to have the form of a concave hemiſphere, which ſhews that the liquid is attracted by the glaſs. The glaſs in that place is conſequently attracted in the oppoſite direction by a force equal to that which is ſo employed in maintaining that concave figure; and if it were at liberty to move, it would be drawn back till the flat ſurface was reſtored. Let us ſuppoſe a ſmall ſtick or piece of glaſs to be looſe within the tube, and to protrude into the vacant ſpace beyond the ſurface of the alcohol. The fluid will be attracted alſo by this glaſs, and form a concave between its ſurface and that of the bore of the tube. But the ſmall interior piece being quite at liberty to move, will be drawn towards the ſpirit ſo long as the attractive force poſſeſſes any activity; that is, ſo long as any additional fluid hangs round the glaſs; or in other words, until the end of the ſtick of glaſs is even with the ſurface. Whence it is ſeen that the ſmall piece of glaſs will be reſiſted, in any action that may tend to protrude it beyond the ſurface of the fluid; and if this reſiſtance be greater than the force required to ſlide it along in the tube (as in fact it is), the piece muſt be ſlided along as the alcohol contracts; ſo as always to keep the piece within the fluid. And this fact is accordingly obſerved to take place.

Abſtract

XV.

Abſtract of a Memoir on Milk. By M. THENARD.*

Component parts of milk.

IN a memoir which I read to the Philomatic Society in Praireal laſt, I ſhewed that milk always contains the free acetous acid in a greater or leſs quantity. At the ſame period Meſſrs. Fourcroy and Vauquelin found that it alſo contains phoſphate of magneſia, and that the lactic acid of Scheele, or that which is obtained from ſerum of milk ſpontaneouſly coagulated, is merely the acid of vinegar combined with an animal matter. So that in the preſent ſtate of our knowledge we muſt conſider milk as compoſed of, 1. Water; 2. Acetous acid; 3. Caſeous matter; 4. Butteraceous matter; 5. Sugar of Milk; 6. Extractive matter; 7. Muriate of ſoda and of potaſh; 8. Sulphate of potaſh; 9. Phoſphate of lime; 10. Phoſphate of magneſia.

Of theſe eleven ſubſtances there is one which I particularly examined ſome months ago, namely cream. I was deſirous of aſcertaining the circumſtances which govern its ſeparation, and particularly its transformation into butter.

The ſeparation of cream and of butter does not require acceſs of air.

I had before obſerved that milk coagulates as readily in cloſed as in open veſſels; I know that no gas is diſengaged in this decompoſition, and that, in order to effect it with rapidity, it is needful only to raiſe the temperature to between 20° and 40° (Reaumur I ſuppoſe; and, if ſo, anſwering to 77° and 122° Fahrenheit). It was clear, therefore, that the air contributes neither to the formation nor the ſeparation of cream, but that it exiſts ready formed in milk; but it remained to be ſhewn what are the principles which enter into its compoſition. Being perſuaded, from various obſervations I had made, that it is only an intimate mixture of butter, cheeſe, and ſerum, I proceeded to aſcertain this point by mixing a pint bottle (Engliſh quart) of recent cream nearly to its neck, from which I diſplaced the remaining air by carbonic acid. I then cloſed it well, and agitated it ſtrongly in every direction for half an hour; at the end of which time the contents having become very thick and adhering ſtrongly to the ſides of the bottle, gradually became detached, and ſoon afterwards were converted into a white liquid, in the midſt of which ſwam a yellow maſs of excellent butter. Hence it follows, that the butter exiſts in the milk, and is ſeparated when the milk, being deprived of the vital action, is left to itſelf. At this time, either by the formation of an acid ariſing doubtleſs from a decompoſition of the extractive matter, or perhaps from the leſs ſpecific gravity of the butter compared with that of the cheeſe; for the butter begins to ſeparate almoſt at the moment that milk is poured into a veſſel;—the milk is decompoſed, the cream riſes to the top, and from this laſt, by agitation, and more particularly by the aſſiſtance of a temperature between 15° and 20° (66° to 77° F.), butter is obtained together with butter-milk, which is a white very mild liquor, in which ſome butter and cheeſe are ſuſpended in a very divided ſtate. But the butter thus obtained is not pure: It ſtill contains a portion of cheeſe amounting ſometimes to the ſixth part of its weight; and this is the cauſe of its ſpeedily becoming rancid, particularly in ſummer. When the cheeſy matter is ſeparated by fuſion, the butter may be kept a long time. It is true indeed, that by this fuſion it acquires an acridneſs which greatly limits its uſes, and makes it unfit to be employed in frying; but this diſadvantage might be remedied by keeping the temperature much lower than is uſual. Clouet firſt made this obſervation; and hence the following proceſs may be adopted for purifying butter, or ſeparating the cheeſy matter without giving it a bad taſte.

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* Soc. Philomath, No. 96.

Proceſs of butter making.

Purification of butter by fuſion, which ſeparates the cheeſy part.

1. Let the butter be melted on the water bath, or at a degree of heat not exceeding the 66° of Reaumur. 2. Keep it melted till all the cheeſy matter is collected in white flakes at the bottom of the veſſel, and the melted butter is tranſparent. 3. At this period decant it, or paſs it through a cloth. 4. Let it be cooled in a mixture of equal parts of pounded ice and ſea-ſalt; or if ice cannot be procured, then in cold ſpring-water, making uſe of broad ſhallow veſſels. Without this precaution the butter would become lumpy by cryſtallizing, in which ſtate it could not be ſerved at table. Beſides which, the parts being condenſed by this ſudden cold, are found to reſiſt the action of the air more effectually. With this laſt intention it is alſo proper

to cover the pot in which the butter is kept very exactly, and to place it in a cold exposure, ſuch as a cellar. By this treatment butter may be kept for ſix months or more, and will be nearly as good as freſh butter, particularly after the top is taken off. It is even poſſible to give this fuſed butter to a certain point the appearance of freſh butter, by beating it with one ſixth part of its weight of the cheeſy matter; and ſo likewiſe rancid butter may be conſiderably amended by the proceſs of fuſion and cooling here preſcribed.

SCIENTIFIC NEWS.

Temperature of the Sea.

General facts reſpecting the temperature of the ſea.

MR. PIRON has lately communicated to the French National Inſtitute a memoir on the temperature of the ſea; an intereſting ſubject, capable of being applied to various uſeful purpoſes, and which has accordingly engaged the attention of a conſiderable number of philoſophical obſervers. His general facts are, 1. The mean temperature of the ſea at its ſurface is commonly more elevated than that of the air. 2. It is higher the nearer to the continents and large iſlands. 3. At a diſtance from the ſhore in deep ſeas the water is colder below than at its ſurface; and the more the greater the depth. All the obſervations ſeem to ſhew, that in the abyſſes of the ocean, as well as on the ſummits of mountains, even under the equator, eternal froſt prevails. 4. A ſimilar cold is obſerved in extenſive lakes, and even within the earth at great depths, but it appears to be leſs ſudden. 5. Theſe reſults concur in proving, that the temperature within the earth is not every where the ſame and equal to 93¼°, as has been long thought (about 50° Fahr. whether this be centigrade or Reaumur's ſcale.)

At great depths it is eternally frozen.

Spent Oil of the Curriers.

Concerning the oil and compounds uſed in currying leather.

The proceſs by which the curriers impregnate their ſkins is is by ſmearing the oil upon the wet ſkin, into which it penetrates as the moiſture evaporates. A pure oil could not perhaps be thus ſpread, and moſt probably would not enter the ſkin with the deſired effect, or render it as ſupple as that oil which from experience they are led to prefer.

The celebrated Seguin has directed his attention to this ingredient of ſuch extenſive manufacturing utility. He remarks, that this material (by the name of *Degras*) is of two kinds in France; *viz.* the common ſort and that of Niort. The firſt is the immediate product of the chamoying of ſkins, which are cleared of their ſurplus oil by ſolution of potaſh. It therefore contains not only ſoap, but likewiſe gelatine. It is evaporated to dryneſs and then ſold as *Degras*. At Niort it is decompoſed by ſulphuric acid, and the precipitate is called the *Degras* of that town.

Mr. Seguin finds by analyſis, that this laſt is oxigenated oil, whereas the other is a compound of ſoap and gelatine. He ſucceeded in giving to whale oil all the properties of the *Degras* of Niort, by boiling one pound for a few minutes with half an ounce of nitric acid at 25 degrees. He obſerved that no gas is diſengaged in this operation; but that water and nitrate of ammonia are formed; and he concludes that the oil was oxigenated, not by abſorbing the oxigen of the acid, but by yielding to it part of the hidrogen which was one of its own component parts. The reſult is the more intereſting, as the *Degras* of Niort being much more eſteemed than the common ſort, the curriers may hereafter, inſtead of paying a great price for it, make it in as large quantities as they pleaſe by following the proceſs here indicated.

Note reſpecting the Decompoſition of Sulphate of Lead by the Muriatic Acid. By M. DESCOTILS *.

If the ſulphate of lead be treated with muriatic acid rather concentrated, that metallic ſalt is totally diſſolved, provided the proportion of acid be rather in exceſs. This ſolution requires heat to effect it. Upon cooling, the muriate of lead cryſtallizes in great quantity; and it is much more ſpeedily obtained by the addition of a ſmall quantity of cold water. If the ſupernatant fluid be ſeparated from the cryſtallized ſalt,

Sulphate of lead is ſoluble by heat in muriatic acid; Muriate of lead ſeparates by cooling. This is ſoluble in water, and may be again decompoſed by ſulph. acid.

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* Soc. Philom. No. 96.

a precipitate is obtained from the former by muriate of barytes. The muriate of lead is ſoluble in water, and may be almoſt entirely decompoſed by ſulphuric acid, which forms ſulphate of lead.

Another inſtance in the analyſis of antimonial galena.

This fact deſerves to be carefully examined with relation to the play of affinities; and it may be of importance in the analyſis of mineral and metallic ſubſtances. In fact, if an alloy contain a ſmall quantity of lead, and it were neceſſary, in order to diſſolve the alloy, to employ the nitro-muriatic acid, it would be very poſſible, and I have found it ſo, that ſulphuric acid would not indicate the preſence of lead. The following is another inſtance: If an antimonial galena be treated with nitric acid and ſulphate of lead thus formed, this laſt would be decompoſed by the muriatic oil (*qu.* acid?) which might be employed to take up the oxide of antimony, and the muriate of lead would remain diſſolved after the addition of water. If care were not taken to examine the filtered liquor, a loſs would be experienced which it would be difficult to account for.

Extract of a Letter from Naples, dated Auguſt 13.

Account of the late eruption of Mount Veſuvius.

"Yeſterday at ten o'clock at night, the eruption of Veſuvius, of which the earthquake ſeemed to be the forerunner, took place. We were going to viſit the crater when the cries of the people and a volume of flame informed us that the volcano had opened. The lava precipitated itſelf in three ſeconds from the laſt peak of the mountain and took a direction towards the valley, ſituated between Torre del Greco and Torre del l'Annunziata, two towns on the ſea coaſt, beyond Portici, and ſeven or eight miles from Naples.

We ſet off immediately to ſee this wonderful and tremendous phenomenon nearer. From the place of our departure, we ſaw the whole courſe of the lava, which extended nearly two miles, from the crater to the houſes that join the two towns. The ſight was the moſt magnificently frightful that could be ſeen. I contemplated the caſcades of flame pouring from the top of the mountain, and ſhuddered at ſeeing an immenſe torrent of fire ravage the fineſt fields, overthrow houſes, and deſtroy in a few minutes the hopes and reſources of a hundred families.

Account of the late eruption of Mount Veſuvius.

A line of fire marked the profile of the mountain: a cloud of ſmoke, which ſeemed to ſend forth from time to time flaſhes of lightning, hung over the ſcene, and the moon appeared to be pale: Nothing can adequately deſcribe the grandeur of the ſcene, or give an accurate idea of the horror of it. As we approached the ſpot ravaged by this river of hell, ruined inhabitants having quitted their houſes—deſolated families trying to ſave their furniture and proviſions, laſt and feeble reſource—an immenſe croud of curious ſpectators, retreating ſtep by ſtep from the advancing lava, and teſtifying by extraordinary cries their wonder, fear, and pity—the frightful bellowing of the mountain, the frequent exploſions which burſt from the bottom of the torrent, the crackling of the trees devoured by the flames, the noiſe of the walls falling, and the lugubrious ſound of a bell, which the religious of the Camaldules, iſolated on a little hill and ſurrounded by two torrents of fire, rang in their diſtreſs. Such are the details of the frightful ſcene to which I was witneſs.

The moment we arrived the lava was croſſing the great road below Torre del Greco. To ſee it better we got into a beautiful houſe on the road ſide—from the terrace we ſaw the fire at no more than fifteen paces from us—in a minute we deſcended, and twenty minutes afterwards there remained of the houſe but three large walls. I approached as near as the heat and flow of the current would permit me, I attempted at different times to burn the end of my handkerchief in it—I could only do it by tying it to my cane. The lava does not run in liquid waves; it reſembles an immenſe quantity of coals on fire, which an invincible ſtrength had heaped up and puſhed on with violence. When it met with a wall, it collected to the height of ſeven or ten feet, burnt it, and overthrew it at once. I ſaw ſome walls get red hot, like iron, and melt, if I may uſe the expreſſion, into the lava. In its greateſt ſpeed and on an horizontal road, I reckoned that the torrent travelled at the rate of eighteen inches a minute. Its ſmell reſembled that of iron red hot.

Morning Chronicle.

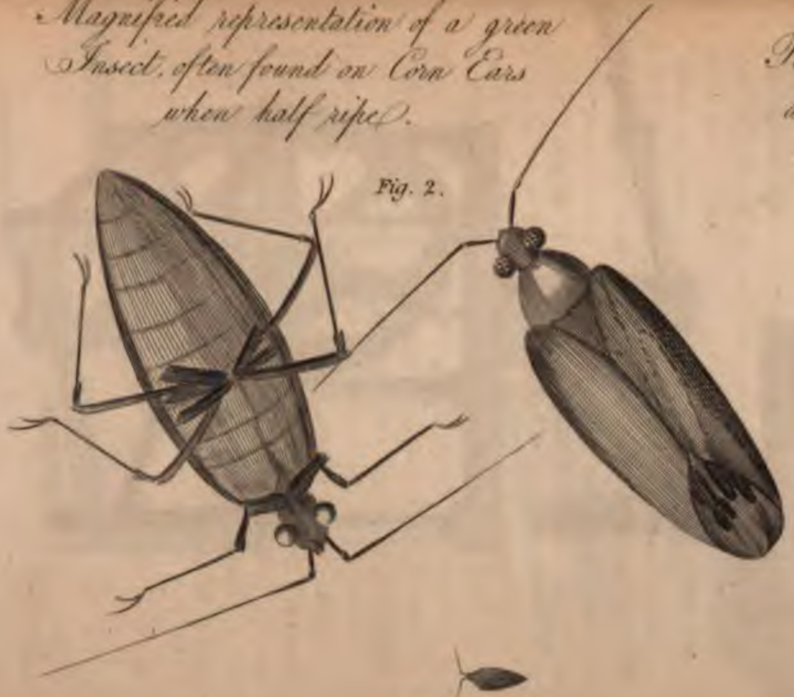
Applicative Compaſs for taking Bearings on a Chart, by N. D. STARCK, *Eſq. of the Royal Navy.*

Compaſs for taking bearings, on a chart.

This inſtrument, ſeen in *Fig.* 6, *Plate* XI. conſiſts of an inner and outer braſs concentric circle; the latter of which, when in uſe, is to be applied to a chart, ſo that its cardinal points may agree with thoſe of the draft, and its central (metallic) point be directly over the ſhip's place. The inner circle is to be ſet to the variation; and the thread from the center being laid, will ſhew either bearings by compaſs, or true bearings, according to the circle upon which they are read. It is obvious alſo, that the inſtrument may be uſed in delineating, plotting, and for various other uſeful purpoſes.

Magnified representation of a green
Insect, often found on Corn Ears
when half ripe.

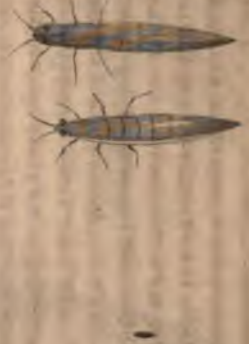
Fig. 2.



Natural Size.

Pale yellow Insect found in
diseased Grain (magnified)

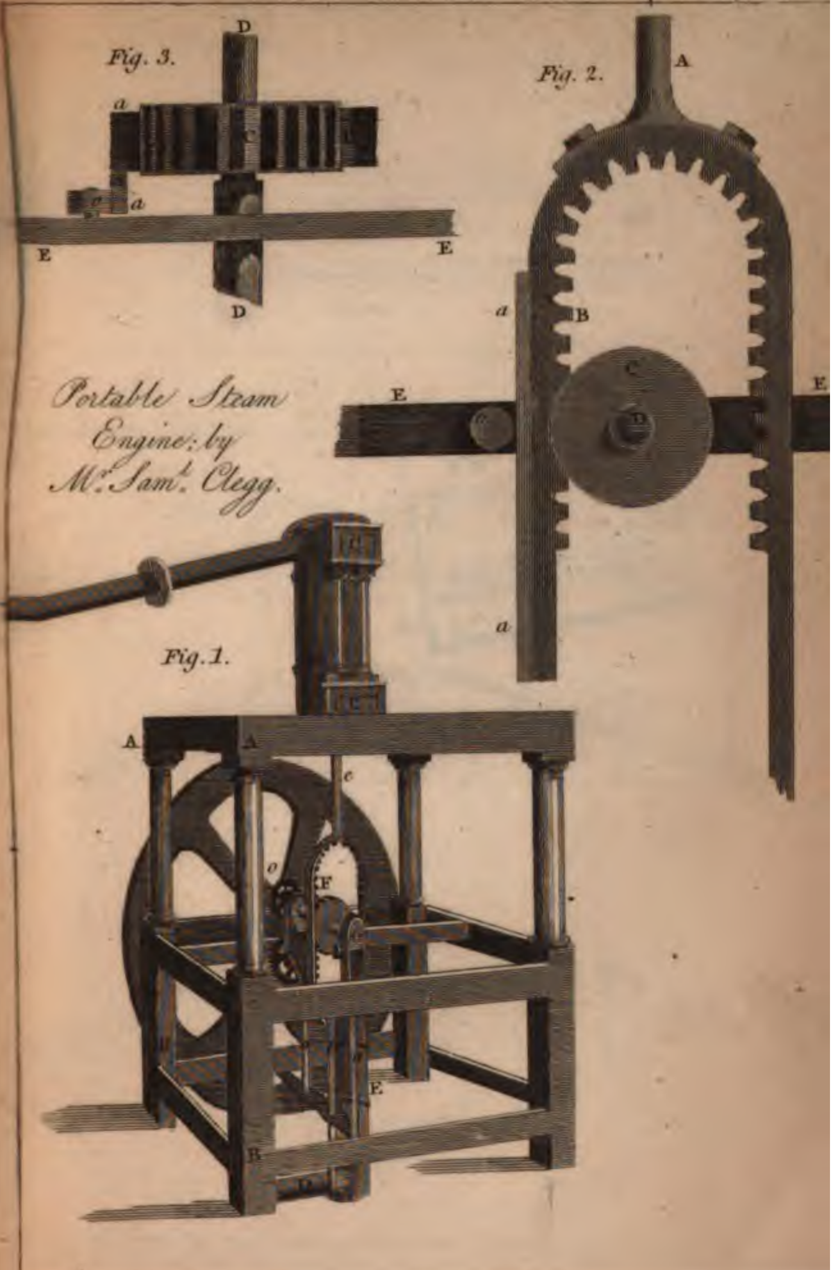
Fig. 1.



Natural Size.

[illegible]

Portable Steam Engine; by Mr. Saml. Clegg.



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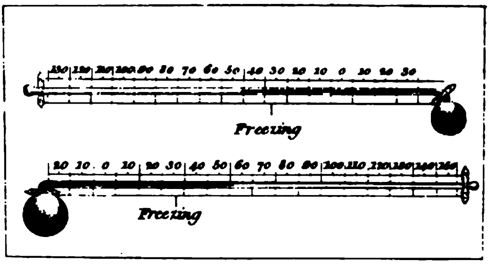
ASTOR, LENOX AND
TILDEN FOUNDATIONS
R L

Self registering Thermometer.

Fig. 1.



Plan of Mr. Bloore's Truss, by which the Roof of Clapham Church was raised.

Fig. 2.

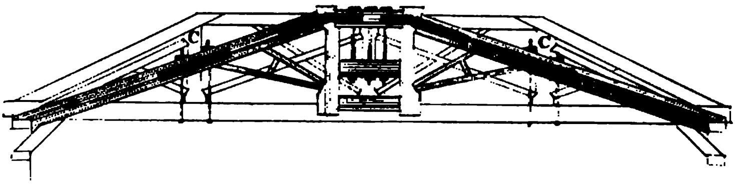
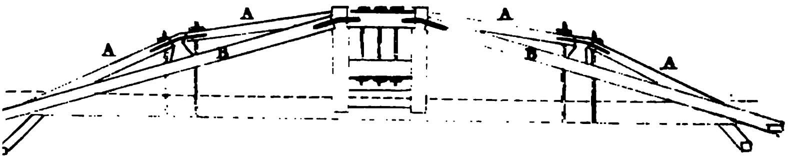


Fig. 3.



Mutlow Sc. Russell Ct.

[illegible]
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[illegible] AND
[illegible] FOUNDATIONS
L

Improved Sheep-fold
by T. Plowman Esqr.

Fig. 1.

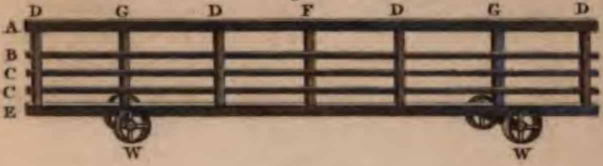


Fig. 3.

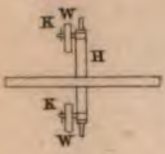


Fig. 2.

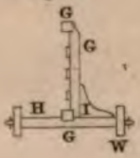
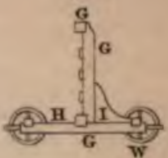
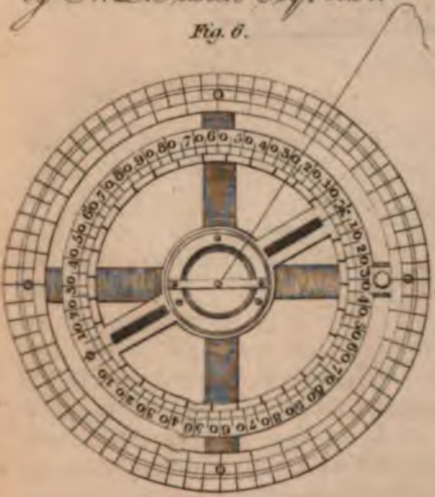


Fig. 4.



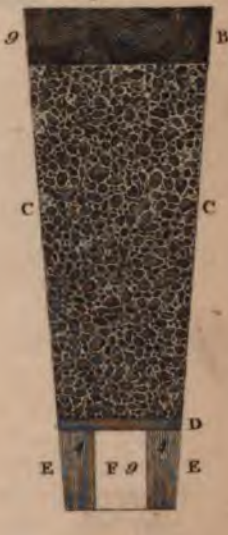
Applicative Compass
by N. D. Starck Esqr. R.N.

Fig. 6.



Drain, by
J. C. Curwen Esqr.

Fig. 5.



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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

DECEMBER, 1805.

ARTICLE I.

On the Diviſion of an Arch of a Circle into two ſuch Parts, that their Sines, or Coſines, or Verſed-Sines, ſhall have a given Relation. In a Letter from JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR,

Introductory letter.

BEING at preſent on a viſit to my friend Michael Fryer, teacher of the mathematics at this place, I have availed myſelf of the opportunity to conſult his very extenſive mathematical library, with a view to diſcover how far the following theorems and problems are original; thinking it poſſible, at leaſt, that ſimilar propoſitions might be met with in the works of the early geometricians, particularly in the tracts on Angular Sections, by Vieta, Oughtred, Wallis, and others, which I had never before been able to meet with; but I have found only one of them to have been already treated, of which notice ſhall be taken in its proper place: nevertheleſs, it is not improbable but that ſimilar theorems and problems are ſcattered up and down in the different works on geometry at preſent in exiſtence: As this eſſay, however, may claim the merit of ex-

exhibiting them in one view, and, which is equally deſirable, of deriving them from a general principle, I have ventured to offer it for inſertion in your Journal.

JOHN GOUGH.

Reeth, near Richmond, Yorkſhire,
Auguſt 28.

PROPOSITION I. THEOREM.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

Let A F be the arch of a circle, (*See Fig.* 1, *Pl.* XII.) A P a tangent at A; F P a perpendicular to A P, then A P is equal to the ſine of A F; F P, the part of the perpendicular intercepted by the tangent and the point F in the arch, is equal to its verſed ſine; and the ſame line, P M, intercepted again by the circle in M, is equal to the verſed ſine of its ſupplement.

Demonſtration.

Draw the diameter A K, and the ſine F S perpendicular thereto; alſo from the center O, draw O L at right angles to P M; then, ſince P A touches the circle in A, P A K is a right angle, (Euc. 16. iij.) Alſo, the angles F P A, A S F, are right, by conſtruction; therefore A S F P is a parallelogram, the oppoſite ſides of which are equal, namely, A P = the ſine S F, and P F = the verſed ſine A S, (Euc. 34. i.)

Again, ſince O L is perpendicular to P M, it is parallel to A P and S F, therefore P L = A O, or O K; and F L = S O, (Euc. 34. I.)—But F L = L M, (Euc. 3. III.) Conſequently P M = S K, or the verſed ſine of the ſupplement A F. Q. E. D.

PROPOSITION II. THEOREM.

If A F B be an arch of a circle, (*See Fig.* 2.) and A P, B R, be tangents at A and B, from any point, F, in the circumference, draw F P, F R, perpendicular to the two tangents, and F Q alſo perpendicular to the chord A B, then will the rectangle $PF \times FR = \overline{FQ}|^2$; and the rectangles $AP \times BR$, and $AQ \times QB$, will alſo be equal.

Demonſtration.

Join A F, F B, and the triangles P F A, Q F B, are equiangular, becauſe they are right-angled at P and Q, by conſtruction; and the angles P A F, Q B F, are equal, (Euc. 32. III.)

Therefore,

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

Therefore, as $AF : FB :: PF : FQ$.
Alſo, the triangles QFA, RFB, are equiangular, for the ſame reaſons.

Therefore, as $AF : FB :: FQ : FR$.

Conſequently, as $PF : FQ :: FQ : FR$, (Euc. 11. V.)

And $PF \times FR = \overline{FQ}|^2$, (Euc. 14. VI.)

Q. E. 1° D.

Again, by the ſame triangles, as $FA : FB :: AP : BQ$,

and as $FA : FB :: AQ : BR$;

hence, as $AP : BQ :: AQ : BR$,

Whence $AP \times BR = BQ \times AQ$:

Q. E. 2° D.

Corol. 1. Produce the perpendicular FQ till it meets the circumference again in G, and $PA \times RB = FQ \times QG$:

For $PA \times RB = AQ \times QB$ by the propoſition; but $AQ \times QB = FQ \times QG$, (Euc. 35. III.)

Corol. 2. If the lines PF, RF, meet the circle again in M and N, then will $PM \times RN = \overline{QG}|^2$:

For $\overline{AP}|^2 = FP \times PM$, and $\overline{BR}|^2 = FR \times RN$, (by Euc. 36. III.)

Therefore, as $\overline{AP}|^2 : FP \times PM :: FR \times RN : \overline{BR}|^2$:

But $\overline{AP}|^2 : FQ \times QG :: FQ \times QG : \overline{BR}|^2$, by Corol. 1.

And $PF : FQ :: FQ : FR$, by the propoſition.

Therefore,

$PF \times PM : FQ \times PM :: FQ \times RN : FR \times RN$.

Hence,

$FQ \times QG : FQ \times PM :: FQ \times RN : FQ \times QG$.

Conſequently, $PM \times RN = \overline{QG}|^2$.

Corol. 3. Draw the diameters AK, BD, and make FS, FT perpendicular to AK, BD; then $AK \times BT$ (the rectangle of the verſed ſines) $= \overline{FQ}|^2$; $SF \times FT$ (the rectangle of the ſines) $= AQ \times QB$; and $SK \times TD$ (the rectangle of the ſupplementary verſed ſines) $= \overline{QG}|^2$. Theſe things follow from Props. I. and II.

PROPOSITION III. PROBLEM.

To divide a given arch of a circle (AB) into two parts (AF, FB), ſo that the rectangle of their verſed ſines (AS, BT) may be equal to a given magnitude, or ſquare, ($m \times m$).

Conſtruction.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

From any point, Y, in the right line A B, draw Y W at right angles to the ſame, making it equal to the given right line *m*; through W, parallel to Y B, draw W F, and let it cut the arch A B in F, then will A F, F B, be the required arches.

Demonſtration.

Draw F Q perpendicular to Y B, then $FQ^2 = WY^2 = m \times m$; by Conſt. and Euc. 34. I.; but the rectangle of the verſed ſines of A F and F B $= \overline{FQ}|^2$, (by Cor. 3. Prop. II.); therefore this rectangle is equal to $m \times m$, the given ſquare. Q. E. D.

PROPOSITION IV. PROBLEM.

To divide A F B, a given arch of a circle, (*See Fig.* 3.) into two parts, A F, F B, ſo that the rectangle of their ſines may be equal to a given ſquare, $(n \times n)$?

Conſtruction.

To make the conſtruction general, let A F B be greater than a ſemicircle, join A B, and in it take Q, making $AQ \times QB = n \times n$; alſo in A B produced take *q*, ſo as to make $Aq \times qB = n \times n$; draw Q F, *q f g*, perpendicular to A B; then will A F, F B, or A *g*, *g* B, or A *f*, *f* B, be the required arches.

Demonſtration.

This is evident from Cor. 3. Prop. II. and the conſtruction.

To find *the limits*, biſect A B in Z, draw alſo the radius O N parallel to Z B, and make N E perpendicular to A B produced; then, if $n \times n$ be greater than $AZ \times ZB$, F is an imaginary point, becauſe $AQ \times QB$ cannot exceed $AZ \times ZB$, by Euc. 5. II. Again, if $n \times n$ be greater than $AE \times EB$, the points *f*, *g*, are imaginary, becauſe $Aq \times qB$ cannot exceed $AE \times EB$, ſeeing E N touches the circle in N, and is parallel to *q g*: Theſe things being premiſed, it will be eaſily perceived, that when A F B is leſs than a ſemicircle, it can only be divided in one point to anſwer the conditions of the queſtion, becauſe the point N will be in the oppoſite ſegment; but when it exceeds a ſemicircle, it will admit of being divided into one, two, or three points, according to circumſtances, or even the conſtruction may prove impoſſible. Q. E. D.

Scholium

Scholium.

This problem is conſtructed at page 342 of the Appendix to Simpſon's Algebra, 2d Edition; and at page 140 of his Select Exerciſes, 1ſt Edition; but the conſtructions given by that able geometrician do not ſhew the various limits of the queſtion with that degree of perſpicuity which appears in the preſent method.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines or v. ſines, in a given ratio.

Lemma.

Let A B C D be a ſquare, (*See Fig.* 4.) from any two adjacent ſides of which, C B, C D, take the ſegments T C, C S, then will the rectangle of the remaining ſegments $\mathrm{BT} \times \mathrm{SD} = \overline{\mathrm{BC}}|^2 + \mathrm{TC} \times \mathrm{CS} - \mathrm{BC} \times \mathrm{CT} - \mathrm{BC} \times \mathrm{CS}$.

Demonſtration.

Draw S G, T H, parallel to B C, C D, and let them interſect in F;—

Then the rectangle $\mathrm{FTCS} = \mathrm{TC} \times \mathrm{CS}$,
and the rectangle $\mathrm{FHAG} = \mathrm{BT} \times \mathrm{SD}$,
But $\mathrm{FHAG} + \mathrm{GBCS} + \mathrm{HFSD} =$ the ſquare A B C D; (Euc. 1. II.)
Add F T C S to both,
Then $\mathrm{FHAG} + \mathrm{GBCS} + \mathrm{TCDH} = \mathrm{ABCD} + \mathrm{FTCS}$;
But C D is equal to B C,
Therefore $\mathrm{FHAG} = \mathrm{ABCD} + \mathrm{FTCS} - \mathrm{BC} \times \mathrm{CT} - \mathrm{BC} \times \mathrm{CS}$;
That is, $\mathrm{BT} \times \mathrm{SD} = \overline{\mathrm{BC}}|^2 + \mathrm{TC} \times \mathrm{CS} - \mathrm{BC} \times \mathrm{CT} - \mathrm{BC} \times \mathrm{CS}$. Q. E. D.

PROPOSITION V. PROBLEM.

To divide A F B, a given arch of a circle, (*See Fig.* 5.) into two parts, A F, and F B; ſo that the rectangle of their coſines may be equal to a given ſquare, $k \times k$?

Conſtruction.

Join A B, and from the center, O, draw O Z perpendicular to A B; in Z O take Z V equal to the given line, k, and join B V; draw the diameter, H h, parallel to A B, and divide it in I ſo as to make $\mathrm{HI} \times \mathrm{I}h = \overline{\mathrm{BV}}|^2$; from I draw I Q perpendicular to A B, and when produced let it meet the given arch in F; then will A F, F B, be the required arches.

Demonſtration.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or verſ. ſines, in a given ratio.

Let F I meet the circle again in G, draw the diameters A K, B D, and the ſines F S, F T;

Then the coſine $QS = OA - AS$,

and the coſine $OT = OB - BT = QA - BT$;

Hence $SQ \times OT = \overline{AO}^2 + AS \times BT - AO \times AS - AO \times BT$;

But $AS \times BT = \overline{FQ}^2$, by Prop. II.

Therefore,

$SQ \times QT = \overline{AO}^2 + \overline{FQ}^2 - AO \times AS - AO \times BT$.

Again, $KS = 2AO - AS$,

and $DT = 2AO - BT$;

Hence, $KS \times DT = 4\overline{AO}^2 + AS \times BT - 2AO \times AS - 2AO \times BT$;

But $KS \times DT = \overline{QG}^2$, by Cor. 3. Prop. II.

Therefore,

$4\overline{AO}^2 + \overline{FQ}^2 - 2AO \times AS - 2AQ \times BT = \overline{QG}^2$;

Hence, $AO \times AS + AO \times BT = \overline{2AO}^2 + \frac{\overline{FQ}^2}{2} - \frac{\overline{QG}^2}{2}$:

But $SO \times OT = AO^2 + FQ^2 - AO \times AS - AO \times BT$

$= \frac{FQ^2}{2} + \frac{QG^2}{2} - \overline{AO}^2$.

$FQ = FI - IQ = BP - OZ$,

and $GQ = GI + IQ = BV + OZ$;

Hence $\frac{FQ^2}{2} + \frac{GQ^2}{2} = \overline{BV}^2 + \overline{OZ}^2$;

Conſequently, $SO \times OT = \overline{BV}^2 + \overline{OZ}^2 - \overline{AO}^2$;

But $\overline{AO}^2 - \overline{OZ}^2 = \overline{BO}^2 - \overline{OZ}^2 = \overline{BZ}^2$;

Therefore $SO \times OT = \overline{BV}^2 - \overline{BZ}^2 = \overline{VZ}^2$,

(Euc. 47. I.) $= k \times k$, by conſtruction. Q. E. D.

Limitation.—If Z V be greater than Z O, B V will be greater than B O; *i. e.* F G will be greater than H *h*, which is impoſſible, Euc. 15. III. therefore Z V, *or k*, cannot exceed Z O.

Proposition VI. Problem.

To divide A F B, a given arch of a circle, (*See Fig.* 6.) into two parts, ſo that the ſum of their verſed ſines may be equal to a given right line, *u*?

Conſtruction.

Diviſion of an arch of a circle into two parts, having their ſines, or coſine or v. ſines, in given ratio.

Draw the radius A O, and the tangent B E; in A O take A I equal to the given line, *u*; and making I V perpendicular to A O, let it meet B E in V; draw F V to biſect the angle E V I, and let it cut the given arch in F; then will A F, F B, be the required arches.

Demonſtration.

Draw the tangent A G, which is parallel to I V, alſo make F P, F R perpendicular to A G, B E, and let P F produced meet I V in H.

Then ſince A G, I V, are parallels, and the angle H P A is right, F H V is alſo a right angle, (by Euc. 29. I.) therefore it is equal to the angle F R V, (by conſtruction.)

But the angles R V F, H V F, are alſo equal, (by conſtruction); conſequently the triangles R F V, H F V, are equiangular; and they have one ſide common, namely the ſide V F; therefore F R = F H, (Euc. 4. VI.) and P F + F R = P H = A I, (Euc. 34. I.) = *u*, (by conſtruction.)

But the ſum of the verſed ſines of A F, F B, is equal to P F + F R, (by Prop. I.) therefore this ſum is equal to the given line, *u*. Q. E. D.

Limitation.—If A I be greater than the verſed ſine of the whole arch A B, the point F will evidently fall in the oppoſite ſegment, and the conſtruction will be impoſſible.

Again, ſince the angle I V E is equal to the angle A O B, draw the radius O C, to biſect the angle A O B, and it will evidently be perpendicular to V F; therefore L C, a tangent at C, will be parallel to V F; conſequently if A I be ſo taken, that V may lie in B L produced, the conſtruction will alſo be impoſſible; which will therefore happen when *u* is leſs than twice the verſed ſine of the arch A C, or B C.

Corol. Since the ſum of the verſed ſines of two arches is the ſame with the difference of the diameter and the ſum of the coſines, if the latter ſum be given the problem may be conſtructed by the laſt propoſition.

PROPOSITION VII. PROBLEM.

To divide A F B, a given arch of a circle, (*Fig.* 7.) into two parts, ſo that the ſum of their ſines may be equal to a given right line, *w*?

Conſtruction.

Conſtruction.

Diviſion of an arch of a circle into two parts, having their ſines, or coſines, or v. ſines, in a given ratio.

Draw the radii A O, O B, and the tangents A G, B E, in which take A S, B T, each equal to the half of *w*; draw S N, T N, parallel to A O, O B; and through their interſection, N, draw N F, parallel to S T, to meet the arch in F, then A F, F B, are the parts required.

Demonſtration.

Draw F K, F M, parallel to N S, N T, and let them meet S T in K, M, and A G, B E, in P, R; then it is eaſily proved that the triangles K F M, S N T, are equal and ſimilar, and that K M = S T; conſequently S K = T M.

But the angles K P S, M R T, are right, being equal to the angles O A G, O B E, by conſtruction; and the angles K S P, M T R, are equal; therefore the triangles P S K, R T M, are equiangular, they are therefore equal, (Euc. 4. VI.) becauſe S K = T M; conſequently S P = R T; therefore A P + B R = A S + B T = *w*.

But the ſum of the ſines of A F, F B = A P + B R; this ſum is therefore equal to *w*.

Limitation.—Join A B, which will be parallel to S F, alſo let the radius O C biſect the angle A O B, when properly produced, or not, it will paſs through the point N. Now if N be in O C produced, N F, being parallel to S T, or A B, will not meet the circle; on the other hand, if N lie between O and A B, F will be in the oppoſite ſegment of the circle, conſequently the conſtruction is impoſſible, unleſs N fall between C and the line A B, or in the verſed ſine of half the given arch: Theſe things being premiſed, it will be eaſily perceived that the ſine of the arch A F B is the leſs limit of the problem, and twice the ſine of A C its greater limit.

II.

Concerning the State in which the true Sap of Trees is depoſited during Winter. *By* THOMAS ANDREW KNIGHT, *Eſq*[*].

IT is well known that the fluid, generally called the ſap in trees, aſcends in the ſpring and ſummer from their roots, and that in the autumn and winter it is not, in any conſiderable quantity, found in them ; and I have obſerved in a former paper, that this fluid riſes wholly through the alburnum, or ſap-wood. But Du Hamel and ſubſequent naturaliſts have proved, that trees contain another kind of ſap, which they have called the true, or peculiar juice, or ſap of the plant. Whence this fluid originates does not appear to have been agreed by naturaliſts ; but I have offered ſome facts to prove that it is generated by the leaf [†]; and that it differs from the common aqueous ſap owing to changes it has undergone in its circulation through that organ : and I have contended that from this fluid (which Du Hamel has called the *ſuc propre*, and which I will call the true ſap,) the whole ſubſtance, which is annually added to the tree, is derived. I ſhall endeavour in the preſent paper to prove that this fluid, in an inſpiſſated ſtate, or ſome concrete matter depoſited by it, exiſts during the winter in the alburnum, and that from this fluid, or ſubſtance, diſſolved in the aſcending aqueous ſap, is derived the matter which enters into the compoſition of the new leaves in the ſpring, and thus furniſhes thoſe organs, which were not wanted during the winter, but which are eſſential to the further progreſs of vegetation.

The common ſap riſes in ſpring and ſummer, but not in winter.

True or peculiar ſap

exiſts in the alburnum during winter : Its ſubſequent deſtination pointed out.

Few perſons at all converſant with timber are ignorant, that the alburnum, or ſap-wood of trees, which are felled in the autumn or winter, is much ſuperior in quality to that of other trees of the ſame ſpecies, which are ſuffered to ſtand till the ſpring, or ſummer : it is at once more firm and tenacious in its texture, and more durable. This ſuperiority in winter-felled wood has been generally attributed to the abſence of the ſap at that ſeaſon ; but the appearance and qualities of the

That winter or autumn felled wood has its alburnum more firm, &c.

commonly attributed to the abſence of the ſap :

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[*] See Phil. Tranſ. of 1801, page 336.

[†] Philoſ. Tranſ. 1805, p. 88.

wood

—but probably to its preſence.

wood ſeem more juſtly to warrant the concluſion, that ſome ſubſtance has been added to, inſtead of taken from it, and many circumſtances induced me to ſuſpect that this ſubſtance is generated, and depoſited within it, in the preceding ſummer and autumn.

Full grown leaves perſpire moſt plentifully,

—and at this period the vegetative powers appear to be employed in increaſing the growth of the vegetable.

Du Hamel has remarked, and is evidently puzzled with the circumſtance, that trees perſpire more in the month of Auguſt, when the leaves are full grown, and when the annual ſhoots have ceaſed to elongate, than at any earlier period; and we cannot ſuppoſe the powers of vegetation to be thus actively employed, but in the execution of ſome very important operation. Bulbous and tuberous roots are almoſt wholly generated after the leaves and ſtems of the plants, to which they belong, have attained their full growth; and I have conſtantly found, in my practice as a farmer, that the produce of my meadows has been immenſely increaſed when the herbage of the preceding year had remained to perform its proper office till the end of the autumn, on ground which had been mowed early in the ſummer. Whence I have been led to imagine, that the leaves, both of trees and herbaceous plants, are alike employed, during the latter part of the ſummer, in the preparation of matter calculated to afford food to the expanding buds and bloſſoms of the ſucceeding ſpring, and to enter into the compoſition of new organs of aſſimilation.

If this be the caſe, it ſhould be found that the aqueous ſap muſt be altered in its aſcent; and the winter feiled wood will be denſer.

If the preceding hypotheſis be well founded, we may expect to find that ſome change will gradually take place in the qualities of the aqueous ſap of trees during its aſcent in the ſpring; and that any given portion of winter-felled wood will at the ſame time poſſeſs a greater degree of ſpecific gravity, and yield a larger quantity of extractive matter, than the ſame quantity of wood which has been felled in the ſpring or in the early part of the ſummer. To aſcertain theſe points I made the experiments, an account of which I have now the honour to lay before you.

Experiments. Birch and ſycamore in ſpring gave ſap moſt aqueous near the bottom; but denſer and more ſaccharine the higher up.

As early in the laſt ſpring as the ſap had riſen in the ſycamore and birch, I made inciſions into the trunks of thoſe trees, ſome cloſe to the ground, and others at the elevation of ſeven feet, and I readily obtained from each inciſion as much ſap as I wanted. Aſcertaining the ſpecific gravity of the ſap of each tree, obtained at the different elevations, I found that of the ſap of the ſycamore with very little variation, in different trees, to be 1.004 when extracted cloſe to the ground, and 1.008 at the height of ſeven feet. The ſap of the birch was ſomewhat lighter; but the increaſe of its ſpecific gravity, at greater elevation, was comparatively the ſame. When extracted near the ground the ſap of both kinds was almoſt free from taſte; but when obtained at a greater height, it was ſenſibly ſweet. The ſhortneſs of the trunks of the ſycamore trees, which were the ſubjects of my experiments, did not permit me to extract the ſap at a greater elevation than ſeven feet, except in one inſtance, and in that, at twelve feet from the ground, I obtained a very ſweet fluid, whoſe ſpecific gravity was 1.012.

The ſap firſt drawn was denſeſt; which ſhews that its augmentation was had from matter in the alburnum.

I conceived it probable, that if the ſap in the preceding caſes derived any conſiderable portion of its increaſed ſpecific gravity from matter previouſly exiſting in the alburnum, I ſhould find ſome diminution of its weight, when it had continued to flow ſome days from the ſame inciſion, becauſe the alburnum in the vicinity of that inciſion would, under ſuch circumſtances, have become in ſome degree exhauſted: and on comparing the ſpecific gravity of the ſap which had flowed from a recent and an old inciſion, I found that from the old to be reduced to 1.002, and that from the recent one to remain 1.004, as in the preceding caſes, the inciſion being made cloſe to the ground. Wherever extracted, whether cloſe to the ground, or at ſome diſtance from it, the ſap always appeared to contain a large portion of air.

It is difficult to make experiments on the denſity or ſp. gravity of the alburnum.

In the experiments to diſcover the variation in the ſpecific gravity of the alburnum of trees at different ſeaſons, ſome obſtacles to the attainment of any very accurate reſults preſented themſelves. The wood of different trees of the ſame ſpecies, and growing in the ſame ſoil, or that taken from different parts of the ſame tree, poſſeſſes different degrees of ſolidity; and the weight of every part of the alburnum appears to increaſe with its age, the external layers being the lighteſt. The ſolidity of wood varies alſo with the greater or leſs rapidity of its growth. Theſe ſources of error might apparently have been avoided by cutting off, at different ſeaſons, portions of the ſame trunk or branch: but the wound thus made might, in ſome degree, have impeded the due progreſs of the ſap in its aſcent, and the part below might have been made heavier by the ſtagnation of the ſap, and that above lighter

Method adopted. By felling poles in an oak coppice in winter and ſpring and comparing them.

lighter by privation of its proper quantity of nutriment. The moſt eligible method therefore, which occurred to me, was to ſelect and mark in the winter ſome of the poles of an oak coppice, where all are of equal age, and where many, of the ſame ſize and growing with equal vigour, ſpring from the ſame ſtool. One half of the poles which I marked and numbered were cut on the 31ſt of December, 1803, and the remainder on the 15th of the following May, when the leaves were nearly half grown. Proper marks were put to diſtinguiſh the winter-felled from the ſummer-felled poles, the bark being left on all, and all being placed in the ſame ſituation to dry.

The winter felled wood was denſeſt after ſeaſoning,

In the beginning of Auguſt I cut off nearly equal portions from a winter and ſummer-felled pole, which had both grown on the ſame ſtool; and both portions were then put in a ſituation, where, during the ſeven ſucceeding weeks, they were kept very warm by a fire. The ſummer-felled wood was, when put to dry, the moſt heavy; but it evidently contained much more water than the other, and, partly at leaſt, from this cauſe, it contracted much more in drying. In the beginning of October both kinds appeared to be perfectly dry, and I then aſcertained the ſpecific gravity of the winter-felled wood to be 0.679, and that of the ſummer-felled wood to be 0.609; after each had been immerſed five minutes in water.

—by more than ten per cent.

This difference of ten *per cent.* was conſiderably more than I had anticipated, and it was not till I had ſuſpended and taken off from the balance each portion, at leaſt ten times, that I ceaſed to believe that ſome error had occurred in the experiment: and indeed I was not at laſt ſatisfied till I had aſcertained by means of compaſſes adapted to the meaſurement of ſolids, that the winter-felled pieces of wood were much leſs than the others which they equalled in weight.

The difference was not quite ſo much in the newly formed layers of each.

The pieces of wood, which had been the ſubjects of theſe experiments, were again put to dry, with other pieces of the ſame poles, and I yeſterday aſcertained the ſpecific gravity of both with ſcarcely any variation in the reſult. But when I omitted the medulla, and parts adjacent to it, and uſed the layers of wood which had been more recently formed, I found the ſpecific gravity of the winter-felled wood to be only 0.583, and that of the ſummer-felled to be 0.533; and trying the ſame experiment with ſimilar pieces of wood, but taken from poles which had grown on a different ſtool, the ſpecific gravity of the winter-felled wood was 0.588, and that of the ſummer-felled 0.534.

The winter felled wood gave out a larger portion of extract.

It is evident that the whole of the preceding difference in the ſpecific gravity of the winter and ſummer-felled wood might have ariſen from a greater degree of contraction in the former kind, whilſt drying; I therefore proceeded to aſcertain whether any given portion of it, by weight, would afford a greater quantity of extractive matter, when ſteeped in water. Having therefore reduced to ſmall fragments 1000 grains of each kind, I poured on each portion ſix ounces of boiling water; and at the end of twenty-four hours, when the temperature of the water had ſunk to 60°, I found that the winter-felled wood had communicated a much deeper colour to the water in which it had been infuſed, and had raiſed its ſpecific gravity to 1.002. The ſpecific gravity of the water in which the ſummer-felled wood had, in the ſame manner, been infuſed was 1.001. The wood in all the preceding caſes was taken from the upper parts of the poles, about eight feet from the ground.

Probability that this ſap is exhauſted more or leſs by the leaves and ſhoots.

Having obſerved, in the preceding experiments, that the ſap of the ſycamore became ſpecifically lighter when it had continued to flow during ſeveral days from the ſame inciſion, I concluded that the alburnum in the vicinity of ſuch inciſion had been deprived of a larger portion of its concrete or inſpiſſated ſap than in other parts of the ſame tree: and I therefore ſuſpected that I ſhould find ſimilar effects to have been produced by the young annual ſhoots and leaves; and that any given weight of the alburnum in their vicinity would be found to contain leſs extractive matter than an equal portion taken from the lower parts of the ſame pole, where no annual ſhoots or leaves had been produced.

Experiment ſhewed that they leave a certain portion of extract in the trunk.

No information could in this caſe be derived from the difference in the ſpecific gravity of the wood; becauſe the ſubſtance of every tree is moſt denſe and ſolid in the lower parts of its trunk; and I could on this account judge only from the quantity of extractive matter which equal portions of the two kinds of wood would afford. Having therefore reduced to pieces ſeveral equal portions of wood taken from different parts of the ſame poles, which had been felled in May, I poured on each portion an equal quantity of boiling water, which I ſuffered to remain twenty hours, as in the preceding experiments: and I then found that in ſome inſtances the wood from the lower, and in the others that from the upper parts of the poles, had given to the water the deepeſt colour and greateſt degree of ſpecific gravity; but that all had afforded much extractive matter, though in every inſtance the quantity yielded was much leſs than I had, in all caſes, found in ſimilar infuſions of winter-felled wood.

Hence many trees have a ſucceſſion of leaves and buds.

It appears, therefore, that the reſervoir of matter depoſited in the alburnum is not wholly exhauſted in the ſucceeding ſpring: and hence we are able to account for the ſeveral ſucceſſions of leaves and buds which trees are capable of producing when thoſe previouſly protruded have been deſtroyed by inſects, or other cauſes; and for the extremely luxuriant ſhoots, which often ſpring from the trunks of trees, whoſe branches have been long in a ſtate of decay.

The matter in the alburnum may remain inactive for ſeveral years.

I have alſo ſome reaſons to believe that the matter depoſited in the alburnum remains unemployed in ſome caſes during ſeveral ſucceſſive years: it does not appear probable that it can be all employed by trees which, after having been tranſplanted, produce very few leaves, or by thoſe which produce neither bloſſoms nor fruit. In making experiments in 1802, to aſcertain the manner in which the buds of trees are reproduced, I cut off in the winter all the branches of a very large old pear tree, at a ſmall diſtance from the trunk; and I pared off, at the ſame time, the whole of the lifeleſs external bark.

Inſtance: in an old pear tree.

The age of this tree, I have good reaſon to believe, ſomewhat exceeded two centuries: its extremities were generally dead; and it afforded few leaves, and no fruit; and I had long expected every ſucceſſive year to terminate its exiſtence. After being deprived of its external bark, and of all its buds, no marks of vegetation appeared in the ſucceeding ſpring, or early part of the ſummer; but in the beginning of July numerous buds penetrated through the bark in every part, many leaves of large ſize every where appeared, and in the autumn every part was covered with very vigorous ſhoots, exceeding, in the aggregate, two feet in length. The number of leaves which, in this caſe, ſprang at once from the trunk and branches appeared to me greatly to exceed the whole of thoſe which the tree had born in the three preceding ſeaſons; and I cannot believe that the matter which compoſed theſe buds and leaves could have been wholly prepared by the feeble vegetation and ſcanty foliage of the preceding year.

It is ſtrongly probable that this matter compoſes the leaf.

But whether the ſubſtance which is found in the alburnum of winter-felled trees, and which diſappears in part in the ſpring and early part of the ſummer, be generated in one or in ſeveral preceding years, there ſeem to be ſtrong grounds of probability, that this ſubſtance enters into the compoſition of the leaf: for we have abundant reaſon to believe that this organ is the principal agent of aſſimilation; and ſcarcely any thing can be more contrary to every concluſion we ſhould draw from analogical reaſoning and compariſon of the vegetable with the animal economy, or in itſelf more improbable, than that the leaf, or any other organ, ſhould ſingly prepare and aſſimilate immediately from the crude aqueous ſap, that matter which compoſes itſelf.

It is not likely that they are ſupported by the crude ſap.

It has been contended[*] that the buds themſelves contain the nutriment neceſſary for the minute unfolding leaves; but trees poſſeſs a power to reproduce their buds, and the matter neceſſary to form theſe buds muſt evidently be derived from ſome other ſource: nor does it appear probable that the young leaves very ſoon enter on this office: for the experiments of Ingenhouz prove that their action on the air which ſurrounds them is very eſſentially different from that of full grown leaves. It is true that buds in many inſtances will vegetate, and produce trees, when a very ſmall portion only of alburnum remains attached to them; but the firſt efforts of vegetation in ſuch buds are much more feeble than in others to which a larger quantity of alburnum is attached, and therefore we have, in this caſe, no grounds to ſuppoſe that the leaves derive their firſt nutriment from the crude ſap.

Seeds are thus nouriſhed not from the ſoil, but from matter depoſited in the cotyledons.

It is alſo generally admitted, from the experiments of Bonnet and Du Hamel, which I have repeated with the ſame reſult, that in the cotyledons of the ſeed is depoſited a quantity of nutriment for the bud, which every ſeed contains; and though no veſſels can be traced[†] which lead immediately from the cotyledons to the bud or plumula, it is not difficult to point out a more circuitous paſſage, which is perfectly ſimilar to that through which I conceive the ſap to be carried from the leaves to the buds, in the ſubſequent growth of the tree; and I am in poſſeſſion of many facts to prove that ſeedling trees, in the firſt ſtage of their exiſtence, depend entirely on the nutriment afforded by the cotyledons; and that they are greatly injured, and in many inſtances killed, by being put to vegetate in rich mould.

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[*] Thomſon's Chemiſtry.

[†] Hedwig.

(To be concluded in the Supplement.)

III.

On the Deliqueſcence and Efflоreſcence of Salts. By C. L. Cadet.*

Deliqueſcence and efflореſcence are occaſioned by the relative attractions of the air and of each ſalt for water.

ALL chemiſts are of the ſame opinion relative to the cauſe of the deliqueſcence or the efflореſcence of a ſalt. The attraction of the ſalt for the water contained in the atmoſphere occaſions the firſt phenomenon, the attraction of the atmoſpheric air for the water of cryſtallization of the ſalt cauſes the ſecond.

The difference between one ſalt and another has been noticed, but not the variations from the ſtate of the air, &c.

This attraction has been found to vary in the different ſalts, whether deliqueſcent or efflореſcent, to be ſtronger in ſome kinds, and more ſpeedy in others; but no one has yet obſerved whether it had any dependence on the conſtitution of the atmoſphere, the electric ſtate of the air, the quantity of caloric it contained, if it was always the ſame in any one ſalt, and if it regularly became weaker in proportion as ſaturation approached, neither have any tables been yet prepared, which might indicate the degree of deliqueſcence, or of efflореſcence of the different ſalts.

Of the hypotheſes which could be made on theſe phenomena, the following ſeemed moſt probable.

Hypotheſes that deliqueſcent ſalts ſhould attract water in proportion as the hygrometer indicated its preſence.

The ſalts which deprived the air of its humidity ought to act in this reſpect in proportion to the quantity of water which the air held in ſolution or in ſuſpenſion. The greater the humidity of the air, the more ſhould the deliqueſcent ſalts augment in weight, ſo that the degree of their weight ſhould be conformable to the progreſs of the hygrometer.

Barometrical changes and

On the other hand atmoſpheric preſſure, which more or leſs oppoſes evaporation, ought to have an influence on the ſaturation of the ſalts, ſince it cauſes the denſity of the air to vary; conſequently there ſhould be an agreement between the variations of the barometer and the deliqueſcence of ſalts.

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* Journal de Phyſique, LX.

The variations of temperatures, by dilating, or by condenſing the maſs of the atmoſphere, ſhould alſo occaſion changes in the proportion of water abſorbed by the ſalts, on which account it would be uſeful to obſerve the thermometer.

—thermometrical ſhould alſo have their influence.

I thought moreover that one ſalt had not only more or leſs attraction for the water contained in the air than another, but that this attraction varied likewiſe in the ſame ſalt in proportion as it had loſt or abſorbed water. I hoped by thus comparing the deliqueſcence and effloreſcence of ſalts with the ſtate of the different meteorological inſtruments, to obtain reſults ſufficiently conſtant to eſtabliſh a theory of deliqueſcence or effloreſcence. I hoped alſo to be able to uſe the ſalts themſelves as inſtruments of meteorological obſervation; but experience proved that reaſoning apparently founded on the trueſt theory frequently deceives expectation. It is nevertheleſs neceſſary to attend to negative facts, which ſometimes are as ſerviceable to ſcience as thoſe of a poſitive nature.

Deliqueſcent ſalts ſhould attract moſt when leaſt ſaturated.

Experience did not confirm theſe poſitions.

I did not find a ſingle ſalt which ſeemed to have the leaſt conformity with the ſtate of the barometer, hygrometer, or thermometer. On the ſame day many ſalts increaſed conſiderably in weight, while others indicated a ſlow progreſs. Some had but a ſmall attraction, when the hygrometer ſhewed a great degree of humidity, and were moſt deliqueſcent when the air ſeemed moſt dry. Atmoſpheric preſſure never had the leaſt agreement with the increaſe of weight of a ſalt, and the thermometer having varied but half a degree during the courſe of the experiments, does not furniſh any obſervation on the influence of temperature. It is therefore impoſſible to explain by the meteorological changes any of the variations which I obſerved in the deliqueſcence, or the effloreſcence of ſalts.

None of the ſalts appeared to gain or loſe weight in conformity to meteoric changes.

Effloreſcent Salts.

I weighed exactly 288 grains of ſulphate of ſoda, of phoſphate of ſoda, and of carbonate of ſoda, which three ſalts are conſidered as the moſt effloreſcent, and placed them in a dry and airy ſituation, after having carefully dried the capſules which contained them. I put alſo in the ſame place an hygrometer, a barometer, and a thermometer: the three ſalts ſhewed the following reſults.

Experiments. Effloreſcent ſalts, ſulphate, phoſphate and carbonate of ſoda expoſed.

Loſs of weight in each by effloreſcence.

	Left to effloreſce.	Loſt.
Sulphate of ſoda	61 days	203 grains of water.
Phoſphate of ſoda	39	91
Carbonate of ſoda	51	86

It ſhould ſeem from this table that theſe three ſalts ought to be claſſed in the preceding order; but it muſt be obſerved that ſalts contain more or leſs water, in proportion as they cryſtalize ſlowly or rapidly. The number of days which were employed in the effloreſcence of theſe ſalts ſhould vary, both in proportion to the water they contained, and to the extent of ſurface which they expoſed to the action of the ſurrounding air; and therefore the time of their effloreſcence can give no appreciation of the force of their attraction for water. This reflection prevented my making experiments on any more effloreſcent ſalts.

Conſiderations why the time ſeems to indicate no uſeful reſult in deliqueſcent ſalts expoſed.

Deliqueſcent Salts.

I took 288 grains of each of the ſalts in the following table, (which are very ſenſibly deliqueſcent, ſince they all abſorbed more than half their weight of water), and placed each of them in a dried capſule, along with the before-mentioned meteorological inſtruments, in a damp ſituation, and after 150 days of obſervations noted what is included in the table.

A Table of Deliqueſcent Salts, in the Order of their Attraction, eſtimated by the Quantity of Water abſorbed.

Table of the increaſe of weight in each of 19 different ſpecies, and the times reſpectively.

	Days employed in their ſaturation.	Water abſorbed.
Acetite of potaſh	146	700 grains.
Muriate of lime	124	684
Muriate of mangaueſe	105	629
Nitrate of mangaueſe	89	527
Nitrate of zinc	124	495
Nitrate of lime	147	448
Muriate of magneſia	139	441
Nitrate of copper	128	397
Muriate of antimony	124	388
Muriate of alumine	149	342
Nitrate of alumine	147	300
Muriate of zinc	76	294
Nitrate of ſoda	137	257
Nitrate of magneſia	73	207

	Days employed in their ſaturation.	Water abſorbed.
Acetite of alumine	104	202
Acid ſulphate of alumine	121	202
Muriate of biſmuth	114	174
Acid phoſphate of lime	93	155
Muriate of copper	119	148

The times of abſorption were not at all proportional to the quantities.

In examining this table it may be remarked that the duration of the abſorption is not in any proportion to the quantity: The muriate of alumine, for example, took 149 days to abſorb 342 grains of water, while the nitrate of manganeſe took but 89 days to abſorb 527 grains. That the force of attraction may be eſtimated from the rapidity with which the bodies unite muſt not be concluded from this; for the ſame table ſhews that nitrate of magneſia ſaturated itſelf in 73 days, and only abſorbed 207 grains of water, a much leſs quantity than that taken up by the nitrate of manganeſe. Although the greater or leſs facility with which deliqueſcent ſalts ſaturate themſelves with water cannot be accounted for, (ſince a ſalt half ſaturated, or half deprived of water, is no longer the ſame body, and conſequently exerciſes other attractions than what the ſame ſalt does in its ordinary ſtate, or in a different ſtate of ſaturation,) the rapidity of their ſaturation is not however an indifferent matter. In the experiments which have been made on producing artificial cold by muriate of lime, it has been remarked that the cold was greater in proportion as the ice was melted; but it is probable that the muriate, and above all the nitrate of manganeſe, which becomes liquid much quicker, would produce with ice a more intenſe cold, and that certain liquors which have hitherto reſiſted coagulation, would be ſolidified by theſe two ſalts, which experiment is highly deſerving of a trial.

Though the rapidity of abſorption does not indicate the proportional affinity yet the facts are uſeful.

For inſtance, the ſalts of manganeſe may produce intenſe refrigeration.

It does not appear that the deliqueſcence depends on the proportion of component parts,

In order to examine whether deliqueſcence depends on the proportion of the baſe, or of the acid which conſtitutes the ſalts, I compared with each other the different analyſes of ſalts publiſhed by Bergman, Klaproth, Fourcroy, and Vauquelin, and I ſaw that no induction could be from their compoſition; for there are ſome ſalts which have the baſe in a very conſiderable proportion, and which are leſs deliqueſcent than thoſe whoſe baſe is leſs; and many others in which the acid is in a ſmall proportion, are more deliqueſcent than thoſe, in which this principle is predominant. The nature of the acids and of the

—nor on the peculiar nature of the ingredients themſelves.

the baſes themſelves do not throw more light on the phenomena of deliqueſcence than their proportions; for there are deliqueſcent ſalts, the component parts of which taken ſeparately, have not any remarkable attraction for water, ſuch is the nitrate of alumine; while on the other hand the ſulphate of ſoda is effloreſcent, although concentrated ſulphuric acid, and cauſtic ſoda each ſeparately attract humidity. Nothing better proves this axiom in chemiſtry,—*Compounds have properties peculiar to themſelves, and differing from thoſe of their component parts.*

Generally the deliqueſcence was moſt rapid when the ſaturation was leaſt.

In general deliqueſcent ſalts encreaſe their weight in a diminiſhing proportion, according as they approach ſaturation; thus the acetate of potaſh, which in the firſt twenty days exhibited the following progreſſion: 21. 34. 44. 54. 60. 70. 85. 100. 110. 120. 128. 138. 142. 148. 160. 169. 177. 186. 192. 198. did not ſhew on the laſt twenty days more than this, 647. 650. 655. 660. 663. 666. 669. 671. 676. 682. 684. 686. 688. 690. 692. 694. 696. 698. 699. 670. The ſalts which were but little deliqueſcent preſented a ſingular phenomenon, which none, I believe, has obſerved before.

Remarkable facts; ſalts which loſe part of the abſorbed water and afterwards attract more and encreaſe till ſaturation.

The acid ſulphate of alumine, and the acid phoſphate of lime, increaſed and diminiſhed ſucceſſively in weight.

The muriate of copper diminiſhed during 45 days before it began to encreaſe. Theſe oſcillations and retrograde movements take place but once, and when the ſalt has abſorbed a certain quantity of water, there is a progreſſive increaſe, although ſlowly, until its perfect ſaturation, which may depend on the attraction of water for water, an attraction which is not perceptible but in certain proportions.

Expediency of further experiments.

Theſe anomalies deſerve to be obſerved again, and compared with experiments made on other ſalts which do not exhibit them. They tend to make us acquainted with all the cauſes that produce effloreſcence and deliqueſcence, ſince they preſent each phenomenon ſucceſſively. The ſalts which we ſubmitted to their action, had certainly an attraction for water very little different from that of air in a medium ſtate of heat and humidity. The point of equilibrium muſt be decided by the ſtate of the atmoſphere, or the ſalts would remain unaltered.

Theſe will probably ſhew that meteoric variations in the air

I ſtill however think that a relation exiſts between the meteorological variations and the alterations of the ſalts; and if I was do influence th changes in ſalts

was not able to diſcover it, without doubt this was cauſed by the ſmall portions of ſalts which I expoſed to the action of the atmoſphere. Some chemiſt more fortunate will determine it, by operating on large maſſes, comparing experiments made in many different ſeaſons, and keeping a regiſter of the electrical ſtate of the atmoſphere, of the water of cryſtallization which the ſalts contain, of their diviſion, and of the ſurface which they preſent to the air.

Extenſive reſearch.

In a labour which would require more than 3000 experiments, the new facts which I have obſerved are too few, and perhaps too little important to engage any one to undertake ſuch prolonged and minute experiments; but I have given a table of deliqueſcent ſalts arranged according to their attraction for water, and I dare hope, that the reſults of it will not be altogether uſeleſs.

IV.

Account of the ſimple and eaſy Means by which the Harbour of Rye was reſtored, and made navigable for Ships of conſiderable Burthen. By the Rev. DANIEL PAPE *.

Memorial of Rye Harbour.

Decayed ſtate of Rye Harbour, in 1796.

RYE Harbour, once ſo very ſafe and convenient for paſſing veſſels up or down the channel, to run to in diſtreſs or in precarious weather, had been for many years, and from various cauſes, in a gradual ſtate of decay, inſomuch that in the years (I believe) 1795 and 1796, it was thought neceſſary to ſend Captain ———, from the Trinity-Houſe, to make a ſurvey, and report to the Board its then ſtate, and the probability of its improvement or redemption. The ſurvey was made, I believe, with conſiderable care and attention; and the reſult was, that the harbour was pronounced loſt, or in ſuch an irreparable decayed ſtate that it was an uſeleſs expenſe to the ſhips paſſing, which paid tonnage to it; and therefore this tonnage was taken from Rye, and given to Ramſgate Harbour, leaving however a reſerve in the hands of the commiſſioners of 6000*l.*

Survey and report,

that the Harbour was irreparable.

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* From his communication to the Society of Arts, who voted him the gold medal. See their Tranſactions, Vol. XXII.

Advertizement for plans of improvement.

The author's plan to make a direct cut, and dam up the old mouth.

The author undertook it at his own riſque,

and completed it.

Farther ſecurity by a pier head and jutties.

It proves to be perfectly durable,

The conſequence of this was an advertiſement, inviting any gentleman to come forward with plans for the improvement of the harbour, and the draining of the upper levels. On the day appointed for the preſentation of ſuch plans, a very ſenſible letter was laid before the Commiſſioners by the Rev. Mr. Jackſon, of Rye, though impracticable on many accounts,—and alſo a plan by myſelf, propoſing to make the preſent cut, and to form a dam of ſtraw or hay and faggots, as repreſented on the chart, for the ſmall ſum of 500*l.* On reverting to the *enormous ſums* that had been already, from time to time, expended by able engineers to no purpoſe, it was judged at the moment an impoſſible attempt; and, after politely voting me their thanks, the Commiſſioners ſeemed to decline carrying their plan into execution.—This, however, did not ſatisfy me; and therefore, confident of ſucceſs, I undertook to perform what I had propoſed, or loſe the money, without ſtipulating for any fee or reward ſhould I ſucceed. On entering upon this agreement, I ſet to work, and chooſing a Mr. Southerden, an active and perſevering man, as foreman, to aſſiſt me, I completed the work in three months, in the very depth of winter, at the expenſe of only 480*l.* though the works were twice filled up with ſea-beach by the tides. But, though this was done to the aſtoniſhment and admiration of many, yet there were evidently an envious few mortified and diſappointed. The cut and dam being thus finiſhed, it was then thought neceſſary, on my recommendation, to ſecure the cut from reverting to its late reduced ſtate, by a pier-head on the eaſt, and jutties on the weſt ſide of it; the execution of which was committed to the eminent ſkill of a Mr. Sutherland, who performed the truſt repoſed in him, to the univerſal ſatisfaction of his employers; and I believe the whole was completed for ſomething leſs than 3000*l*, in a very maſterly and workman-like manner. Of this I think there cannot be a better proof adduced, than that it ſtill ſtands *firm*, without the leaſt apparent decay, and maintains its firſt poſition without the ſmalleſt variation: and no doubt a very trifling annual expenſe will keep it in its preſent improved ſtate.

and admit ſhips of five times the tonnage before admitted.

The advantages derived from it are particularly great; for ſhips of 250 tons burden, and even veſſels of 300 tons, run in with the greateſt ſafety at ſpring tides: whereas, before, thoſe of 50 tons could not come in, but with the utmoſt difficulty and danger.

That

Other advantages.

That part of Romney Marſh too, which lies contiguous, and was threatened by every boiſterous tide with a total overflow, is now in ſafety, and the drainage of the levels is rendered complete.

Very eaſy and beneficial method of conſtructing the dam.

A double roof or covering of hay, &c. was pinned down upon a foundation of ſand, &c.

The interſtice was filled with gravel, and covered with faggots ſecured down.

I beg leave now to offer to your attention a ſhort deſcription of the Dam, the form and materials of which may be uſed with ſucceſs in ſimilar ſituations, whether in places adjacent to the ſea, or in gentlemen's fiſh-ponds, or rivers in the country, where weirs may be neceſſary for the preſervation of the banks. The dam was merely formed of hay, ſtraw, and faggots, pinned down to a *foundation* of *ſand* or *ſilt* by ſhort piles. I formed it as in the chart, of the ſhape of a double-roofed houſe, firſt putting down ſtraw, and then over it hazel faggots, from 12 to 14 feet in length, and afterwards pinning down the whole with piles. I next filled the ſpace between the two roofs with gravel or ſea-beach, and ſecured this alſo with faggots pinned down upon it, over which reſiſtance being precluded from its peculiar form, the influx and reflux of the tides glided ſo gently, that conſequently every probability, not to ſay poſſibility, was annihilated of its being ever undermined or blown up.

Difficulty that the old mouth ſhould be cloſed and the new one opened in one ſingle tide.

Succeſsful reſult.

It was alſo neceſſary that this dam ſhould be put down in one tide, and that the mouth of the cut ſhould be opened in the ſame time; for it was evident to me, that it was impoſſible ever to cut to ſea in any other way. For unleſs the dam had been ready to turn the water through the cut as ſoon as opened, and the cut, on the other hand, ready to receive the current the moment the dam began to act, the whole work muſt have been entirely and unavoidably deſtroyed by the influx and reflux of the enſuing tide. All this I clearly foreſaw: and by procuring a ſufficient number of men, nearly three hundred, the buſineſs was completely finiſhed, juſt as the tide touched the foot of the dam; and when it was full ſea, the ſtraw of courſe acted as a receiver and retainer to the *ſilt* brought in by the tide; which being repeated by each returning tide, the dam ſoon became entirely fixed, beyond a poſſibility of ever being deſtroyed; and it is now ſo entirely covered, that if the pier is kept in repair, the dam muſt ever remain unimpaired by time, and proof againſt the moſt violent floods of waters.

The author's reward for his contrivance, and

For this work, the Commiſſioners voted me fifty guineas (half of which I gave to my aſſiſtant) and alledged that, on

account

attending the works: 50l.!

account of the loſs of the tonnage, and the poverty of the fund, they were ſorry it was not more. This to me, under theſe circumſtances, was a ſufficient apology, and I was content. I now offer it to the conſideration of the Society of Arts, as a body in ſome degree intereſted in the proſperity of this kingdom. Should they deem what I have already received an adequate compenſation for ſuch a work, and ſuch an undertaking, at ſo inclement a ſeaſon, I am ſtill content. But if they ſhould think proper to grant me an additional remuneration, it will be received with peculiar ſatisfaction, and conſidered as a very great honour by,

Sir,

Your obedient and humble Servant,

To Charles Taylor, Eſq. DANIEL PAPE.

Cambridge, Trinity Hall,
April 2, 1803.

Reference to the Engraving of the Rev. Mr. PAPE's *Improvement of Rye Harbour, Plate* XIII. *Fig.* 1.

AA. The double roof, filled with ſtraw.

BBB. Hazel faggots, 12 to 14 feet long.

C. The ſpace betwixt the roofs filled with gravel or ſea-beach.

D. The faggots which covered the gravel ſo laid.

E. Piles of wood driven through the faggots and ſtraw into earth, at the bottom of the river, the heads of which piles are united by croſs pieces of wood.

F. The ſolid bed of the river.

G. The river at low water.

H. The high-water mark.

I. The upper ſide of the dam, which oppoſes the current of the river.

K. The lower ſide of the dam, which reſiſts the coming-in of the tide.

Fig. 2. L. Shows the place where the dam was placed.

M. The old courſe of the river repreſented by dotted lines, and which is now filled up with gravel by the tide.

N. The new canal, cut by Mr. Pape's directions, and which is now the regular channel for ſhipping.

OO. The pierhead, on the eaſt ſide of Mr. Pape's cut.

PP. The two jutties, on the weſt ſide of Mr. Pape's cut.

RR. The former canal, cut under the direction of Mr. Smeaton, and other able engineers; but which failed, and is ſince blocked up by a bank made acroſs it, over which the preſent high road between Rye and Winchelſea paſſes.

V.

New Experiments on the Reſpiration of Atmoſpheric Air, principally with regard to the Abſorption of Azote, and on the Reſpiration of the Gaſeous Oxide of Azote. By *Profeſſor* PFAFF, *of Kiel*[*].

Short hiſtory of chemical reſearches concerning reſpiration.

THE great diſcoveries in pneumatic chemiſtry, the ingenious and uſeful applications of theſe diſcoveries to explain the phenomena of organized beings, particularly the animal economy, and the valuable reſearches of eminent philoſophers have greatly contributed to throw light on the doctrine of the chemical effects of reſpiration. In conſequence of theſe reſearches, phiſiologiſts are in general agreed with regard to the moſt eſſential points of this doctrine; ſuch as the production of carbonic acid, the uſe of oxigen gas, and the animal heat which reſults from its abſorption. But the activity of philoſophical enquirers has not yet ſucceeded in removing all the obſcurities of this ſubject, and the diſagreement between the reſults of various experiments relating to them, ſufficiently ſhew, that new enquiries are requiſite to aſcertain the ſources of ſuch errors as ſtill continue, and to remove them altogether.

Davy on nitrous oxide.

The experiments of the celebrated Davy have done much in this reſpect, and the reſearches on the nitrous oxide afford a new epocha in the chemical doctrine of reſpiration. The celebrated editors of the Bibliotheque Brittannique, have ſhewn their conviction of the great value of theſe reſearches, by the ample and inſtructive extract they have given, and the manner in which Berthollet in the 45th volume of the *Annales de Chemie* has given an account of the ſame, has ſufficiently fixed the attention of philoſophers on that excellent work.

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[*] This memoir was addreſſed to the French National Inſtitute, and read at their ſitting of the 25th of Meſſidor laſt (July 13.)

The differences which exiſted in the reſults of former experiments, as to the quantity of carbonic acid produced in the act of reſpiration were leſs important, and might entirely depend on the conſtitution of the different individuals upon whom the experiments were made; and under this point of view, a reviſion of the experiments was leſs neceſſary. But the part which is performed by azote gas in the act of reſpiration has been too little attended to. It has been generally ſuppoſed to be altogether without activity. Goodwin alone thought he had obſerved a conſiderable abſorption of azote gas; but his experiments were not made with all the neceſſary accuracy, and were too directly oppoſite to the experiments of Lavoiſier, Seguin, Abernethy, Fothergill, Menzies, &c. to fix the attention. The experiments on the ſlow combuſtion of phoſphorus, which does not ſucceed in pure oxigen gas, but is ſo greatly forwarded by the preſence of the azote gas of atmoſpheric air, ſhew to a certain degree the advantages which this great quantity of azote gas is likely to produce in reſpiration; and the unfortunately too conciſe reſults of the laſt experiments of the immortal Lavoiſier on reſpiration, in which it was found, that a much greater maſs of oxigen gas is decompoſed in the ſame time by reſpiration in atmoſpheric air than in oxigen gas, ſtand in confirmation of the former fact. But hitherto we have poſſeſſed only probabilities, or reſults not ſufficiently connected with the ſubject. To Davy it is that we are indebted for an exact and inconteſtible knowledge of the active part which azote gas performs in the proceſs of reſpiration. But in proportion to the novelty and intereſting nature of theſe reſults do they require to be confirmed by the experiments of others; and it was in this point of view that I undertook laſt winter a ſeries of experiments upon reſpiration in atmoſpheric air, and alſo in the gaſeous oxide of azote; the principal reſults of which I now venture to communicate to the National Inſtitute.

Azote neceſſary for the ſlow combuſtion of phoſphorus.

Experiments on the Reſpiration of Atmoſpheric Air, and Oxigen Gas.

Experiments of reſpiration.

All the following experiments were made in the academical laboratory of the Univerſity of Kiel, which is provided with all the accurate apparatus of modern chemiſtry. They were made for the moſt part in the preſence of my pupils, particularly one named *Dierks*, who was moſt commonly the ſubject of experiment.

In order to determine with preciſion the changes which atmoſpheric air undergoes by reſpiration, and to decide reſpecting the abſorption of azote gas, we muſt begin with aſcertaining the diminution which a given volume of air undergoes by reſpiration. This firſt point was to be determined by accurate experiments.

Quantity of diminution undergone by air by the proceſs of reſpiration.

1. The quantity of 170 duodecimal cubic inches of Paris* were reſpired from one of the great reſervoirs of a gaſometer, conſtructed at Paris after the model of that of Charles, over water covered with oil, to prevent the abſorption of the carbonic acid gas produced by reſpiration. The reſpiration was performed once only during the time of ten or twelve ſeconds. The diminution was 4,72 cubic inches, or $\frac{1}{36}$ part of the firſt volume. This experiment being repeated twenty times in the ſame manner, afforded the ſame reſult.

2. 144 Cubic inches were reſpired once in the ſpace of ten or twelve ſeconds. The diminution was four cubic inches or $\frac{1}{36}$ part of the primitive volume.

3. The ſame volume was reſpired twice during 22 ſeconds, and the diminution amounted to eight cubic inches, or $\frac{1}{18}$ part of the primitive volume. The ſame volume having been reſpired three times during 30 ſeconds, the diminution amounted to 12 cubic inches, or $\frac{1}{12}$ part of the primitive volume.

4. 60 Cubic inches were reſpired three times during 25 ſeconds, the diminution was ſix cubic inches, or $\frac{1}{10}$ of the primitive volume.

5. 170 Cubic inches were reſpired four times during one minute, and the diminution amounted to 20 cubic inches.

This experiment was ſeveral times repeated, and the diminution was almoſt conſtantly the ſame. Namely, 18, 19, 21, and 20 cubic inches, or $\frac{1}{27}$.

6. 168 Cubic inches reſpired during 50 ſeconds, by four great and four ſmall reſpirations, ſuffered a diminution of 14, or $\frac{1}{12}$ of the primitive volume.

7. 430 Cubic inches by 12 reſpirations in 90 ſeconds, ſuffered a diminution of 24, or $\frac{1}{18}$ part.

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* As theſe quantities are merely relative, I have not reduced them. T.

Theſe reſults agree very well with thoſe obtained by Davy on the diminution of air by reſpiration. He found the diminution by one ſingle reſpiration to be $\frac{1}{17}$ part, and by reſpiration continued for one minute $\frac{1}{18}$ part.

The magnitude of the diminution depends not only on the time during which a given volume of air is reſpired, but principally on the magnitude of this volume itſelf; it muſt be proportionally leſs the greater the quantity of air inſpired. A very eſſential error is ſeen in the reſults of Abernethy, who gives a greater volume to the expired than to the inſpired air; and the calculations of Goodwin are founded on a miſtaken baſis; for he ſuppoſes the two volumes equal.

Diminution of oxigen gas by reſpiration.

In order to determine comparatively the diminution of oxigen gas by reſpiration, 170 cubic inches of oxigen gas obtained from manganeſe were reſpired in the ſame manner, and under the ſame circumſtances as the 170 cubic inches of atmoſpheric air in the 5th paragraph. The diminution amounted to 30 cubic inches, and in other experiments, to 33, 29, 31. The mean term of which is $\frac{2}{11}$ parts of the primitive volume.

This diminution being eſtabliſhed with accuracy, may be applied to determine the abſorption of azote gas.

Experiments to determine how much azote is abſorbed in the proceſs of reſpiration.

8. 80 Cubic inches were reſpired one time ſlowly during ten or 12 ſeconds, and the air expired was received over mercury.

The relative quantity of the conſtituent parts of this reſpired air was in the centenary 4,16 carbonic acid, 16,55 oxigen gas obſerved by the ſlow combuſtion of phoſphorus, 79,19 of azote gas. An eudiometric experiment made at the ſame time, gave the following proportion of the parts in atmoſpheric air, one carbonic acid, 21 oxigen gas, and 78 azote. The total diminution of the air was from the preceding experiments $\frac{1}{36}$. We may therefore find the true quantity of azote gas by the following proportion, 36 : 35 :: 79,19 : 76,99. If we ſubtract this 76,99 from 78, the primitive quantity of azote in the atmoſpheric air before reſpiration, we find a loſs of 1,01 on the hundred parts of the whole maſs of air breathed. But as the quantity of air inſpired was really no more than 80 cubic inches, the abſolute diminution or diſappearance of azote gas by one reſpiration, muſt be diminiſhed in the ſame proportion of 100 to 80, and thus proves 0,808 cubic inches.

9. In another experiment 60 cubic inches were reſpired once in the time of 10 or 12 ſeconds, and the laſt portion of the expired air was received over mercury. The proportion of the conſtituent parts after reſpiration, were in the centenary 4,68 carbonic acid gas, 17,68 oxigen gas, and 77,74 azote gas. An eudiometric experiment made at the ſame time on the atmoſpheric air, gave 1 carbonic acid, 22 oxigen gas, and 77 azote gas. The true quantity of azote gas found as before, by diminiſhing the 77,74 $\frac{1}{36}$, is, 75,58. And this being ſubtracted from 77, the quantity of azote gas previous to the reſpiration leaves 1,42 for the azote which diſappeared, ſuppoſing the reſpired air to be divided into 100 parts. But if we take the real number in inches, which was 60, this quantity will be expreſſed by 0,852 cubic inches.

10. 30 Cubic inches were reſpired in the ſame manner three times during 16 ſeconds. The expired air contained in the centenary 5 carbonic acid gas, 14,5 oxigen gas, and 80,5 azote gas. The atmoſpheric air contained by experiment at the ſame time, 1 carbonic acid gas, 29,75 oxigen gas, and 80,025 azote gas. This by the ſame proceſs of computation gives a diminution of 4,235 in the 100, or in cubic inches 1,2705.

Remarks on uſes, &c. of azote.

Theſe experiments which were ſeveral times repeated, and conſtantly with the ſame reſult, deciſively ſhew that azote gas is abſorbed in the act of reſpiration, and the active part it performs. Hence we may more eaſily underſtand, why azote gas compared with other mephitic gaſes is ſo little noxious to our lungs; ſo that according to the experiments of Lavoiſier and Seguin, animals live very well in a mixture of 15 parts azote gas, and one part oxigen gas; whereas the ſame animals were ſuddenly ſuffocated in a mixture of 40 parts oxigen gas, 45 azote gas, and 15 carbonic acid gas. Hence we may comprehend, at leaſt to a certain extent, the extraordinary effects of the gaſeous oxide of azote; we may form ſome notion of the transformation of the chyle, which is leſs anamalized or azotized in the lymphatic part of the blood, but becomes more ſo in the act of reſpiration. But the quantity of azote gas abſorbed by one ſingle reſpiration is not very conſiderable, which agrees perfectly with the experiments of Davy, who found that no more than 5,1 cubic inches of azote gas were abſorbed by 19 reſpirations of a volume of 161 cubic inches.

Experiments ſhewing the quantities of carbonic acid produced in reſpiration.

11. To determine the quantity of carbonic acid gas produced by the reſpiration of atmoſpheric air, 60 cubic inches were reſpired once during ten or twelve ſeconds, and received over mercury when expired. Lime water abſorbed 4,68 parts in 100. This experiment being ſeveral times repeated gave the ſame reſult. The laſt portion of expired air being ſeveral times transferred through lime water was diminiſhed 4,9 parts in 100.

12. 20 Cubic inches reſpired three ſucceſſive times during 10 ſeconds afforded no more than five hundredths of carbonic acid gas.

13. 170 Cubic inches were reſpired four times during 50 ſeconds, the quantity of carbonic acid gas obtained was 5,8 hundredths.

14. 170 Cubic inches were reſpired from a bladder eight times in one minute. Lime water abſorbed 8,2 hundredths.

This quantity of carbonic acid produced by reſpiration, afforded a term of compariſon to aſcertain the quantity of the decompoſition of oxigen gas in reſpiration from the ſame quantities of atmoſpheric air, and of pure oxigen gas.

Oxigen gas produces more carbonic acid in reſpiration than atmoſpheric air does.

The preceding experiments (7) had ſhewn that the diminution of oxigen gas was more conſiderable than that of atmoſpheric air. From this fact it might be expected, that the production of carbonic acid gas would likewiſe be more conſiderable; and this was confirmed by direct experiments.

15. 170 Cubic inches of oxigen gas obtained from manganeſe, were reſpired four times during 50 ſeconds; the diminution was 30 cubic inches. The quantity of carbonic acid produced was 8,2 hundredths. Atmoſpheric air reſpired in the ſame manner, and under the ſame circumſtances, gave only 5,8 carbonic acid.

16. 70 Cubic inches reſpired from a bladder during 50 ſeconds, alſo gave eight hundredths of carbonic acid.

Experiments on the Reſpiration of the Gaſeous Oxide of Carbon.

Obſervations on the method of obtaining gaſeous oxide of azote.

The gaſeous oxide of azote was obtained by the proceſs of Davy from cryſtallized nitrate of ammonia. This nitrate of ammonia affords very different products in different temperatures. I have made a conſiderable ſeries of experiments on this ſubject, which I ſhall ſhortly ſubmit to the National Inſtitute. I ſhall only remark in this place, that the oxigenated muriatic acid is obtained at the commencement, if the nitrate of ammonia be not entirely free from muriatic acid; that at a temperature not exceeding 220 degrees of the centigrade thermometer, the gaſeous oxide of azote is obtained in great quantity, and very pure, without any mixture of thoſe white vapours which have the taſte of muſtard; but that a temperature ſtill higher, eſpecially at a red heat, the gaſeous oxide of azote is no longer diſengaged but nitrous gas is formed, and very peculiar white vapours which I am at preſent examining. To prevent any exploſion, I always mix the nitrate of ammonia with very pure ſand. To obtain the gaſeous oxide of azote in a very pure ſtate, the diſtillation muſt be made on a ſand bath, and the fire carefully managed. When every thing ſucceeds properly, the gas is ſo pure, that it may be reſpired immediately; it has an agreeable taſte, almoſt ſacchar vinous. If it be mixed with the white vapours produced by too ſtrong a heat, time muſt be allowed for them to be depoſited. The effects which Davy has obſerved, and Pictet has deſcribed with ſo much intereſt in his ſecond letter in the 17th Volume of the *Bibliotheque Britannique*, were perfectly confirmed in my experiments. Several perſons who reſpired this gas were exalted abſolutely in the ſame manner. One of thoſe who reſpired it was very ſpeedily intoxicated, and put into a very extraordinary and moſt agreeable extacy. Others reſiſted ſomewhat longer; one only ſeemed to be ſcarcely at all affected. The exaltation always paſſed over without leaving any perceptible relaxation. I ſtill continue theſe experiments. Perhaps this gas may become a powerful remedy for melancholy affections. I ſhall not fail to communicate the reſults of my experiments to the National Inſtitute.

Davy's experiments ſucceeded perfectly with our author.

VI.

Experiments on Gum Arabic and Gum Adracanth.

By M. VAUQUELIN*.

Red gum adracanth left after combuſtion 3½ hundredths chiefly carbonate of lime, with a little iron and phoſphate of lime.

TEN grams of red gum adracanth produced on combuſtion three decigrams and a half of white aſhes. Theſe aſhes diſſolved in muriatic acid with efferveſcence, and gave forth an odour of ſulphurated hidrogen. Their ſolution depoſited a precipitate by ammonia, which was phoſphate of lime and oxide of iron. The oxalate of ammonia precipitated from it much lime. Thus red gum adracanth contains in 100 parts about 3½ of aſhes, which was compoſed for the moſt part of carbonate of lime, a ſmall quantity of iron, of phoſphate of lime, and perhaps of a very minute portion of alkali.

White gum adracanth left a reſidue of 3, which contained the ſame principles and alkali.

2. Ten grains of white gum adracanth ſubmitted to the ſame proofs, gave three decigrams of aſhes, which were compoſed of the ſame principles as the red kind, with the addition of a little potaſh.

Gum arabic left 3 containing no alkali.

3. Ten grains of gum arabic burnt as the others, left three decigrams of aſhes, which were compoſed of the ſame elements as the preceding, except that they gave no ſign of the preſence of alkali or of ſulphur.

Opacity and difficult ſolubility of gum adracanth.

I formerly thought that the opacity of gum adracanth, and the difficulty of its ſolution in water, might be occaſioned by a greater proportion of earthy matter; but after theſe experiments it appears, that they are due to another cauſe.

The lime in gums is combined with acid, forming a ſoluble ſalt.

The lime which I found in the gums, which I am about to mention, was doubtleſs neither in the ſtate of carbonate, and ſtill leſs in that of quicklime; for the ſolutions of the gum were not in the leaſt alkaline, but on the contrary, ſlightly acid; at leaſt a bit of the gum rubbed on ſome paper well moiſtened (with blue vegetable juice) made it ſenſibly red. It is alſo certain, that oxalate of ammonia and carbonate of potaſh occaſion precipitates in the ſolution of gum arabic, and that acetite of lead does not form any. It follows from this, that the lime is moſt probably united to ſome acid in the gums, which doubtleſs is a vegetable acid; for in being decompoſed they leave their baſes combined with carbonic acid; but can be neither the oxalic, the tartarous, or the citric, becauſe their combinations are inſoluble in water, and that beſides they exiſt but in a ſmall number of vegetables; ſtill leſs can it be the benzoic, the gallic, the *moroxalic*, or the *honeſtic*, which are very rarely found in naturally, and of which the three laſt alſo form inſoluble compounds.

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* Annales de Chimie, Tom. 54.

The acid muſt be either the acetous or the malic.

There only remains to decide between the acetous and the malic acids, which are the moſt abundant in the vegetable kingdom. The firſt forms, as is well known, ſoluble combinations with all the ſubſtances with which they are capable of union; ſome of them are even deliqueſcent. It is beſides the moſt frequent reſult of the operations of nature in the vegetable and animal ſyſtems, ſince it is formed by vegetation, by fermentation, the action of the more powerful acids, and by the influence of heat.

But the malic forms inſoluble compounds with lime,

The combinations of the ſecond are for the moſt part inſoluble in water; that which it forms with lime particularly, is not ſenſibly ſoluble, but when there is an exceſs of acid; and its exiſtence in nature is by no means ſo frequent as that of the acetous acid; and as the lime which is found in the tranſparent gums has been inconteſtibly diſſolved in the juices of the vegetables which produce theſe ſubſtances, it is much more probable, that this earth is in them combined with acetous acid than with any other.

conſequently the acid is more likely acetous.

The potaſh in gums alſo forms an acetite.

It is alſo probable, that the ſmall quantity of potaſh which I found in the aſhes of the burnt gums, is united to the ſame acid, which explains why theſe ſubſtances are ſo ſenſible to humidity, and ſoften ſo much as to prevent their pulverization.

Some gums contain lime in greater proportions.

I am, however, much inclined to think, that in certain opaque adracanth gums, which are of difficult ſolution, and yield much lime on incineration, this earth is combined with malic acid. I have had occaſion lately to examine a gum gathered by M. Paliſſot Bauvois, from the cochineal nopal, which was opaque, ſwelled with water, did not diſſolve uniformly, and which yielded eight per cent. of lime. And as the ſap of every cactus which I have analyzed, yielded more or leſs acidulous malate of lime, there is great reaſon to believe, that the ſpecies of it which nouriſhes the cochineal contains it likewiſe: and that it is the preſence of this ſalt proceeding from the vegetable, and diſſolved in the ſap along with

General reſults.

with the gum, which cauſes its opacity, and obſtructs its ſolution in water. It reſults at leaſt from theſe experiments, that the gums contain, firſt a calcareous ſalt, moſt commonly the acetate of lime; ſecondly, ſometimes a malate of lime with an exceſs of acid; thirdly, phoſphate of lime; fourthly and laſtly, ſome iron which is probably alſo united to phoſphoric acid.

VII.

Method of obtaining Cobalt pure. *By* M. Tromsdorf[*].

Zaffre detonated with charcoal and nitre.

FOUR parts of zaffre well pulverized are to be mixed carefully with one part of nitre, and half a part of charcoal in powder: this mixture is to be projected in ſmall quantities into a red hot crucible, and this operation repeated three times, adding each time to the reſidue new portions of the nitre and the charcoal.

Fuſion with black flux.

The maſs reſulting from theſe detonations ought to be mixed with one part of black flux, and expoſed during an hour in a crucible to a red heat.

The metallic button again detonated.

The whole is then to be left to cool; the metallic cobalt to be ſeparated, pulverized, mixed with three times its weight of nitre, and the mixture detonated with the precautions above mentioned.

Lixiviation ſeparates the acid and arſenic.

The iron contained in the cobalt will thus become ſtrongly oxidated, and the arſenic acidified combines with the potaſh: The maſs pulverized is to be lixiviated many times, and repeatedly filtered; and thus the arſeniate of potaſh formed will be ſeparated from the inſoluble reſidue that contains the cobalt,

Nitric acid diſſolves the cobalt alone.

This reſidue is then to be treated with nitric acid, which diſſolves the cobalt without attacking the iron which is found oxidated to its *maximum* of oxidation,

Evaporate and rediſſolve.

The ſolution is then to be evaporated to dryneſs, the reſidue rediſſolved in nitrous acid, and the liquor filtered, to ſeparate the laſt portions of the oxide of iron, which might have eſcaped in the firſt operation.

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[*] From the Annals de Chimie, Tom. 65.

Precipitate the cobalt by potaſh and reduce.

All that remains to be done after this is, to decompoſe the nitrate of cobalt by potaſh, to waſh the precipitate, and to effect its reduction by means of heat.

VIII.

A new Method of extracting Sugar from Beet-Root.

By M. ACHARD *.

Beet roots are ſliced, preſſed, and the juice ſeparated.

THE roots of the beet, after being properly cleanſed, are ſliced and preſſed. The juice obtained is thick, and of a deep colour: it contains, beſides ſugar, albumen, fecula, and other matters from which it muſt be cleared, in order to obtain the pure ſugar. In this proceſs of ſeparation it is, that the art of procuring ſugar from the beet-root conſiſts.

One 26th part of ſulphuric acid is added to the juice, and after ſtanding 1-12th of wood aſhes, and one part of lime. The albumen, &c. are thus ſeparated.

In a boiler of tin, or of tinned copper, mix 100 lbs. of the juice of beet root with 3¾ ounces of ſulphuric acid diluted with one pound of water; then pour it off, and let it ſtand for 12, 18, or 24 hours; 12 hours are ſufficient, but 24 will not be detrimental to the proceſs, as the acid prevents any change in the juice. In order to ſeparate the ſulphuric acid, put into the liquor 7½ ounces of wood aſhes, to which add ſoon afterwards, 2 ounces or 6½ drachms of lime ſlaked in water. The ſulphuric acid coagulates the albumen, the wood aſhes, conſiſting chiefly of lime, and the lime itſelf ſeparate in their turn the acid, in form of an almoſt inſoluble ſalt. It will here be recollected, that in the Weſt Indies, in the fabrication of coarſe ſugar, and in the refining houſes of Europe, lime is uſed to aſſiſt the ſeparation, and the cryſtallization of this article.

Farther purification by boiling, ſcumming, and ſtraining.

After this firſt operation the beet root muſt be clarified; for which purpoſe it muſt be poured into a boiler ſo placed as that the fire may act equally upon all the whole ſurface of the bottom, in which it is to be heated to a ſtate bordering upon ebullition, but muſt not be ſuffered actually to boil. After drawing out the fire, the ſyrup is to be ſkimmed till the ſkum ariſes in blackiſh flakes. The liquor is now to be filtered through flannel, which muſt be done with caution, leſt the dregs paſs through with the ſyrup. The ſkum and the dregs are good for fattening ſwine.

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* Van Mons's Journal, Vol. VI.

Briſk evaporation.

The ſyrup, thus clarified and filtered, is placed in a ſhallow cauldron, to the depth of not more than ſix inches, and evaporated over a briſk fire, whereby it is prevented from becoming a liquid ſaccharine mucus, which reſiſts all attempts to cryſtallize it.

Cooling and farther purification by ſubſidence.

Evaporation till the ſyrup draws a thread.

When reduced to about one half of its quantity, the ſyrup is to be poured into tin veſſels about ſix feet in height, and half a foot in diameter, with cocks about ſix inches from the bottom. It muſt here be left for two or three days, during which time it precipitates whatever remaining impurities it may contain, particularly gypſum. At the end of this period, the liquid may be drawn clear off, and replaced in the ſhallow boiler, but only to the height of three inches, to evaporate; the fire to be gradually augmented, as the ſyrup thickens, until it be in a ſtate of ebullition. The fire is then to be damped to prevent the ſugar from burning, which would render it unfit for cryſtallization.

When the ſyrup becomes fibrous, the fire is to be extinguiſhed.

Cryſtallization or graining in a warm apartment.

In about half an hour afterwards, the ſyrup is to be poured into cones, of which the mouths are ſtopped with linen cloths, and containing a little coarſe ſugar-candy, groſsly pounded. Theſe cones are ſet in a room whoſe temperature is from 10° to 20° of Reaumur.

When the ſeveral operations have been dexterouſly managed, the ſugar will cryſtallize in 24 hours: but if the evaporation, or baking, has been too haſty, the whole becomes a granulous maſs, with the interſtices filled with melaſſes.

The melaſſes drawn off

When the ſugar is well cryſtallized, the mouth of the cone is to be opened, and an earthern veſſel placed under to catch the melaſſes: this operation, according as the ſyrup has been more or leſs baked takes three or four weeks.

leave coarſe ſugar.

The ſubſtance remaining in the cones, of a yellow colour, more or leſs tinged with white as the baking has been well or ill conducted, in granulated cryſtals of various ſizes, is the coarſe ſugar of beet-root.

Improvement.

Mr. Achard, in order to ſave time, and to avoid the uſe of veſſels for ſettling the liquor, afterwards deviated from his original plan, by adding to the ſyrup, when half evaporated as above deſcribed, for every twelve quintals of roots uſed, five quarts of ſkimmed milk, and ſhortly afterwards one quart of vinegar. He then proceeded with the ſecond evaporation in the boiler.

Subſequent refining as uſual.

This ſugar by refining may be made to anſwer all the uſes of that of the Weſt Indies, and may be rendered equally white by the uſual proceſs.

IX.

On Nickeline (Niccolanum), *a Metal in many Reſpects reſembling Nickel, lately diſcovered by Dr.* J. B. Richter.*

Suſpicion of a new metal in cobalt.

I HAVE long ſince conjectured in analyſing the cobalt ores of Saxony, that they contained, beſides cobalt, arſenic, copper, nickel, and iron, another metal which reſembled nickel in many of its properties, but the means which I have hitherto employed to ſeparate it did not before afford me any ſatisfactory reſults.

Remarkable fact, that nickel cleared of iron and arſenic and fuſed, is almoſt diſperſed in ſmall globules through a hard, heavy, blackiſh-grey maſs.

I was chiefly ſurpriſed that nickel, after being purified by the liquid proceſs from cobalt, iron, and arſenic, and after that reduced without the addition of a combuſtible body, never formed a maſs, but was always found diſperſed in ſmall particles in a hard heavy maſs, which had the appearance of the remains from vitrified copper.

This hard matter had no metallic luſtre, neither was it attracted by the magnet: Its colour was of a blackiſh grey on the ſurface, with a ſmall degree of brightneſs; and in powder it was brown, greyiſh, and greeniſh.

Experiment with a large quantity of the oxide of nickel.

Some weeks ago I endeavoured to reduce *per ſe* almoſt half a pound of oxide of nickel, which I had purified as well as poſſible by the liquid proceſs, for the greateſt part of a year, at a conſiderable expence: as this oxide was not of a lively green, I thought this was cauſed by the "extractive matter" which might be in the potaſh employed for the precipitation of the ſulphate of nickel from the ammoniacal preparation: it is true that this triple combination had not that beautiful graſs-green colour which it commonly had; but I thought this might be cauſed by the ſubſtitution of the potaſh to the ammonia mixed with the copper, which could not be ſeparated but by the reduction *per ſe*.

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* *Annales de Chimie*, LXIV.

Only a ſmall quantity of nickel in a maſs was obtained.

From theſe ideas I hoped to have at leaſt four ounces of perfectly pure nickel, but was diſagreeably ſurpriſed by finding in the crucibles, which were deformed in the uſual manner, and perforated by the vitrified copper, a rough maſs with the appearance I have before mentioned, and which contained only a morſel of about two and a half drams, and conſequently only five drams of pure nickel in the two crucibles. I reduced to powder in an iron mortar the remaining maſs (which could not properly be called ſcoriæ), and ſeparated from it by the ſieve and the magnet, the particles of nickel which it might contain, which produced near two and a half drams more; and that nothing might be loſt, I treated the powder with nitric acid, which attacked it vigorouſly at the firſt, and gave a ſolution of nickel, but after that did not act on it in the leaſt, ſo that the powder was but little diminiſhed in weight: in expoſing this matter to reduction *per ſe*, it produced no regulus, but merely agglutinated its parts.

The denſe accompanying matter was then pulverized and the nickel ſeparated by the magnet and by nitric acid.

Strong heat applied to the maſs gave no more regulus.

The maſs being again powdered was urged with charcoal, and afforded more than half its weight of metal in one maſs.

Having again pulverized the maſs, which weighed almoſt 4½ ounces, I mixed with it one ounce of charcoal in powder, and expoſed to the fire of a porcelain furnace during eighteen hours, in a crucible cloſed with a luted cover, in a part of the furnace which ſeemed to me to have moſt heat. After having broken the crucible, which was in a ſound ſtate, I found, under a ſcoria of a deep blackiſh-brown colour, a well fuſed button of metal which weighed two ounces and three quarters: it was not at all connected with the adjoining parts of the ſcoria, and had at its inferior part a particular ſhape, which was cauſed by cavities which were not produced by the crucible.

It was ſteel colour, rather hard, ſcarcely malleable, magnetical, &c.

This metal had the grey colour of ſteel, inclining a little to red: it preſented in its fracture a grain not very fine: it was rather hard: could be extended a little under the hammer in a cold ſtate: heated to redneſs it endured little the ſtrokes of the hammer: it was attracted by the magnet, but not ſo ſtrongly as either iron or nickel: it had many properties common to nickel, but it was diſtinguiſhed from it entirely by others. As many of theſe properties were ſuch, that thoſe not well acquainted with nickel in its perfectly pure ſtate might take it for that metal, I have called it Nickeline (*Niccolanum.*)

Name *Nickeline.*

The nickeline was free from all the metals which are found in the cobalt ores, except a little copper.

Specific gravity 8,6. Nitric acid diſſolves it.

The ſpecific gravity of caſt nickeline, which enters more readily into fuſion than nickel, is 8,55; and of forged nickeline 8,60. On putting it into nitric acid and heating it, it is attacked more quickly than nickel: I remember having obſerved an equally violent action of nitric acid on nickel reduced by charcoal, which I then conſidered as pure, and which I diſſolved in order to precipitate from it by potaſh an oxide, which I might reduce *per ſe.*

The ſolution of the nickeline went on well; being come to the point of ſaturation, it had a blackiſh-green colour, and aſſumed a gelatinous conſiſtence.

Separation by drying of the acid. Reſidue a black powder.

I employed my firſt care to ſeparate from it a part of the iron which I thought it contained, and left it to dry a little over a ſpirit lamp: the maſs became continually of a deeper green, and in approaching to dryneſs it gave out much red vapours, and the reſidue became of a blackiſh grey; I added diſtilled water to it, which diſſolved but little of it, and that which was diſſolved was an inſignificant quantity of nickel.

Soluble in muriatic acid.

I poured muriatic acid on the blackiſh powder well waſhed, which gave a green ſolution, in diſengaging a ſtrong odour of oxigenated muriatic acid.

Green ſolution; which when dried gave a reddiſh maſs that turns green by damp.

The muriatic ſolution was, as well as the nitric ſolution, of a deep blackiſh graſs-green colour: being evaporated to dryneſs, it produced a reddiſh maſs, which became green in a moiſt air, and which communicated the green colour to water in which it was diſſolved.

Dark oxide of nickeline not ſoluble in nitric or ſulphuric acids without combuſtible matter.

This dark-coloured oxide of nickeline was inſoluble in nitric acid, and in ſulphuric acid; but if ſugar or alcohol was added, the ſolution took place with facility at the boiling point.

Sulphate of nickeline.

The ſulphate of nickeline, being combined with water, is alſo of a blackiſh green; but it aſſumes a pale red colour on being deprived of the water.

Precipitate by carbonate of potaſh;

If carbonate of potaſh be added to the preceding ſolutions of nickeline, it occaſions a precipitate of blue carbonate of nickeline, inclining a little to grey and green, and of a pale tint: This combination is very light and ſoft, and diſſolves in the acids with a ſtrong efferveſcence. I remember to have had, ſome years ago, this precipitate of a bad colour, and not then to have examined it, conſidering it as a mixture of iron, nickel, and arſenic, (which laſt continually made itſelf noticed by its odour of garlic): But at laſt I ſuſpected its nature.

by cauſtic potaſh;

If the ſolution of nickeline is decompoſed by cauſtic potaſh, it gives a precipitate which reſembles in its colour carbonate of chrome; that is to ſay, it is of a deep greeniſh-blue, which does not change when it is waſhed: being dried with a gentle heat, it aſſumes a pale colour, which becomes deeper when it is moiſtened with water.

by ammonia.

If any of the foregoing ſolutions of nickeline is mixed with ammonia to exceſs, the liquor aſſumes a pomegranate red colour, and remains tranſparent; which proves that it does not contain any iron, beauſe that this latter is not ſoluble in ammonia. By candle-light this ſolution is with difficulty diſtinguiſhed from that of perfectly pure nickel; but by day-light, this latter is of an amethyſt red colour, as I have elſewhere remarked.

Points of compariſon between nickeline and nickel or cobalt.

I ſhall now compare the principal properties in which nickeline reſembles altogether, or in part, nickel or cobalt, and thoſe in which it is diſtinct from them.

It reſembles cobalt—

Reſemblances of nickeline and cobalt.

1. By its property of ſuper-ſaturating itſelf with oxigen at the expence of the nitric acid, and thus forming a body which reſembles the black oxide of manganeſe with regard to its ſolubility in the acids: 2. By its property of not being reducible but by the intervention of a combuſtible body.

It differs from cobalt—

Differences between nickeline and cobalt.

1. By the blackiſh-green colour of its ſolutions, even when they are entirely neutralized. It is known that the neutral ſolutions of cobalt in the ſulphuric, nitric, and muriatic acids, are of a crimſon-red colour; and that the muriate of cobalt alone becomes of a greeniſh-blue on being deprived of its water: from whence it happens that an exceſs of acid produces this colour, becauſe it combines with the water: With the muriate of nickeline preciſely the reverſe takes place; when mixed with water it is green (although of a leſs beautiful colour than the cobalt without water), and when deprived of its water it becomes reddiſh.—2. By the colour of

its carbonate: that of cobalt is of a beautiful poppy-blue, but the carbonate of nickeline is a bluiſh-green inclining to a pale grey.—3. By the colour of its oxide precipitated without carbonic acid: that of cobalt is of a deep blue, and changes on waſhing to a blackiſh-brown; but this oxide of nickeline is of a greeniſh-blue, and its colour does not change.

Nickeline reſembles nickel—

Reſemblances between nickeline and nickel.

1. By its ſtrong magnetic quality; although this is not ſo great as that of nickel.—2. By its malleability, which however is leſs than that of nickel.—3. By the deep green of its ſolutions; although this colour is not ſo beautiful as that of the ſolutions of nickel.—4. By the loſs of this green colour when its neutral combinations are deprived of water.—5. By the colour of the acid ſolution with an exceſs of ammonia, which cannot be well perceived by candle-light.

Nickeline differs very diſtinctly from nickel—

Differences between nickeline and nickel.

1. Becauſe it cannot be reduced without a combuſtible body added to it.—2. Becauſe nitric acid attacks and oxidates it more eaſily. Nickel is not near ſo readily acted on by the nitric acid if it is not mixed with the nickeline, which almoſt always happens with the magnetic nickel which is conſidered to be in a ſtate of purity, and which has not been reduced *per ſe* before my diſcovery.—3. It alſo differs from nickel by the property firſt mentioned of thoſe in which it reſembles cobalt.—4. By the colour of its combinations with the acids, when deprived of water: This colour in nickel is almoſt a buff *(chamois)*, and in nickeline a reddiſh, except in the nitrate of nickeline, which cannot be deprived of water without decompoſing it.—5. By the colour of the precipitates, mentioned in the ſecond and third articles concerning the properties wherein this new metal differs from cobalt, which are in thoſe of nickel of a green colour entirely different from thoſe of nickeline, which latter are of a much more agreeable green, eſpecially thoſe of the carbonate.

X.

Letter from G. Cumberland, *Eſq. on a Project for extended Roads on the Principle of the inclined Plane.*

To Mr. NICHOLSON.

SIR, *Oct.* 26, 1805. *Weſton-ſupra-Mare.*

Account of rail-roads,

ABOUT ten years ago having frequent occaſion to remark, and ſuffer from, the miſerable ſtate of the roads from Staines to London during the winter ſeaſon, I ventured to propoſe (not having at that time either ſeen or heard of rail-roads) a plan which I called a *truck-road* for the whole of that ſtage, becauſe it was intended to convey all ſorts of goods and even carriages on trucks, going to town on one ſide of the old road and returning by the other.

communicated to Dr. Anderſon.

This plan I ſent ſome time after to Dr. Anderſon, with a drawing, for his *Recreations*, but by ſome accident it was miſlaid and loſt; and the reward of my trouble was the ſly ſneers of my grave Windſor neighbours, to whom it was known, accompanied with a ſort of pity for heads capable of propoſing ſuch eccentric inventions.

Time however revenged my cauſe, by ſhowing them the practicability of ſuch ſchemes in the progreſs of the Surry undertaking.

Another plan by an inclined road.

At the ſame time *another* plan of expeditious conveyance occurred to my mind, but which I was deterred from then producing owing to the cold reception my firſt contrivance met with—And as no one, as far as I can learn, has hitherto brought forward any improvement of the kind (although ſo very obvious that it might eaſily be ſuggeſted to the mind of a child who had heard of roads on inclined planes,) I take the liberty to recommend it to your patriotic publication, convinced that whatever may, at one time or other, be of ſervice to mankind, will be always ſure of a favourable reception at your hands.

Particular detail. Diſpatches may be rolled in a ſpherical caſe down a long inclined channel.

The plan I propoſe then is this:—That all diſpatches and poſt-letters may, wherever it is compatible with the inclination of the ground, be conveyed ten or fifteen miles to and from London by means of *iron or wooden ſhells of a globular form, rolling*

rolling in a cylinder of brick or ſtone. When cloſed and locked, a due momentum being given at a proper elevation, it is eaſy to ſee that their ſpeed and ſecurity muſt far ſurpaſs any other mode of conveyance that we at preſent know of; and all that would be neceſſary in addition to the machine would be to have proper beds of ſand or wool bags to blunt their projectile force at the end of their career.

I ſhall not at preſent enter into the diſcuſſion of the conſtruction of the tube-road, or go to a calculation of their expence; but if you think the bare hint worth publiſhing it will give me pleaſure, ſhould the idea be approved, to go into all the minutiæ of their utility in other reſpects, and the means of their ultimate accompliſhment;

Being always, Sir,
Your obliged humble ſervant,
G. C.

XI.

On the State of Provincial Societies for Scientific and Literary Improvement. By a CORRESPONDENT.

To Mr. NICHOLSON.

SIR,

Great advantage that would reſult to the town of Aberdeen from the eſtabliſhment of a public library, &c.

BEING in the cuſtom of viſiting Aberdeen, in one of my laſt tours, I inquired if there were any Antiquarian or Literary Society, or Subſcription Library there, and was much ſurprized to find neither one nor the other; there is, I was told, an Athæneum, in which a good number of newſpapers, and ſome of the moſt reſpectable periodical publications, are taken in, and in a room above that, a circulating library; this laſt I knew to be the property of two very reſpectable bookſellers in Aberdeen, and I believe the former is alſo, but the two united by no means effect the utility of either a literary ſociety, or a *ſubſcription* library, *in which the books, &c. are the property of the members, and whoſe concerns, ſuch as chooſing and ordering books, and the like, are conducted by a committee, choſen out of the ſubſcribers.* Few of thoſe who know that there is no ſuch inſtitution there, when they conſider the reſpectability of the place, either in a commercial or literary view, but muſt feel greatly aſtoniſhed; and more particularly will the want appear, when it is alſo known, that in Montroſe, Arbroath, Dundee, and Perth, places much ſmaller than Aberdeen, and not poſſeſſing any college eſtabliſhment, there are *ſubſcription* libraries, *on the above plan*; nay, that Perth hath alſo an Antiquarian Society! Subjoined is a liſt of ſome other places in North Britain, enjoying the advantages of ſuch eſtabliſhments as I would recommend to Aberdeen; ſome of whom, it is obvious, have not near the proſpect of ſucceſs that that place could command.

Glaſgow, Paiſley, Greenock,
Kilmarnock, Linlithgow, Haddington.
On the borders of Northumberland, Dunſe and Kelſo,

The annual ſubſcription to none *of theſe is more*, in ſome caſes *not ſo much*, as to the Athæneum of Aberdeen and others, and they all poſſeſs very excellent and increaſing ſelections of books.

I am, Sir,
Your's reſpectfully,
A TRAVELLER.

York Hotel, Bridge Street,
Black Friars.

and to other reſpectable and opulent places.

P. S. I am ſorry to be informed that neither Inverneſs, Banff, or Peterhead, poſſeſs ſuch inſtitutions, particularly the firſt, which preſents ſuch an abundant number of objects to the antiquarian, and is ſurrounded by, and contains, ſo many gentlemen of diſtinguiſhed liberality, and ingenuity; at this place the northern meeting was eſtabliſhed for the avowed purpoſe o promoting intercourſe amongſt diſtant families, but how much more might be effected of general amelioration and comfort, by the eſtabliſhment of a Literary and Antiquarian Society, in which ſubjects connected with general improvement might be diſcuſſed, and books in chemiſtry, agriculture, and other more immediately uſeful parts of knowledge, collected.

Notice

XII.

Notice of certain Inſtances of waſteful Negligence in ſome Fiſheries in the North. By an ENQUIRER.

To Mr. NICHOLSON.

SIR, *London, Oct.* 10, 1805.

Inſtances of waſteful negligence in ſome fiſheries.

IT is mentioned in the Statiſtical Reports of Banff and Peterhead, that the fiſhermen there never think of carrying their fiſh along the coaſt ſouthward, which they might do, to Leith, in 24 hours, or with a good briſk wind to Berwick-upon-Tweed, or even Newcaſtle-upon-Tyne: but when their reſpective towns are ſupplied *they throw the remainder upon the dunghills for manure!* this was poſitively affirmed to me as a truth, by a gentleman of great reſpectability of Aberdeen.—At Arbroath another cuſtom equally as extravagant in its kind prevails, and of which I have been a witneſs: the crab fiſhery there is ſo productive, that after boiling them, *the bodies of the crabs are thrown away*, and the *large claws only* brought to table! Ought not ſuch amazing waſte to be remedied? *

Your's reſpectfully,
An ENQUIRER.

XIII.

On Bile. By M. THENARD †.

Bile conſidered as a ſoap with albumen.

BILE has been commonly conſidered as a ſaponaceous liquor charged with albumen: but it has been found, upon cloſer inveſtigation, to preſent phenomena which cannot be accounted for merely by the preſence of theſe principles: this is more particularly to be obſerved on ſubmitting it to the action of fire and of acids.

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* *Qu.* What may the value of manure procured from fiſh at theſe places, compared with the price of the article at the neighbouring markets, ſubject to the deduction of carriage (coaſtwiſe), and the effect of a rival ſupply from nearer parts of the coaſt?—The facts which would ſolve this queſtion, would ſhew whether the fiſhermen neglect their intereſts in theſe proceedings.—W. N.

† Bulletin des Sciences, No. 95.

Deſtructive diſtillation leaves one-eighth, in which is only one-fifth of ſoda; and this cannot ſaponify the oil.

Bile, if diſtilled to dryneſs, leaves a reſiduum equal to $\frac{1}{8}$th of its original weight; from 100 parts of which calcined is obtained a carbonaceous matter, compriſing ſeveral kinds of ſalt; as marine ſalt, phoſphate of ſoda, ſulphate of ſoda, phoſphate of lime, oxide of iron, and four parts of ſoda. Bile therefore contains no more than $\frac{2}{100}$ parts of its weight of ſoda.

So ſmall a portion of alkali would not be ſufficient of itſelf to diſſolve that quantity of oil which is known to exiſt in bile: a fair preſumption may therefore be entertained that this liquor contains ſome other property to ſupply the abſence of alkali. This conjecture increaſes to ſtrong probability, if not to abſolute certainty, in attending to the action of acids on bile.

Acid ſeparates oil and albumen from bile; the clear fluid is bitter, and affords by evap. a reſiduum.

If a few drops of acid be mixed with bile, a liquor of a reddiſh tint is obtained, which ſtains paper of a bright yellow. In this experiment little or no precipitation is perceived; but on the addition of more acid, it takes place abundantly: the matter depoſited conſiſts of albumen joined with a very ſmall portion of oil, not at all correſpondent to the quantity of theſe ſubſtances to be found united in pure bile. The liquor remaining after filtration is of an extremely bitter taſte, and leaves on evaporation a reſiduum equal to what is obtained from a like quantity of bile in its original ſtate,

The oil with an alkali and albumen is not bile.

On diſſolving the oil, which had been previouſly obtained from bile, in alkali, and adding to the ley produced, a portion of albumen, a combination took place which was decompoſed by the moſt feeble acids, and from which vinegar precipitated all the oil. This combination, therefore, was not bile; conſequently bile conſiſts not merely of albumen, oil, and ſoda; and this is the reaſon why ſoluble ſalts, barites, ſtrontian, lime, and ſeveral metallic diſſolvents, make no impreſſion upon bile. No longer doubting that there exiſted in bile a matter peculiar to itſelf, I endeavoured to ſeparate it; and after a few trials, I ſucceeded, by means of a combination of acetous acid with lead.

Bile contains a peculiar matter.

Acetite of lead precip. the oil and alb. The liquor by evap.

On pouring into bile acetite with a ſlight exceſs of oxide of lead (that is, acetite of common lead boiled with about the 6th part of its own weight of litharge deprived of its carbonic acid)

acid) the whole of the albumen and oil were precipitated; the liquor being filtered, the oxide of lead and acetite were ſeparated from it by means of ſulphurated hidrogen; and by evaporation, after having again filtered the liquor, a ſubſtance was obtained whoſe flavour was at once ſaccharine and acrid, ſomewhat ſimilar to the juice of certain kinds of liquorice. But as this ſubſtance was ſtill ſuppoſed to be charged with the ſalts of the bile, changed into acetite, by the acetite of lead, it was precipitated with acetite ſuper-ſaturated with oxide of lead, containing one part of the quantity of acid found in common lead; the precipitate was diſſolved in vinegar, to free it from the ſulphurated hidrogen, filtered, and again evaporated; by which means the matter was obtained in its greateſt purity.

gave a ſubſtance which when pure

Its principal qualities are:

had the peculiar qualities here enumerated.

1. Being ſoluble in water, and in alcohol, ſlightly deliqueſcent.

2. It is not precipitated by acetite of common lead; but is entirely ſo by the ſaturated acetite of lead, which precipitate is ſoluble in acetite of ſoda.

3. It will not ferment with yeaſt; will give no ammonia by diſtillation; and is not affected by the preſence of nutgall.

4. It diſſolves the oily matter of bile: but to facilitate this operation, it is neceſſary to diſſolve the two matters together in alcohol, evaporate, and waſh the reſiduum in water. One part of the ſaccharine and acrid ſubſtance diſſolves only three-fourths of the oily matter. Now, as theſe matters are nearly in equal proportions in bile itſelf, it muſt be admitted that ſoda contributes towards the complete diſſolution of the oil; nevertheleſs acids ſcarcely, if at all, affect it.

Bile conſiſts of little ſoda and much oily and ſaccharine matter, &c.

In reflecting on the above experiment and its reſults, I concluded that bile was a triple compound of a little ſoda and much oily and ſaccharine matter; that acids decompoſed it but in part; in other words, that it was capable of containing an exceſs of acid without having its portion of ſoda neutralized. I therefore calcined bile that had been acidulated with ſulphuric, muriatic, and other acids, and found in each caſe the ſoda left in the calx: it is therefore very probable that the ſaccharine matter, in conjunction with the oil, decompoſed a certain quantity of marine ſalt, and deſtroyed the power of the acid.

Determination of component parts or bile.

It would have been of little ſervice to deſcribe the conſtituent parts of bile, had their proportions been left unaſcertained; I have therefore endeavoured to determine them in the following analyſis:

Analyſis.

By means of nitric acid, I ſeparated the animal ſubſtance, which is ſuppoſed to be albumen, with a very ſmall portion of oil: this being ſoluble in alcohol and the other not, it was eaſy to aſcertain the weight of each. I then precipitated all the oily matter, with acetite and a ſmall exceſs of oxide of lead: this precipitate being mixed with the metallic oxide, I diſſolved it in weak nitric acid; after filtering the liquor, I deprived it of the lead which remained, by means of ſulphurated hydrogen; and by evaporation, I obtained the peculiar ſubſtance, mixed, indeed with the ſalts of the bile, which had moſtly undergone a change by the acetite of lead, and whoſe weight had noted.

I aſcertained the quantity of ſoda by calcining 100 parts extracted from bile, and comparing with much care how much the reſiduum would imbibe of acid at 16°, with the quantity imbibed by pure ſoda. I alſo, by means not neceſſary to ſtate here, obtained the quantity of each of the other ſalts contained in bile; from all which experiments, made with the utmoſt care, I conclude that 800 parts of the bile of an ox contain—

Numerical reſult.

Water - - -	700 Parts
Oily matter - - -	43
Particular ſubſtance - -	41
Animal ſubſtance - -	4
Soda - - -	4
Marine ſalt - - -	3.2
Sulphate of ſoda - -	0.8
Phoſphate of ſoda - -	2
Phoſphate of lime - -	1.2
Oxide of iron - - -	0.5
	799.7

N. B. This calculation is $\frac{3}{10}$ deficient of the given quantity.

Bile forms an intereſting ſubject for a number of other reſearches: the varieties to be found in the ſeveral ſpecies of animals, and which a multitude of circumſtances, particularly a morbid affection of the organ which ſecretes it, may modify;

the calculi which are there formed, and are of a peculiar nature; the oleaginous and animal ſubſtances; and that particular matter, differing from all others hitherto known; will not fail to excite a lively intereſt, and are the ſubject of ſeveral other Papers which I purpoſe ſhortly to bring before the public.

XIV.

Quotation from Sir GEORGE STAUNTON's *Embaſſy, containing a Deſcription of Fire Works unknown in Europe. Propoſed by a Correſpondent with a View to obtain Explanation of the Means by which they were produced.*

To Mr. NICHOLSON.

SIR,

Introduction.

I preſume to think it will accord with the general aim of your excellent collection to inſert the following quotation; and I indulge the hope that your compliance with my requeſt for that purpoſe may produce an explanation from ſome of your ingenious correſpondents.

I am Sir,

Your conſtant reader,

P. M.

Remarkable fire-works of the Chineſe.

"After the ballets, Fire-works were played off; and even in the day-time had a ſtriking effect. Some of the contrivances were new to the Engliſh ſpectators. Out of a large box, among other inſtances, lifted up to a conſiderable height, and the bottom falling out as if it were by accident, came down a multitude of paper lanterns, folded flat as they iſſued from the box, but unfolding themſelves from one another by degrees. As each lantern aſſumed a regular form, a light was ſuddenly perceived of a beautifully coloured flame, burning brightly within it; leaving doubtful by what deluſion of the ſight thoſe lanterns appeared, or by what property of combuſtible materials they became thus lighted, without any communication from the outſide to produce the flame within. This devolution and developement were ſeveral times repeated, with a difference of figure every time, as well as of the colours, with which the Chineſe ſeem to have the art of clothing fire at pleaſure. On each ſide of the large box, was a correſpondence of ſmaller boxes, which opened in like manner, and let down a kind of net work of fire, with diviſions of various forms, which ſhone like burniſhed copper, and flaſhed like lightning at every impulſe of the wind. The whole ended with a volcano, or eruption of artificial fire, in the grandeſt ſtile."—*See Staunton's Embaſſy to China, Volume* III. *page* 73.

XV.

On the Carbonate of Potaſh. By M. STEINACHER [*].

Carbonate of potaſh requires to be formed by paſſing the gas through a cold alkaline ſolution till it cryſtallizes, and not by evaporation.

CHEMISTS know that carbonate of potaſh well ſaturated, ſo as to effloreſce, can only be formed by making paſs through a cold alcaline ſolution a quantity of carbonic acid ſufficient to cauſe a ſpontaneous cryſtallization. For on ſtopping the diſengagement of gas at the moment when the earth of the alkali appears to be depoſited, and evaporating the liquor by a mild heat, there are only laminated cryſtals obtained, which ſoon deliqueſce.

The gentle heat of Curaudeau does not ſucceed.

Alkaline ley warmed by heat of tan, according to the method of M. Curaudeau, does not ſucceed any better in forming a well ſaturated alkali by evaporation. I have experienced that its cryſtals grow moiſt, and the author himſelf acknowledges a ſlight deliqueſcence.

Welter's apparatus is too complicated; and ſo is that of Pelletier.

It is generally agreed, that the apparatus of Welter, deſcribed in the twenty-ſeventh Volume of the Annales de Chimie, is too complicated, and that of M. Pelletier has been adopted in its place in almoſt all laboratories, with ſome alterations in the diſpoſition of the firſt bottle, to which is fixed a tube with a double or triple perpendicular curvature, for the introduction of the acid, or a long pipe of glaſs terminated in a tunnel. The chalk mixed with water to the conſiſtence of thin ſoup, is poured by degrees into this pipe, which is ſtopped by a glaſs rod accordingly, and the gas is forced to traverſe the bottles containing the alkaline ley.

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[*] From the Annales de Chimie, Tom. LV.

This method is in my opinion attended with much inconvenience, for when a tube with many perpendicular flexures is uſed, it muſt be charged with a column of the fluid ſufficient to counterbalance the preſſure of the carbonic acid gas, and conſequently to give an elevation which expoſes it to be eaſily broken; and when a long horizontal pipe of glaſs is employed, it often happens that the chalk is exploded into the air, when the piſton is opened.

The reaſon ſtated.

Another method appears to be more ſimple and commodious than this, which is ſomething like that of M. Brugnatelli, but the Italian chemiſt has not publiſhed the details of his method, without which it is impoſſible to be followed.

Simpler method not yet deſcribed.

A kilogram of chalk in powder is to be put into a bottle with two or three necks, capable of containing 12 kilograms of water; on this is to be poured a litre of a mixture of one and a half kilogram of vitriolic acid, with nine kilograms of ſpring water: the gas is expelled, and a cruſt of ſulphate is formed at the ſurface of the calcareous carbonate. At the end of two hours all the reſt of the acid water is to be added, and the bottle ſtopped quickly. Bubbles of the gas will be rapidly diſengaged, but they will by degrees be diſcharged more ſlowly, and continue to be ſo moderately for twenty-four hours; then the mixture is to be ſtirred with an iron rod, and the gas will continue to be developed for 24 hours more, with little interruption.

I found the term of the effervеſcence prolonged by the reſiſtance which was oppoſed to the action of the ſulphuric acid on the chalk by the denſity which the combination neceſſarily acquired; which denſity the tendency of the ſulphuric acid to augment the ſolubility of the ſulphate of lime, fixed to proper limits.

As my apparatus, with the exception of the firſt bottle, is the ſame of that of M. Pelletier deſcribed in the fifteenth Volume of the Annales, I ſhall not ſpeak of it, but only make the following remarks:

1. Pelletier was wrong in neglecting to uſe an intermediate bottle half filled with water: which is very neceſſary to ſeparate the ſulphuric acid which the gas always brings over.

2. The tubes of an inch diameter, being too difficult to be bent, may be replaced by others of ſeven or eight lines aperture, which will do equally well.

3. The alkaline ſolution, made by two lb. alkali to three lb. water, cryſtallizes too quick, and before the precipitation of the ſilex.

4. The doſes of alkali and water moſt favourable to a regular cryſtallization, at the temperature of from 5 to 10 above 0 of Reaumur, are one part of diſtilled water, and half a part of purified potaſh.

If the reſults of my experiments ſhall improve the preparation of carbonate of potaſh, and if the diſpoſition of my apparatus prevents the neceſſity of continually watching its direction, by procuring without trouble that gentle and continued preſſure, of which Pelletier perceived the efficacy for the ſaturation of the alkali, I have reaſon to think that the true friends of chemiſtry, I mean thoſe who practiſe it, will conſider my obſervations with indulgence.

XVI.

*Method of preparing a luminous Bottle, which long preſerves its Effect.**

A luminous bottle. Put a ſmall piece of phoſphorus in a long phial: pour on it boiling oil. The fluid will give light in the vacuous ſpace whenever the cork is pulled out.

IT is eaſy to prepare a bottle which ſhall give ſufficient light during the night to admit of the hour being eaſily ſeen on the dial of a watch, as well as other objects, by the following means.

A phial of clear white glaſs, of a long form, ſhould be choſen, and ſome fine olive oil ſhould be heated to ebullition in another veſſel: A bit of phoſphorus, of the ſize of a pea, ſhould be thrown into the phial, and the boiling oil ſhould then be carefully poured over it, till the phial is one-third filled: The phial ſhould then be carefully corked; and, when it is to be uſed, it ſhould be unſtopped, to admit the external air, and cloſed again: The empty ſpace of the phial will then appear luminous, and give as much light as a dull ordinary lamp. Each time that the light diſappears, on removing the ſtopper it will inſtantly re-appear. It is proper to obſerve, that in cold weather it will be neceſſary to warm the bottle for a little while in the hands before the ſtopper is removed, without which precaution it would not yield any light.

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* Sonini's Journal.

A phial thus prepared may be uſed every night for ſix months; there is no danger of fire from it, and its coſt is very ſmall.

XVII.

Analyſis of the Magneſian Earth of Baudiſſero in Canavais (in the Department of the Loire,) known by the Name of Porcelain Earth, and hitherto conſidered as a Clay. *By* M. GIOBERT.*

Porcelain earth conſidered as native alumine.

THE earth of Baudiſſero, known by the name of porcelain earth, has been hitherto conſidered as one of the pureſt argillaceous earths known in the hiſtory of foſſils, and is arranged in our cabinets of minerals as native alumine.

Uſed as ſuch in the ſtone-ware pottery at Vineuf.

Various chemiſts adopted the ſame concluſion.

In a manufacture of ſtone-ware pottery, which has been eſtabliſhed at Vineuf, this earth has been uſed for a long time, as a clay of extraordinary purity. The celebrated Macquer, and with him Baume, to whom ſpecimens of this earth were ſent from the above manufacture, pronounces poſitively that it was a clay of ſuperior quality to that which they uſed in the manufactory of porcelain at Sevres.

Doctor Gioanetti continued to uſe it in the manufacture of his fine porcelain at the ſame Vineuf; and he engaged in, if not an analyſis, at leaſt ſome experiments on this earth, to aſcertain more preciſely the proportions in it between ſilex and earth, which he believed to be pure alumen. Theſe experiments convinced Doctor Gioanetti, that, with the exception of a little carbonic acid which he found in it, the earth of Baudiſſero was an alumen almoſt perfectly pure, or at leaſt the pureſt that he had ever met with.

The alumine was eſtimated at 80 per cent.

This chemiſt, when I made enquiries of him relative to this earth, aſſured me frequently, that picked pieces yielded him ſometimes ninety *per cent.* of alumen, including a little carbonic acid, and that in the groſs it yielded conſtantly at leaſt 80.

Other authority.

On the peruſal of the mineralogical deſcription of the mountains of Canavais by the Chevalier Napion, it will be found that this eſtimable mineralogiſt has not heſitated to declare the earth of Baudiſſero to be the moſt pure alumen ever found in Piedmont; and again in his elements of mineralogy, he mentions the earth of Baudiſſero as native alumen.

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* Journal de Phyſique, LX.

Contrary to theſe aſſertions this native earth contains no alumine at all.

Facts ſo poſitively aſſerted by ſcientific men ſo eſtimable as Maquer, Baume, and our colleagues Gioanetti and Napion, admitted no doubt of the nature of this earth; to which authorities might be added the ſucceſs with which Gioanetti conſtantly uſed it in his porcelain manufacture.

Among a number of reſearches which I made relative to the artificial fabrication of ſulphate of alumen, I employed myſelf on this earth, and to my great ſurprize found that the earth of Baudiſſero not only was not pure alumen, but did not even contain an atom of it.

Immenſe quantities of ſulphuret of iron at Baudiſſero.

The town of Baudiſſero is ſituated at leaſt three leagues from Ivree and from Brozo, this laſt village, as celebrated for its iron mines as for the manner in which they are wrought, contains in a mountain, among other minerals, an inexhauſtible maſs of ſulphuret of iron of a remarkable purity, where there is eſtabliſhed a manufacture of ſulphate of iron by the combuſtion of the ſulphur.

Effloreſcence of the neighbouring blocks of ſtone by ſulphureous vapours.

On inſpecting this manufacture laſt year, I was ſtruck with the ſingularly powerful action, which the ſulphureous acid, formed by the combuſtion of the ſulphur, (and of which a part expanded itſelf to neighbouring places,) exerciſed on the great blocks of ſtone.

Theſe ſtones were a ſort of granite ſchiſtus; and the ſulphureous acid attacked them ſo forcibly that it made them exfoliate, and at laſt reduced them to a ſpecies of effloreſcence, or white powder evidently ſaline, in which its aſtringent taſte announced ſulphate of alumen.

Probability that ſulphate of alumine or alum might be advantageouſly made from the porcelain earth.

This circumſtance made me think that if a good argil was expoſed to the action of the acid it would be alumenated; and the earth of Baudiſſero, which I believed to be almoſt pure alumen, being at ſuch an inconſiderable diſtance, made me conceive the hope of being able to eſtabliſh with economy at Piedmont, a manufacture of artificial ſulphate of alumen.

Very promiſing local advantages.

The idea of this eſtabliſhment appeared to me to be ſo much the more fortunate as there was at the foot of the ſame mountain which contained the pyrites, a great turbary, which extended almoſt as far as Chinſella, that is to ſay, almoſt to Baudiſſero, and which might furniſh fuel at a very ſmall expenſe;

penſe; and it ſeemed to me that nature, in placing at one ſide an inexhauſtible mine of ſulphur, and at the other inexhauſtible maſſes of the proper earth of an extremely rare purity, and between them an abundant ſupply of fuel, of the ſort moſt proper for this kind of work, had done its utmoſt in favour of the eſtabliſhment I intended.

Experiments preparatory.

There only remained to make ſome experiments with a view to aſcertain the moſt profitable way of proceeding; and to examine principally if the iron which is united to the ſulphur in the pyrites, would not be injurious to the ſulphate of alumen obtained.

With this deſign I began by examining the action of the earth of Baudiſſero on the ſulphate of iron, and the quantity of the earth neceſſary for the decompoſition of a given weight of ſulphur.

The earth of Baudiſſero decompoſed ſulphate of iron in the humid way. A little potaſh was added and the liquor ſet to cryſtallize.

In the different experiments the ſulphate of iron diſſolved in water, and boiled with this earth in different proportions became evidently decompoſed after boiling for leſs than a quarter of an hour; the iron was precipitated of a blackiſh grey, while the ſolution was colourleſs, and ammonia dropped into it formed only a very white precipitate, which did not announce much iron: I filtered the liquor, of which one part was mixed with a little potaſh, and placed ſo as to cryſtalize; and to aſcertain whether there was any potaſh in the earth of Baudiſſero, I ſet another part to cryſtalize without any alkali.

It gave ſulphate of magneſia and not allum.

I obſerved that the liquors cryſtallized immediately after becoming cold; but in the place of octahedrons, I found the moſt perfect, the moſt beautiful, and pureſt cryſtals of ſulphate of magneſia.

All the cryſtals were of this kind, and hence magneſia is an excellent porcelain earth.

The liquor which remained produced, on a new evaporation, the ſame pure cryſtals of ſulphate of magneſia; and did the ſame on ſucceſſive evaporations and cryſtalizations to the laſt drop of the liquor. In this manner was a natural alumen transformed entirely into magneſia, and at the ſame inſtant magneſia became at once an excellent porcelain earth. If examples of this kind ſhould multiply, the neceſſity of chemical analyſis for the knowledge of foſſils will become more and more manifeſt, and leſs reliance will be learned to be placed on their external and phyſical characters, which at preſent ſeems to me to be too much abuſed.

The

More careful examination of the native earth.

The above unexpected reſults engaged me to make a more careful examination of the earth of Baudiſſero, and which is the object of this memoir.

At the time when I found that the ſuppoſed alumen of Baudiſſero in Canavais, was really a magneſian earth, I knew of no other example of an earth truly magneſian, but that of the earth of Salinelle, or of Sommieres, which Berard had made known (*Annales de Chimie, Tome* XXXIX. p. 65.)

Other ſpecimens of native magneſia.

In this magneſian earth there is no mixture of any other earth except ſilex, and that in a very ſmall proportion, of which fact there are but few examples. But on receiving the twelfth volume of Brochant's Mineralogy, I found that the diſcovery of a magneſian earth was announced in it, which is the native carbonate of magneſia found by Doctor Mitchel at Roubſchitz in Moravia. From the analyſis which he made of it, and which is mentioned by Brochant, we are aſſured that the native carbonate of magneſia of Moravia is compoſed only of magneſia and carbonic acid in almoſt equal parts; but the yellowiſh grey colour ſpotted with black, which Doctor Mitchel gives to this earth, ſeems ſufficiently to indicate the exiſtence of ſome other conſtituent parts. On comparing the characters and nature of the magneſian earth of Baudiſſero, it will be eaſy to perceive the differences which diſtinguiſh it from the other preceding magneſian earths.

Local ſituation of the earth of Baudiſſero.

The magneſian earth of Baudiſſero is found diſpoſed in a vein in a ſteatite rock, of which the mountain is compoſed that encloſes it. It is accompanied by an horn-ſtone, ſometimes of a tranſparent pale colour, ſometimes, when its decompoſition commences, of a white almoſt opaque. In this ſtate the horn-ſtone does not appear to be that of which Doctor Bonvoiſin has given the deſcription and analyſis, under the name of the Hydrophane of Piedmont.

It is found in maſſes, lumps, or fragments.

Our magneſian earth appears in maſſes, ſometimes in roundiſh lumps (mamelonnés) and ſometimes in fragments more or leſs large; the lumps and fragments are ſometimes, but rarely, tuberculoſe.

Beautifully white.

This earth is of the moſt beautiful white, in which it differs from that of Moravia, of which the colour is a yellowiſh grey ſpotted with black, and from that of Salinelle, or of Sommieres, which is of a chocolate colour.

Soft or hard ſo much as to ſcratch ſteel.

The hardneſs of this earth is variable, ſometimes it is ſoft, in which ſtate I ſhall call it the *earthy* ſort, and ſome pieces of it have a conſiderable hardneſs; as in all my experiments I tried them comparatively, I ſhall name this laſt variety the *ſtony* kind, to diſtinguiſh it from the preceding.

Pulverable, tho' not finely, permanent in the air.

The ſtony variety is ſcratched by ſteel, ſometimes, on the contrary, it is hard enough to ſcratch ſteel. It can be eaſily reduced to powder; but with much difficulty to very fine powder, and this only takes place after long trituration in a mortar of porphyry. Its hardneſs neither increaſes nor diminiſhes by the action of the air; in this reſpect it differs from the magneſia of Moravia, which is very friable, and from that of Salinelle, which is ſoft in its bed, and only grows hard on exſiccation in the air.

Fracture conchoidal.

The fracture of this variety is conchoidal and unequal.

Dull, opake, moderately heavy.

Its ſurface is dull; ſometimes, but very rarely, ſhining ſpots appear. It is conſtantly perfectly opaque, and moderately heavy; its ſpecific gravity is variable.

Slightly unctuous and adheſive.

It is a little unctuous to the touch in the friable and earthy ſort, and but very little ſo in the ſtony variety.

It ſenſibly adheres to the tongue, though not much; it acquires this property in a conſiderable degree when it is moderately warmed at the fire.

The ſoft ſpecimens abſorb water and mix like clay.

Plunged in water, the ſtony variety does not abſorb it at all; the friable ſort abſorbs it greedily, and with an hiſſing, but the mixture does not grow hot.

Are not fuſible by the blow-pipe.

The friable ſpecies mixes with water to a conſiderable degree, in the ſame manner as clay; the fine particles of this earth, like thoſe of clay, continue a long time ſuſpended in water, with this difference from thoſe of clay, that they do not unite together. Urged by the blow-pipe, on a cianite cryſtal, it is infuſible.

Treated in a maſs, on the fire in a crucible, eſpecially in a red hot crucible, it ſoon decrepitates, and divides into thick ſcaly pieces, which leap out of the crucible; this does not happen if it is heated by degrees and moderately.

But apparently ſo in a crucible.

If it is reduced to a fine powder, and then traited on the fire, as ſoon as the bottom of the crucible begins to grow red hot, this earth boils for a ſhort time, and ſeems to unite together, as if moderately moiſtened.

With loſs of one ſeventh.

An hundred parts of this earth treated in this manner, until the boiling ceaſes, after an hour of incondeſcence, became reduced to 85, and 0,40. The earth calcined in this manner throws out that blueiſh light which has been obſerved from common magneſia.

Giving out carbonic acid.

If the calcination is made in a retort of earthen ware, to which a ſyphon is adapted, and plunged into a bottle filled with lime water, there is formed in the bottle carbonate of lime; ſo that the diminution of weight is partly due to the diſengagement of carbonic acid.

It contains a minute quantity of ſulphate of lime.

A thouſand grains of this earth in fine powder were boiled in ſix pounds of diſtilled water. The liquor being filtered, and then eſſayed by various reagents, preſented the following reſults.

With the ſolutions of the acetate, nitrate, and muriate of barytes, the mixture became troubled almoſt inſtantly, and formed a ſediment of ſulphate of barytes, but in a very ſmall quantity.

The oxalate of ammonia formed oxalate of lime with it, but alſo in a very ſmall portion.

Theſe experiments repeated different times on the earth both of the ſtony and friable varieties, conſtantly gave the ſame reſults.

Lime and ſulphuric acid, or ſulphate of lime, is therefore, although in a ſmall degree, a conſtituent part of the earth of Baudiſſero both in the ſtony and ſoft ſtate.

A minute portion of muriatic acid ſeems to be preſent in the ſtony variety.

The nitrate of ſilver formed a precipitate equally with both ſorts; but remarkable differences were obſerved between its effects in the water from the ſtony ſpecies, and on that from the ſoft variety; with the latter it formed a precipitate, which collected in a powder at the bottom of the glaſs; but with the water from the ſtony kind, beſides the precipitate, filaments were produced conſtantly, which indicated the preſence of muriatic acid. Many times the "ſulphuric acid*" was firſt removed by the acetate of barytes, and after filtration, on being treated with the nitrate of ſilver, ſtill formed a precipitate of muriate of ſilver.

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* It is not clear what the removal of the *ſulphuric acid* mentioned here had to do with making the appearance of the *muriate of ſilver* ſeem extraordinary; perhaps it is an error in the original; the tranſlation is literal and correct. B.

The infuſion of the ſtony ſort afforded differences from the other, with ammonia alſo; this reagent did not ever trouble the infuſion of the friable ſpecies, but always troubled, though ſlightly, the infuſion of the ſtony variety.

It follows from theſe obſervations that, beſides the ſulphate of lime which both kinds of the earth of Baudiſſero contained, the ſtony variety held in union muriatic acid, perhaps in combination, partly with the lime, which there was not ſufficient ſulphuric acid to ſaturate, and partly united to another earth, which was not lime, ſince its ſolution permitted itſelf to be decompoſed by ammonia; and it will appear that this earth was magneſia.

The ancient mineral acids attack this native earth.

The ſulphuric, nitric, and muriatic acids attack this earth, when it is well divided into an extremely fine powder.

Their action however is but little apparent, but on the leaſt addition of heat it becomes ſtrongly marked. Very ſmall bubbles of gas, which riſe from the bottom of the liquor, a ſlight white ſcum which forms itſelf at the ſurface, and a ſmall hiſſing, ſhews plainly that there is a diſengagement of an aeriform fluid or effervescence.

With great force if previouſly calcined.

When the earth has been previouſly calcined in the fire, their action is very different. There is not, as may be foreſeen, any effervescence; but the mixture grows hot, to that degree that a true ebullition enſues; in ſome minutes the mixture aſſumes a ſolid form, cauſed by a kind of jelly produced by it.

The muriatic acid acts more ſtrongly.

The acid which has the greateſt action on it is the muriatic acid, and after this the nitric, and the ſulphuric acid after both. This laſt however does not diſſolve without much difficulty the whole of the ſoluble part, and that after a long continued ebullition.

The ſolution made in the cloſed veſſels diſpoſed ſo as that the gas may be received, forms with lime-water carbonate of lime, which confirms the diſengagement of a little carbonic acid before indicated by the calcination of this earth in the fire.

The ſolutions of this earth in the acids are perfectly colourleſs.

The pruſſiate of lime or the oxalate of ammonia do not at all trouble them.

Ammonia precipitates the ſolution as does likewiſe the carbonate of potaſh.

Ammonia forms with them an abundant precipitate.

The common unſaturated carbonate of potaſh forms alſo a precipitate with them.

When this carbonate ceaſes to trouble the liquor, and that, after being left to ſettle and being filtered, the clear liquor is ſubmitted to ebullition, it becomes troubled again and throws down a ſecond time an earthy precipitate.

But not if ſaturated with c. acid.

Finally, if inſtead of the unſaturated carbonate, the carbonate of potaſh, well ſaturated with acid is uſed, there is not the leaſt precipitate formed.

The earth is pure magneſia.

Experiments, which I will relate, ſhew not only that the earth diſſolved by the acids is magneſia, but that there is not mixed with it the leaſt particle of lime, which can be diſcovered by the oxalate of ammonia, that there is no trace of alumen in it, which the ſaturated carbonate of potaſh precipitates, and does not again diſſolve; that it does not contain the leaſt oxide of iron, that can be indicated by the pruſſiate of lime; and finally, that it is magneſia perfectly pure.

As is ſhewn by the ſulphate.

This reſult is farther confirmed by the ſulphate of magneſia, which the cryſtallization of the ſolution of this earth in ſulphuric acid yields excluſively.

Inſoluble reſidue or ſilex about one 6th.

The acids in diſſolving this earth leaves a reſidue, the quantity of which ſeems to be variable; that which the ſulphuric acid leaves is conſtantly more than what is left by the muriatic or nitric acids. An hundred and twenty grains of this earth, after being well lixiviated in pure water, left a reſidue of which the weight, in the different experiments which I made, did not ever exceed 17 grains, and never was leſs than 14. The ſtony variety was that which in general gave the moſt of this inſoluble reſidue. Many experiments, which I have made, and which it would be uſeleſs to repeat here, have convinced me that this reſidue is perfectly pure ſilex.

Component parts recapitulated.

The earth of Baudiſſero, from the preceding experiments, conſiſts entirely of magneſia with a little carbonic acid, a ſmall quantity of ſilex, and a very minute portion of ſulphate of lime, with, in the ſtony variety, ſome traces of muriate of magneſia.

(To be concluded in the Supplement.)

XVIII.

Experiments for aſcertaining how far Teleſcopes will enable us to determine very ſmall Angles, and to diſtinguiſh the real from the ſpurious Diameters of celeſtial and terreſtrial Objects: with an Application of the Reſult of theſe Experiments to a Series of Obſervations on the Nature and Magnitude of Mr. HARDING'S *lately diſcovered Star. By* WILLIAM HERSCHEL, *L. L. D. F. R. S. Abridged from the Phil. Tranſ.* 1805.

Enquiry as to the ſmalleſt angle under which the eye by a teleſcope can determine the figure of an object.

THE diſcovery of Mr. Harding having added a moving celeſtial body to the liſt of thoſe that were known before, Dr. Herſchel was deſirous of aſcertaining its magnitude: and as in the obſervations which it was neceſſary to make he intended chiefly to uſe a ten-feet reflector, it appeared to be a deſideratum highly worthy of inveſtigation to determine how ſmall a diameter of an object might be ſeen by this inſtrument. It is known that a very thin line may be perceived, and that objects may be ſeen when they ſubtend a very ſmall angle; but the caſe he wanted to determine related to a viſible diſk; a round, well defined appearance, which might without heſitation be affirmed to be circular, if not ſpherical.

The author's unaſſiſted eye cannot diſtinguiſh a ſmaller circle than of 2′ 17″ from an equal ſquare.

In April of the year 1774, the Doctor determined a ſimilar queſtion relating to the natural eye: and found that a ſquare area could not be diſtinguiſhed from an equal circular one till the diameter of the latter came to ſubtend an angle of 2′ 17″.

He did not think it right to apply the ſame concluſions to a teleſcopic view of an object, and therefore had recourſe to the a ſeries of experiments.

Experiments with the teleſcope: the objects were pin's heads.

The firſt courſe of experiment, was made with the heads of pins deprived of their poliſh by tarniſhing them in the flame of a candle. The diameters of the heads were meaſured by a microſcopic projection, with a magnifying power of 80. Theſe meaſures were ſo exact, than when repeated they ſeldom differed more than a few ten thouſandths part of an inch from each other. The focal length of the mirror on Arcturus is 119,64 inches, but on theſe objects 125,9, and the diſtance was meaſured with deal rods.

And the reſult of this experiment was that an object having a diameter ,0425 could be eaſily ſeen in the author's ten-foot teleſcope to be a round body, when the magnified angle under which it appeared was 2′ 18″,9, and that with a high power (522) a part of it, ſubtending an angle of 0″,364 may be conveniently perceived.

When the purpoſe of this experiment, was conſidered, the reſult was not found ſufficient to anſwer the intention; for as the ſize of the object required the uſe of a low power, a doubt aroſe whether the inſtrument would be equally diſtinct when a higher ſhould be required. To reſolve this queſtion, it was neceſſary either to remove my objects to a greater diſtance, or to make them ſmaller.

With globules of ſealing-wax.

2. Small globules of ſealing-wax were therefore made by dipping the point of a fine needle, a little heated, into it, which took up a ſmall globule. To prevent ſeeing them at a diſtance in a different aſpect from that in which they were meaſured under the microſcope, the needles were fixed with ſealing-wax on ſmall ſlips of cards before the meaſures were taken.

By this experiment it was found, that with a globule ſo ſmall as ,00763 of a ſubſtance not reflecting much light, the magnified angle muſt be between 4 and 5 minutes before we can ſee it round. But it alſo ſhewed that a teleſcope with a ſufficient power (522) will ſhow the diſk of a faint object when the angle it ſubtends as the neked eye is no more than 0″,653.

Globules of ſilver.

The third experiment was made with globules of ſilver. As the objects made of ſealing-wax, on account of their colour, did not appear to be fairly ſelected for theſe inveſtigations, a ſet of ſilver ones were made. They were formed by running end of ſilver wires, the 305th and 340th part of an inch diameter, into the flame of a candle.

By this experiment it was found that the teleſcope acted very well with a high power (522), and will ſhow an object ſubtending only 0″,484 ſo large that it may be divided into quarters of its diameter.

Experiments with other globules, and with ſilver at greater diſtances, &c.

The fourth experiment was made with globules of pitch, bee's-wax, and brimſtone, and did not prove ſo generally advantageous as thoſe with ſilver which reflected more light.

And a fifth and ſixth experiment was made with the ſilver globules at greater diſtances; and by illuminations at night by the flame of a lamp of which for brevity the particulars are here omitted.

The author then proceeded to make direct obſervations on the ſpurious diameters of celeſtial bodies, from which he deduces the following reſults:*

(1.) As the diameters of fixed ſtars are undoubtedly ſpurious, it follows that, with the ſtars, the ſpurious diameters are larger than the real ones, which are too ſmall to be ſeen.

Spurious diameters of ſtars greater than real.

(2.) From many eſtimations of the ſpurious diameters of the ſtars † it follows, not only that they are of different ſizes, but alſo that under the ſame circumſtances, their dimenſions are of a permanent nature.

Sizes are the ſame in like circumſtances;

(3.) By this and many other obſervations it appears, that the ſpurious diameters of the ſtars are differently coloured, and that theſe colours are permanent when circumſtances are the ſame.

and colours.

(4.) By many obſervations, a number of inſtances of which may be ſeen in Dr. Herſchel's catalogues of double ſtars, their ſpurious diameters are leſſened by increaſing the magnifying power, and increaſe when the power is lowered.

They are leſs with high powers;

(5.) It is alſo proved by the ſame obſervations, that the increaſe and decreaſe of the ſpurious diameters, is not inverſely as the increaſe and decreaſe of the magnifying power, but in a much leſs ratio.

but not proportionally.

(6.) The magnifying power acts unequally on ſpurious diameters of different magnitudes; leſs on the large diameters, and more on the ſmall ones.

Small ſtars are moſt enlarged.

(7.) When the aperture of the teleſcope is leſſened, it will occaſion an increaſe of the ſpurious diameters, and when increaſed will reduce them.

(8.) It alſo ſhows that the increaſe and decreaſe of the unequal ſpurious diameters, by an alteration of the aperture of the teleſcope, is not proportional to the diameters of the ſtars:

Leſs aperture cauſes greater ſp. diameter; and ſmall ſtars are moſt affected by this change.

(9.) But that this alteration acts more upon ſmall ſpurious diameters, and leſs upon large ones.

(10.) From this we find that ſtars, when they are extremely ſmall, loſe their ſpurious diameters, and become nebulous.

Very ſmall ſtars appear nebulous,

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* On this ſubject ſee our Journal, Vol. VI. p. 15, and Fig. 1, Plate IV.

† See Catalogue of double Stars. Phil. Tranſ. for 1782, p. 115; and for 1785, p. 40.

(11.) Many

Other cauſes affect the ſpurious diameters.

(11.) Many other cauſes will have an influence on the apparent diameter of the ſpurious diſks of the ſtars, ſuch as the goodneſs of the ſpecula and lenſes; but they are ſo far within the reach of our knowledge, that with a proper regard to them, the concluſion he has drawn in Rem. (2.) "that under the ſame circumſtances their dimenſions are permanent," will ſtill remain good.

Similar experiments were made on the ſpurious diameters of terreſtrial objects, namely ſilver globules, which afforded the following reſults:

Spurious diſks of globules are ſmaller than the real diſks.

(1.) The terreſtrial ſpurious diſks of globules are leſs than the real diſks; whereas we have ſeen, in Remark (1.) of the celeſtial ſpurious diſks, that theſe are larger than the real ones.*

Larger magnitudes give larger ſp. diſks;

(2.) The leſs globule has the largeſt ſpurious diſk. This agrees with the ſpurious diſks of celeſtial objects: the ſtars of the firſt, ſecond, and third magnitude, having a larger ſpurious diſk than thoſe that are of inferior magnitudes.

coloured like the celeſtial;

(3.) With reſpect to colours, the terreſtrial alſo agree with the celeſtial ſpurious diſks.

Leſs with greater mag. power;

(4.) The ſpurious diameters of the globules, like the ſpurious diſks of the ſtars, are proportionally leſſened by increaſing the magnifying power, and increaſed when the power is lowered.

But not proportionally.

(5.) When the eſtimations are compared with the powers, it will alſo be ſeen that the increaſe and decreaſe of the ſpurious diſks of the globules is not inverſely as the powers, but in a much leſs ratio.

Power acts more on ſmall than large ſp. diſks,

(6.) The effect of magnifying power is unequally exerted on ſpurious diameters; and that, as with celeſtial objects, ſo with terreſtrial, this power acts more on the ſmall ſpurious diſks than on the large ones.

and diminution of aperture;

(7.) The ſpurious terreſtrial diſks alſo reſemble thoſe of the ſtars, by increaſing when the aperture is leſſened, and decreaſing when it is enlarged.

greater on ſmall diſks.

(8.) By theſe experiments it is proved, that the increaſe and decreaſe of the diameters occaſioned by different apertures is not proportional to the diameters of the ſpurious diſks.

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* It appears from the context, that this ariſes from the terreſtrial ſpurious diſks being formed by the ſmall ſpot of reflected light from the metallic globule, and not from its whole diameter.

(9.) But that the change of the apertures acts more on the ſmall, and leſs on the large ones.

Theſe diſks are not changed by diminution of light.

(10.) The ſpurious diſks of globules are loſt for want of proper illumination, but do not change their magnitude on that account. The brightneſs of the atmoſphere in a fine day is ſufficient to produce them; though the illumination of the ſun is generally the principal cauſe of them.

(11.) The diameters of ſpurious diſks are liable to change from various cauſes; an alteration in the direction of the illumination will make the reflection come from a different part of the globule, which can hardly be expected to be equally poliſhed in its ſurface, or of equal convexity every where, being very ſeldom perfectly ſpherical; but as upon the whole the figure of them is pretty regular, the apparent diameter of the ſpurious diſks will generally return to its former ſize.

Mercurial globules.

Globules of mercury were uſed inſtead of thoſe of ſilver, and with the ſame reſults.

Meaſurement of ſpurious diſks. They may be diſtinguiſhed from real diſks by uſing firſt a central, and then an annular aperture. The firſt enlarges and the ſecond diminiſhes them.

The ſpurious terreſtrial diſks were then meaſured by comparing them with circles on a tablet: and it was found that a variation in their illumination did not affect their magnitude. It was alſo found that the rays from the central part of the mirror gave a larger image than thoſe from its circumference. So that when a central aperture of three inches gave an image correſponding with a circle of 0.465 inch, an annular opening from 6.5 to 8.8 inches gave only 0.18 inch for the image: and the experiments were ſufficiently varied as to the magnitudes and ſituations of the apertures to ſhew that this difference did not ariſe from more or leſs light.

Trials.

This property of the mirror ſerves admirably to diſtinguiſh a ſpurious diſk from a real one; and proved to be ſo on trial with terreſtrial and celeſtial objects.

Obſervations on the Nature and Magnitude of Mr. Harding's *lately diſcovered Star.*

Obſervations on the planet Juno.

On the day Dr. Herſchel received an account of Mr. Harding's new ſtar, which was the 24th of September, he directed his teleſcope to the calculated place of the new object, and noted all the ſmall ſtars within a limited compaſs about it. They were then examined with a diſtinct high magnifying power; and ſince no difference in their appearance

Obſervations on the planet Juno.

was perceivable, it became neceſſary to attend to the changes that might happen in the ſituation of any one of them. They were delineated as in Fig. 1, (Plate XIV.) which is a mere eye-draught, to ſerve as an elucidation to a deſcription given with it in the journal; and the ſtar marked *k*, was the new object.

Sept. 29. Being the firſt clear night, he began a regular ſeries of obſervations: and as the power of determining ſmall angles, and diſtinctneſs in ſhowing minute diſks, whether ſpurious or real, of the inſtrument he uſed on this occaſion, had been ſufficiently inveſtigated by the foregoing experiments, there could be no difficulty in the obſervation, with reſources that were then ſo well underſtood, and have now been ſo fully aſcertained.

"Mr. Harding's new celeſtial body precedes the very ſmall ſtar in Fig. 3, between 29 and 33 Piſcium, and is a little larger than that ſtar; it is marked A. *f g h* are taken from Fig. 1. I ſuppoſe *g* to be of about the 9th magnitude, ſo that the new ſtar may be called a ſmall one of the 8th."

With his ten-feet reflector, power 496.3, he viewed it attentively, and comparing it with *g* and *h*, Fig. 3, could find no difference in the appearance but what might be owing to its being a larger ſtar.

By way of putting this to a trial, he changed the power to 879,4, but could not find that it magnified the new one more than it did the ſtars *g* and *h*.

"I cannot perceive any diſk; its apparent magnitude with this power is greater than that of the ſtar *g*, and alſo a very little greater than that of *h*; but in the finder, and the night-glaſs *g* is conſiderably ſmaller than the new ſtar, and *h* is alſo a very little ſmaller."

He compared it now with a ſtar which in the finder appeared to be a very little larger; and in the teleſcope with 879,4 the apparent magnitude of this ſtar was alſo larger than that of the new one.

"As far as I can judge without ſeeing the aſteroids of Mr. Piazzi and Dr. Olbers at the ſame time with Mr. Harding's, the laſt muſt be at leaſt as ſmall as the ſmalleſt of the former, which is that of Dr. Olbers."

"The ſtar *k*, Fig. 1, obſerved Sept. 24, is wanting, and was therefore the object I was in ſearch of, which by computation muſt have been that day in the place where I ſaw it."

Obſervations on the planet Juno.

"The new ſtar being now in the meridian with all thoſe to which I am comparing it, and the air at this altitude being very clear, I ſtill find appearances as before deſcribed: the new object cannot be diſtinguiſhed from the ſtars by magnifying power, ſo that this celeſtial body is a true aſteroid."

Mr. Bode's ſtars 19, 25 and 27 Ceti are marked 7m, and by comparing the aſteroid, called Juno, with theſe ſtars, it has the appearance of a ſmall one of the 8th magnitude.

With regard to the diameter of Juno, the author remarks that had it been half a ſecond, he muſt have inſtantly perceived a viſible diſk. Such a diameter, when he ſaw it magnified 879,4 times, would have appeared under an angle of 7′ 19″,7, one half of which, it will be allowed, from the experiments that have been detailed, could not have eſcaped his notice.

Oct. 1. Between flying clouds, the aſteroid was ſeen, which in its true ſtarry form has left the place where it was ſeen Sept. 29. It has taken the path in which by calculation it was expected to move. This aſcertains that no miſtake in the ſtar was made when laſt obſerved.

Oct 2, 7^h. Mr. Harding's aſteroid is again removed, but is too low for high powers.

8^h 30′. Viewed it now with 220,3 288,4 410,5 496,3 and 879,4. No other diſk was viſible than that ſpurious one which ſuch ſmall ſtars have, and which is not proportionally magnified by power.

With 288,4, the aſteroid had a larger ſpurious diſk than a ſtar which was a little leſs bright, and a ſmaller ſpurious diſk than another ſtar that was a little more bright.

Oct 5, with 410,5. The ſituation of the aſteroid is now as in Fig. 4. Its diſk, which is probably the ſpurious appearance of ſtars of that magnitude, was compared with a larger, an equal, and a ſmaller ſtar. It was leſs than the ſpurious diſk of the larger, equal to that of the equal, and larger than that of the ſmaller ſtar. The gradual difference between the three ſtars is exceedingly ſmall.

"With 496,3, and the air uncommonly pure and calm, I ſee ſo well that I am certain the diſk, if it be not a ſpurious one, is leſs than one of the ſmalleſt globules I ſaw this morning in the tree."

Obſervations on the planet Juno.

The diameter of this globule was ,02. It ſubtended an angle of 0″,429, and was of ſealing-wax; had it been a ſilver one, it would have been ſtill more viſible.

With 879,4. All comparative magnitudes of the aſteroid and ſtars, remain as with 496,3.

The minute double ſtar *q* Ophiuchi * was ſeen in high perfection, which proves that the air is clear, and the teleſcope in good order.

The aſteroid being now in the meridian, and the air very pure, the comparative diameter ſeems a little larger than that of an equal ſtar, and its light alſo differs from ſtar-light. Its apparent magnitude, however, can hardly be equal to that of the ſmalleſt globule obſerved this morning. This globule meaſured ,01358, and at the diſtance of 9620,4 inches ſubtended an angle of 0″,214.

When the aſteroid was viewed with 879,4, more hazineſs was found than an equal ſtar would have given: but this the Doctor aſcribes to want of light. What he calls an equal ſtar, is one that in an achromatic finder appears of equal light.

Oct. 7. Mr. Harding's aſteroid has continued its retrograde motion. The weather is not clear enough to allow the uſe of high powers.

Oct. 8. If the appearance reſembling the ſpurious diſks of ſmall ſtars, which I ſee with 410,5 in Mr. Harding's aſteroid, ſhould be a real diameter, its quantity then by eſtimation may amount to about 0″,3. This judgment is founded on the facility with which I can ſee two globules often viewed for this purpoſe.

The angle of the firſt is 0″,429, and of the other 0″,214; and the aſteroid might be larger than the latter, but certainly was not equal to the former.

With 496,3, there is an ill-defined hazy appearance, but nothing that may be called a diſk viſible. When there is a glimpſe of more condenſed light to be ſeen in the centre, it is ſo ſmall that it muſt be leſs than two-tenths of a ſecond.

To decide whether this apparent condenſed light was a real or ſpurious diſk, he applied different limitations to the aperture of the teleſcope, but found that the light of the new ſtar was too feeble to permit the uſe of them. From this he concluded

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* See Cat. of double Stars, I. 87.

Obſervations o the planet Jun

that an increaſe of light might now be of great uſe, and viewed the aſteroid with a fine 10-feet mirror of 24 inches diameter, but found that nothing was gained by the change. The temperature indeed of theſe large mirrors is very ſeldom the ſame as that of the air in which they are to act, and till a perfect uniformity takes place no high powers can be uſed.

The aſteroid in the meridian, and the night beautiful. After many repeated compariſons of equal ſtars with the aſteroid, I think it ſhows more of a diſk than they do, but it is ſo ſmall that it cannot amount to ſo much as 3-tenths of a ſecond, or at leaſt to no more.

It is accompanied with rather more nebuloſity than ſtars of the ſame ſize.

The night is ſo clear, that I cannot ſuppoſe viſion at this altitude to be leſs perfect on the ſtars, than it is on day objects at the diſtance of 800 feet in a direction almoſt horizontal.

Oct. 11. By comparing the aſteroid alternately and often with equal ſtars, its diſk, if it be a real one, cannot exceed 2, or at moſt 3-tenths of a ſecond. This eſtimation is founded on the comparative readineſs with which every fine day I have ſeen globules ſubtending ſuch angles in the ſame teleſcope, and with the ſame magnifying power.

"The aſteroid is in the meridian, and in high perfection. I perceive a well defined diſk that may amount to 2 or 3-tenths of a ſecond; but an equal ſtar ſhows exactly the ſame appearance, and has a diſk as well defined and as large as that of the aſteroid."

Reſult and Application of the Experiments and Obſervations.

We may now proceed to draw a few very uſeful concluſions from the experiments that have been given, and apply them to the obſervations of the ſtar diſcovered by Mr. Harding; and alſo to the ſimilar ſtars of Mr. Piazzi and Dr. Olbers. Theſe kind of corollaries may be expreſſed as follows.

(1.) A 10-feet reflector will ſhew the ſpurious or real diſks of celeſtial and terreſtrial objects, when their diameter is ¼ of a ſecond of a degree; and when every circumſtance is favourable, ſuch a diameter may be perceived ſo diſtinctly, that it can be divided by eſtimation into two or three parts.

bſervations on the planet Juno.

(2.) A diſk of ¼ of a ſecond in diameter, whether ſpurious or real, in order to be ſeen as a round, well defined body, requires a diſtinct magnifying power of 5 or 6 hundred, and muſt be ſufficiently bright to bear that power.

(3.) A real diſk of half a ſecond in diameter will become ſo much larger by the application of a magnifying power of 5 or 6 hundred, that it will be eaſily diſtinguiſhed from an equal ſpurious one, the latter not being affected by power in the ſame proportion as the former.

(4.) The different effects of the inſide and outſide rays of a mirror, with regard to the appearance of a diſk, are a criterion that will ſhow whether it is real or ſpurious, provided its diameter is more than ¼ of a ſecond.

(5.) When diſks, either ſpurious or real, are leſs than ¼ of a ſecond in diameter, they cannot be diſtinguiſhed from each other; becauſe the magnifying power will not be ſufficient to make them appear round and well defined.

(6.) The ſame kinds of experiments are applicable to teleſcopes of different ſorts and ſizes, but will give a different reſult for the quantity which has been ſtated at ¼ of a ſecond of a degree. This will be more when the inſtrument is leſs perfect, and leſs when it is more ſo. It will alſo differ even with the ſame inſtrument, according to the clearneſs of the air, the condition, and adjuſtment of the mirrors, and the practical habits of the obſerver.

XIX.

Account of ſome new Improvements on Steam-Engines. By Mr. ARTHUR WOOLF.

Mr. Woolf's improvements in ſteam-engines.

IN our eighth volume, p. 262, we gave a ſhort account of a former improvement made by Mr. Woolf on the ſteam-engine, founded on a diſcovery that ſteam, of any higher temperature than that of boiling water, if allowed to paſs into another veſſel kept at the ſame temperature as the ſteam itſelf, will expand to as many times its volume, and ſtill be equal to the preſſure of the common atmoſphere, as the number of pounds which ſuch ſteam, before being allowed to expand, could maintain on each ſquare inch of a ſafety-valve expoſed

Mr. Woolf's improvements in ſteam-engines.

expoſed to the atmoſphere: for example, that maſſes or quantities of ſteam of the expanſive force of 20, 30, or 50 pounds the ſquare inch of a common ſafety-valve, will expand to 20, 30, or 50 times its volume, and ſtill be reſpectively equal to the atmoſphere, or capable of producing a ſufficient action againſt the piſton of a ſteam-engine to cauſe the ſame to riſe in the old engine (with a counterpoiſe) of Newcomen, or to be carried into the vacuous part of the cylinder in the improved engines firſt brought into effect by Meſſrs. Boulton and Watt.

In conſequence of this diſcovery Mr. Woolf was enabled to uſe his ſteam twice (if he choſe), and with complete effect; nothing more being neceſſary than to admit high ſteam, ſuppoſe of 40 pounds the ſquare inch, into one cylinder, to work there by its expanſive force, and then to allow the ſame ſteam to paſs into, and expand itſelf in, another cylinder of forty times the ſize of the firſt, there to work by condenſation in the common way. Or with only one cylinder, by admitting a proportionally ſmall quantity of high ſteam into it from the boiler, Mr. Woolf, found that he could effect a conſiderable ſaving in fuel.

In this firſt improvement of Mr. Woolf, though the ſaving might be carried a conſiderable length, it was ſtill neceſſarily limited by the ſtrength of materials; for in the employment of high ſteam there muſt always be ſome danger of an exploſion. Mr. Woolf, however, by a happy thought, has completely obviated every danger of this kind, and can now take the full advantage of the expanſive principle without the leaſt danger whatever. This he effects by throwing into common ſteam the additional temperature neceſſary for its high expanſion, *after the ſteam is admitted into the working cylinder*, which is heated by means adequate to the end intended to be gained; and the advantage which he thus gains he effectually ſecures by a moſt ingenious improvement in the piſton. It may be eaſily conceived that ſteam of ſuch high rarity as Mr. Woolf employs, could not be made fully effective with the piſton in common uſe; for in proportion to its rarity ſo muſt be the facility with which a portion of it would eſcape, and paſs by the ſide of the piſton to the vacuous part of the cylinder: but Mr. Woolf's contrivance ſeems perfectly adapted to prevent the loſs of even the ſmalleſt portion of the ſteam.

Beſides theſe improvements on the common ſteam-engine, he has alſo found means to apply the ſame principles to the old engine, known by the name of Savary's, in ſuch a way as to render the ſame a powerful and œconomical engine for a great variety of purpoſes.

Such is the outline of Mr. Woolf's improvements on this uſeful engine: but, for the general information of practical engineers, we ſhall ſubjoin a more technical deſcription, in Mr. Woolf's own words, extracted from his ſpecification of his patent.

(To be continued.)

SCIENTIFIC NEWS.

Geometry.

Two theorems from the Horologium of Huygens.

HUYGENS has given the two following theorems in his *Horologium Oſcillatorium*, which are applicable to all ſolid bodies: " *The center of oſcillation, and that of ſuſpenſion are always reciprocal to one another. The ſame body is always iſochronal to itſelf, while it oſcillates round parallel axis's taken at equal diſtances from the center of gravity.* M. Biot has given a remarkable extenſion to theſe two theorems.

—extended farther by M. Biot.

All theſe parallel axis's form the ſurface of a right angled cylinder of which the axis paſſes through the center of gravity. But the analytical expreſſion under which M. Biot preſents the theorem of Huygens, made him inſtantly perceive, that an arbitrary inclination might be given to this axis, the radius of the cylinder being ſuitably changed at the ſame time; and that thus according to the different degrees of inclination of the axis, an infinity of cylinders might be obtained. The ſuperficies of which cylinders ſhould have the ſame property as that of the primitive cylinder. Beſides this, the axis without changing its inclination may deſcribe a conical ſurface about its primitive poſition, which will multiply the number of cylinders already found, as often times as right lines can be conceived to be drawn on the upper ſurface of the cone.

Aſtronomy.

M. Pictet on the occultation of the pleiades by the moon.

M. Pictet has made an obſervation of an occultation of the pleiades by the moon, on the 19th of *November*, 1804, from the Obſervatory of Geneva.

M. Mechain on the occultation of π ſcorpion,

An account of an occultation of π ſcorpion, obſerved on the 17th of *July*, 1803, from the ſummit of Caſuleta, a mountain in the kingdom of Spain, was found among the papers of the late M. Mechain, which will appear in the 6th Volume of the Memoirs of the French National Inſtitute: this is the laſt obſervation of this kind made by a man of ſcience, whoſe premature loſs the Inſtitute will long regret.

and of the comet in 1793.

A long ſucceſſion of obſervations was alſo found among his papers, relative to the comet which he had diſcovered from Barcelona in 1793, which will alſo appear in the ſame publication.

Geography.

The longitude of Mexico determined accurately by M. Humboldt.

M. Humboldt has read before the *Inſtitute Nationale*, *A Memoir on the Longitude of Mexico*, the capital of the kingdom ſo called.

Geographers diſagree with regard to the poſition of this important point. The conſiderable difference which M. Humboldt found between his firſt obſervation, and the laſt which had been formerly made by others before him, engaged him to repeat it as often as he could, and by different methods. The diſtances of the moon from the ſtars, and ſeveral eclipſes of Jupiter's moons, always gave the ſame reſult, which is doubtleſsly preferable to all thoſe which have appeared hitherto.

Electricity.

Conductor contrived to prevent accidents,

Since the diſcovery of electrical conductors by Dr. Franklin, many philoſophers have repeated experiments to eſtabliſh the identity of electrical fire and lightning, by experiments with ſuch inſolated conductors.

Theſe experiments ſucceeded to the wiſh of all who tried them; but it was ſoon perceived, that they were attended with much danger: and ſince the death of Profeſſor Richman, of Peterſburg, who was ſtruck by lightning from his conductor in 1753, few have ventured to repeat the experiment.

M. Beyer

can be inſolated or not, at pleaſure.

M. Beyer of Paris has formed in his garden an apparatus of this kind, which is very ſimple, and at the ſame time perfectly effectual without any danger: It is a conductor which can alternately at pleaſure be inſolated, or not inſolated, and made to act either with a ball, or with a point. The communications between it and the earth are well eſtabliſhed, and as the obſervations can be made at more than an hundred feet from the apparatus, there is not the leaſt danger of any accident.

Aeroſtation.

Balloon projected by Mr. Robertſon, 132 feet diameter, to carry up 50 men,

The celebrated Aeronaut Robertſon, who aſcended from Peterſburg laſt year, is endeavouring to obtain the neceſſary aſſiſtance at that place for the conſtruction of an air balloon on a very large ſcale; he propoſes that it ſhall be 732 feet in diameter, which he calculates will carry up 37 ton, and which he ſuppoſes, therefore, will eaſily ſupport 50 people, and all neceſſary accommodations for them.

and a veſſel with ſails, &c.

It is to have attached to it a veſſel furniſhed with maſts, ſails, and every other article requiſite for navigating the ſea in caſe of accidents, and provided with a cabin for the aeronauts, properly fitted up, galley for cooking, proper ſtores for ſtowing proviſions, and ſeveral other conveniencies. To render the aſcent more ſafe, it is to take up another ſmaller balloon within it, and a parachute, which will render the deſcent perfectly gentle, if the outer balloon burſts.

and an internal balloon and parachute.

From its conſtruction it will be calculated to remain in the air ſeveral weeks, in which time many experiments in natural philoſophy, and aſtronomical obſervations may be made: It is alſo ſuppoſed, that geography may be conſiderably improved by its means, as the aeronauts will be neither ſtopped in their courſe by mountains or foreſts; and ſome have even thought, that with the aſſiſtance of the trade winds, a voyage round the earth might be made in it between the tropics. Its coſt, it is calculated, will be nearly equal to that of a ſhip of the line.

Univerſity of Charkow.

The court of Peterſburg publiſhed the act of confirmation of the Univerſity of Charkow, on the 16th of May, of which the following are the chief particulars.

The Univerſity is under the care of the Miniſter of public inſtruction: It has, however, its own particular adminiſtration and juriſdiction; the ordinances, by which its members are governed, are regulated by itſelf: It has the right of cenſure both with regard to the books printed by itſelf, or thoſe brought from abroad. All articles which it may want are allowed to paſs the frontiers without examination or tax. Its correſpondence is poſt free, and its paper is not ſubject to duty. The houſes of the profeſſors are free from taxes and all charges. The profeſſors have the rank of the ſeventh claſs, and the ſtudents that of the twelfth, or that of ſubaltern officers, receive commiſſion as ſuch, and wear ſwords. The profeſſors after twenty-five years duty, or in caſe of incurable ſickneſs, receive their penſions for life, and may even receive them while reſident in other countries. On the death of a profeſſor, his widow and children continue to receive his penſion, until the widow marries again, or the children attain the age of twenty-one years. The Emperor has granted a yearly revenue to the Univerſity of 130,000 rubles.

Forms its own ordinances and regulations.

Various privileges,

Proviſion for profeſſors, &c.

Botanic Garden, &c. at Copenhagen.

A ſum of 4,500 rix dollars, which the government had granted in 1803, for the Botanic Garden at Copenhagen, has been employed, partly in paying the debts of the eſtabliſhment, and partly in conſtructing a new hothouſe. This garden, which poſſeſſes 5,500 plants of different kinds, is open one day in each week for the curious, and every day for botanic ſtudents. The directors in their laſt report, having made ſome propoſals for the improvement and eſtabliſhment of the garden, the government has granted them an additional ſum of 4000 rix dollars, and an annual ſum of 200 for repairs; and have beſides ſettled, that the appointments of perſons employed in the garden, ſhall be increaſed to 720 rix dollars, to commence this year.

has 5500 plants for inſpection,

additional grants made to it.

M. Gieſecke, a Pruſſian mineralogiſt, who has been a conſiderable time at Copenhagen, is about to be employed by government on a voyage to Greenland, where he is to paſs ſome years in examining that country, with regard to its mineralogy and geology. Hitherto the Moravian religious miſſionaries have alone been able to reſolve to live ſome

Propoſed mineralogical expedition to Greenland.

years in that country for the converſion of the natives: It will be no little honour to the ſciences, if M. Gieſecke ſhall bring himſelf to make a like ſacrifice for their advancement.

Charts printed by moveable types.

The Royal Academy of the Fine Arts, and the Mechanical Arts of Berlin, have received among their members M. F. H. Wagener, who has diſcovered a new method of printing geographical charts by a ſpecies of moveable types, which is found to anſwer better than engraving, and will undoubtedly be much cheaper.

Prizes given by the French war miniſter for

Marſhal Berthier, war miniſter of France, at the requeſt of general Mareſcot, has again eſtabliſhed the prizes which was given for the beſt works on fortification.

a work on ſubterranean fortification,

Two prizes have been granted to the beſt *treatiſes on ſubterraneous works.* The firſt was adjudged by the committee of fortification to Major Mouze, the ſecond to Captain Gillot; the committee has adjudged a third treatiſe deſerving of honourable mention, which has for its inſcription *artem experientia fecit*; the author of it is not known.

and one on a plan for a fortified barrack.

Another prize on the ſubject of a *project for a fortified barrack*, has been given to Captain Laurent. The committee have judged the project of Captain Bioſchevalier, and that of Lieutenant Colonel Geſbert, to deſerve honourable mention. The committee has rejected, for not correſponding with the propoſed ſubject, a project of Captain Mallet, for *a barrack intrenchment*; but have thought it worthy of particular mention, as a work which gives a very advantageous idea of the talents of this officer.

Many of the works which neither received prizes, or particular mention, exhibit ingenious contrivances, and intereſting obſervations. In general theſe two conteſts have fully proved the goodneſs of the Inſtitution, of which the object is to exite emulation in all the corps of the army, to propagate knowledge among them, and to extend the perfection of all the branches of the military art.

Catalogue of Leipſic fair, contained 3647 publications.

THE catalogue of the Leipſic fair, has this year contained two ſheets more than uſual. The muſical publications have been added to it. It contains 3,647 articles, furniſhed by 380 bookſellers. The number of romances is 271, of theatrical pieces 81, and muſic 95, forms 447 articles.

Publication of Arabic antiquities of Spain.

The academy of painting and ſculpture at Madrid, are about to publiſh a compleat collection of the Arabic antiquities of the kingdoms of Grenada and Cordova. In this work will be found not only views and plans of the monuments, and other remarkable matters of theſe countries, but alſo an explanation of all the inſcriptions, cyphers and hieroglyphics.

Dictionary of the Bunda language, ſerviceable to thoſe who have buſineſs in Africa.

THERE is ſoon to be publiſhed at Liſbon, a Dictionary of the Angola or Bunda language, with the explanation of all the words in Portugueſe. There has never before been a dictionary of this language. This will be publiſhed for the uſe of thoſe Portugueſe who have buſineſs to tranſact with the eſtabliſhments which their country poſſeſſes on the coaſt of Africa. No language is ſpoken there to ſo great an extent as this.

Mauſoleum of Chriſtina, by M. Canova,

THE celebrated ſculptor M. Canova, is engaged in erecting at Vienna, the ſplendid Mauſoleum of the Arch-dutcheſs Chriſtina, an immenſe compoſition of eight marble figures, larger than life; the models and execution of which have been long admired at Rome, where they were formed. M. Canova before his departure from Rome exhibited a coloſſal group, repreſenting Theſeus combating with a Centaur. This group is to be executed in marble for the city of Milan. The artiſts and connoiſſeurs of Rome ſeem to eſteem this work ſuperior to every other which has been executed by this ingenious and indefatigable artiſt.

and his Theſeus and Centaur.

Piracy of books puniſhed at Augſburg.

THE Magiſtracy of Augſburg have had the honour of being the firſt government of ſouth Germany, which have taken deciſive meaſures againſt the ſhameful traffic of book-piracy. It has confiſcated the entire edition, conſiſting of 500 impreſſions, of the work of Goener, on the political rights of Germany, which was pirated by Kranzfelder, a dealer in ſuch tranſactions, and has beſides compelled Kranzfelder to pay to the legitimate editor, the price of the copies which he had ſold.

Ruſſian marine Inſtitution.

THE Ruſſian government have formed at Peterſburg, an Inſtitution, whoſe deſign is the perfecting of all that belongs to naval armaments, and which is to be called the *Marine Muſeum*. This inſtitution is not merely to be a ſchool: all the ſciences neceſſary to a naval officer will be there taught and the *Muſeum* will beſides publiſh a journal which will treat of every thing relative to the marine. It will have alſo a cabinet of natural hiſtory, which will be open to all the pupils. This eſtabliſhment will be under the direction of the miniſter of the marine, and its members are to wear the naval uniform.

Ruſſian eſtabliſhments for education. Numbers of maſters and pupils.

ACCORDING to the report of the miniſter of public inſtruction, there is at preſent in Ruſſia 494 inſtitutions for education, directed by 1475 maſters, and attended by 33484 ſcholars. The expence of theſe eſtabliſhments coſts government annually almoſt two millions of Rubles. Among theſe are not reckoned thoſe for the corps of cadets, or for pages, the Academy of Arts, the Schools of Commerce, nor the Inſtitution for Female Education. Thoſe who know the ſtate in which Ruſſian education was at the acceſſion of Alexander, may judge by this detail what he has done towards enlightning his vaſt empire.

The Ruſſian catholics earneſtly concur in ſeconding his views. At an eccleſiaſtical aſſembly, convoked by the biſhop of Luſk and Shetomir, various meaſures have been taken in favour of the eſtabliſhments for education.

Theatre in the Crimea.

IN the town of Odeſſa in the Crimea, a theatre is building with much activity, according to the plans of M. Thomas de Thomon architect to the Emperor, and a profeſſor of the Peterſburg academy.

(This ſhews that the arts are even extending to this hitherto neglected part of the world, which certainly from its fine climate, and many other advantages, merits every attention of its enlightened and humane maſter.)

Geographical Dictionary of Ruſſia.

THE third volume of the *Geographical Dictionary of the Ruſſian Empire* has been publiſhed by the bookſellers, Gavy, Popow, and Luby.

AN important work is ſoon expected to appear at Peterſburg, by the ſcientific M. Delaunay, counſellor of ſtate, relative to Siberia, and the bordering countries.

Ancient manuſcripts diſcovered at Portici are

M. KOTZEBUE in his laſt tour to Naples relates ſome particulars which he ſaw in viſiting the Muſeum of Portici, which will be intereſting to the admirers of ancient literature.

" Eleven young men are at preſent employed in unrolling the manuſcrips, and two copy them. An Engliſhman called Haiter, is at the head of the eſtabliſhment. He relates that his aſſiſtants are much more expert and expeditious than they were formerly. He has great hope that he ſhall have the 600 manuſcripts, (which yet remain) decyphered, and has little doubt that he ſhall diſcover among them an *Ennius* and a *Menander*, as he flatters himſelf he has already a *Polybius* in hands.

On the day of the viſit, a Greek author, hitherto unknown named Kolotos, was diſcovered; his work is on philoſophy. As the names of the author are always inſerted at the end of each manuſcript, they can never be known until it is entirely unrolled. Seven latin authors have paſſed through the hands of Mr. Haiter, but all ſo much damaged that it was impoſſible to unroll them, which he the more laments as one of them appeared to be a *Livy*, at leaſt it was an hiſtorical work written in his ſtyle; all that he can diſcover of it, is, that it begins with an harangue, in which mention is made of the family of Acilius. They have to this time diſcovered five authors; *Philodemus*, moſt of whoſe writings have been found, and among others, a treatiſe on the *vices which are nearly, allied to virtues*. *Epicurus*, *Phædrus*, *Demetrius Phalereus*

the works of Epicurus, Phædrus,

and

Demetrius, and a Kolotos. and his *Kolotos* above-mentioned. Mr. Haiter regrets that he has hitherto only met with works on philoſophy, although among theſe many hiſtorical ideas, hitherto unknown, occur here and there: as happened in a diſſertation on anger, in which is cited the example of Cadmus puniſhed by Bacchus for having given himſelf up to this paſſion, a circumſtance hitherto unknown.

IT has for ſome time been an object of deliberation with me, to aſcertain by what means I might moſt effectually remedy an inconvenience which has ariſen from the diſtinguiſhed patronage this Journal has been honoured with: The great extent and value of original communications cannot but be duly eſtimated by the public; though at the ſame time it has neceſſarily followed, that various articles of news and other ſubjects in the foreign Journals, have in many inſtances been poſtponed, and in ſome rejected. To retain all the peculiar advantages of this work, and to afford ample ſpace for occaſional and foreign articles of value, the obvious means have appeared, that according to the practice of ſeveral other reſpectable works, each volume ſhould be concluded by a Supplementary Number; containing ſix ſheets, or 96 pages of printed matter, and two plates. And, as many of the former plates, like thoſe in the preſent number, have contained mathematical figures or outline delineations, capable of being advantageouſly condenſed, it is purpoſed in all the future numbers to give two very full plates, and ſixteen extra pages of matter, inſtead of the four plates hitherto given. By this arrangement every volume will in future contain 30 ſheets or 480 pages of matter, and 10 full plates; inſtead of 20 ſheets or 320 pages, with 16 plates leſs fully occupied. This addition of new matter to the amount of full one half more, will admit the inſertion of many intereſting articles which want of room muſt alſo have excluded.

⁂ *The plate of Rye-Harbour could not be finiſhed in time on account of the ſudden illneſs of the Engraver. It will be given gratis in the Supplement, which will be publiſhed Jan.* 1. *next, at the ſame time as No.* 50.

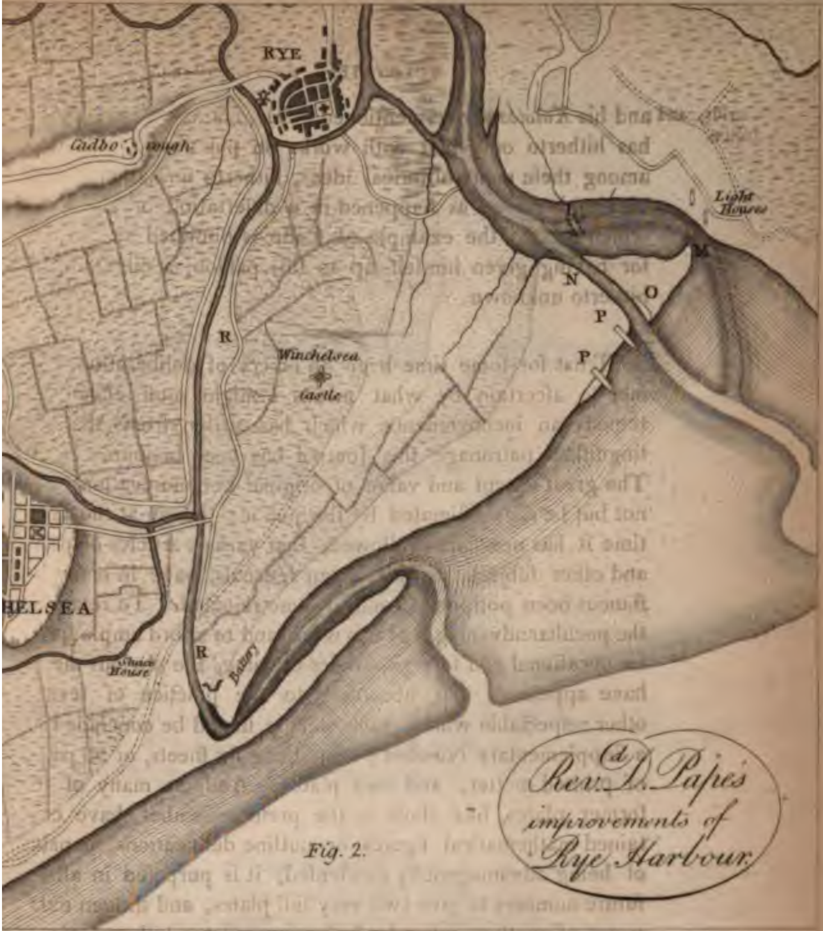
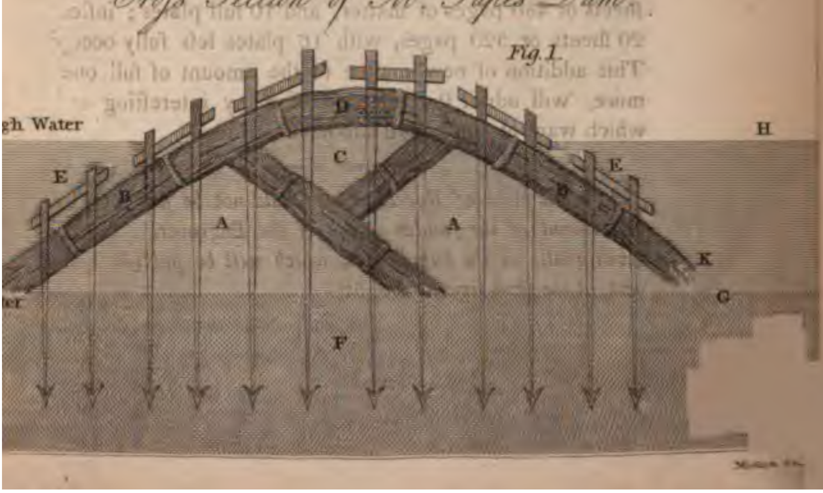


Fig. 2.

Cross Section of Mr. Pape's Dam

Fig. 1.



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A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

SUPPLEMENT TO VOL. XII.

ARTICLE I.

A Deſcription of an Air Pump upon a new Conſtruction. By **Elizur Wright.** *Communicated by* **Benj. Silliman,** *Eſq. Profeſſor of Natural Philoſophy, &c. in Yale College, Newhaven, America.*

The general imperfections of the air pump explained.

UPON reading the improvements made in the air pump by Smeaton, Haas, Prince, Ruſſel, and Cuthbertſon, it occurred to me that the end which they aimed at might in ſome meaſure be attained upon a principle that is different from either of thoſe by which their pumps have been conſtructed It is well known that in a common air pump the valve at the bottom of the barrel depends upon the air in the receiver to open it. When the air in the receiver is rarified to a certain degree, its ſpring becomes too weak to overcome even the ſmall reſiſtance which will ariſe from the weight of the valve, its coheſion to the plate occaſioned by the oil, and its being ſtretched tight over the hole. Here the progreſs of exhauſtion will ſtop. And this would hold true, could it be poſſible to produce a perfect vacuum in the barrel. But as the ſame obſtructions belong to the piſton valve, together with the additional one ariſing from the preſſure of the external air upon it, and becauſe the piſton cannot be ſo accurately fitted to the lower valve as, when put down upon it, to leave no vacuity between them; a portion of air will neceſſarily be retained in the barrel, which by its preſſure ſtill further prevents the opening of the lower valve, and cauſes the operator to come to the limit of rarefaction much ſooner than he would upon the ſuppoſition that a perfect vacuum were made in the barrel. Several very ingenious contrivances have been invented to remove this imperfection, among which thoſe of Cuthbertſon and Prince are among the lateſt, and cannot fail of giving the reader a very high idea of their ſagacity and talents for invention. The method uſed by the Rev. Mr. Prince, of removing the lower valve by opening the bottom of the barrel into a ciſtern which has a communication with the receiver, firſt gave the hint that it might be poſſible in ſome ſimilar way to diſpenſe with both the valves, and by this means carry the air pump to a greater degree of perfection. In purſuing this ſubject I found that all this might be effected, and in a way that admitted of much ſimplicity of conſtruction.

Improvements of Cuthbertſon and Prince.

The air-pump of Prince improved.

Deſcription of a new air pump.

The principle upon which this pump operates may be ſeen in the following deſcription of it. F, (*Plate* XIV. *Fig.* 1.) is the pump plate. O C is the barrel lying in a horizontal poſition underneath the pump plate, and nearly in contact with it. A and B are two ducts leading from the pump plate into the barrel. The piſton P is without a valve, being ſolid and accurately fitted to the barrel. The piſton rod M is cylindrical and moves air tight in the leathern collar O. There is another piſton, N, made like the former, but ſhorter, and acted upon by the ſpring S, which is thence termed the ſpring piſton. The ends of theſe piſtons are very carefully fitted to each other, ſo that when they are brought into contact they will form one uniform cylinder without any vacuity betwixt them. H is the winch, with a pinion and rack by which it is worked. The pump is ſupported by a pedeſtal upon which it is firmly fixed.

A ſolid piſton works in a barrel. Its rod paſſes through a collar of leather. Two holes in the barrel ſerve, one to admit air from the receiver and the other to diſcharge it.

The manner in which it operates is this: Suppoſe the receiver placed over the duct A, leaving the duct B open to the external air, alſo the ſpring piſton in the ſituation N, excluding the external air from the barrel, as repreſented in the figure and the piſton P in contact with it. The piſton P, by moving towards the duct A, forms a vacuum in the barrel. When it paſſes by the duct A, it opens a communication between the

receiver

Mr. Gough's Propositions respecting a Division of the Arch of a Circle.

Fig. 2.

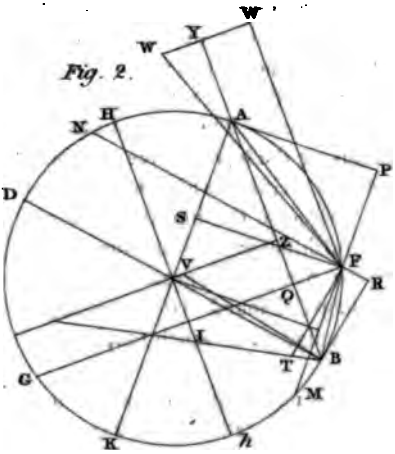


Fig. 3.

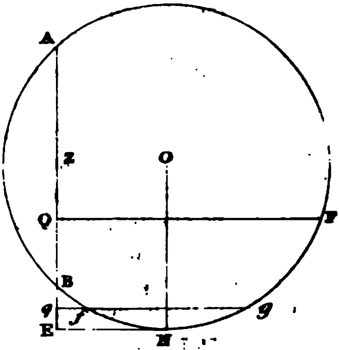


Fig. 1.

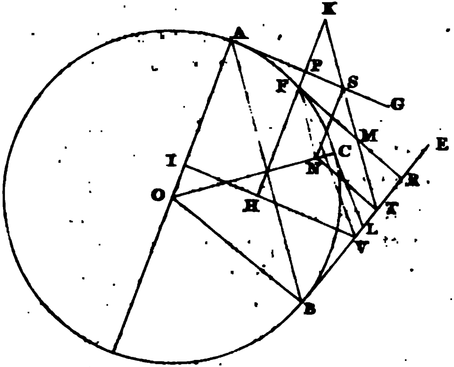
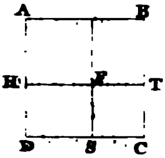


Fig. 4.



Mutlow Sc. Russell Ct.

Nicholson's Philos. Journal. Vol. XII. Pl. XIV. p. 306.

Dr. Herschell's investigation of the power of telescopes to distinguish the figures of very minute objects.

Fig. 1.

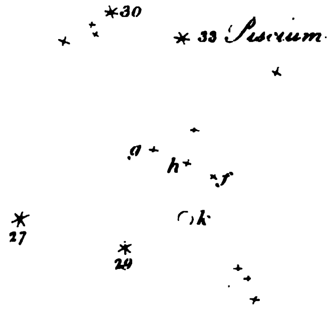


Fig. 2.

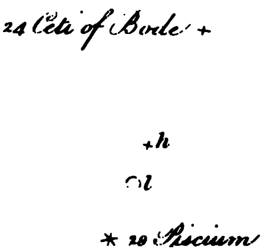


Fig. 3.

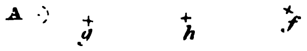
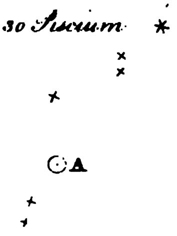


Fig. 4.



Mutlow Sc. Russell Ct.

receiver and barrel, and the air by its elaſtic force ruſhes into the barrel and fills it. The piſton now returns towards the duct B, and drives before it the air contained in the barrel, together with the ſpring piſton N, until they are ſtopped by the ſhoulder D at the inſtant in which the ends of the two piſtons come againſt the middle of the duct B. By forcing the air out at the duct B, the piſtons come into contact, and form one uniform cylinder, that prevents any communication of the barrel with the external air. The piſton P is now drawn back toward the duct A, and the ſpring piſton N, by the action of its ſpring, follows in cloſe contact with it, until it is ſtopped by its ſhoulder C meeting with the end of the barrel, after having paſſed the duct B, and having continued to intercept the communication between the barrel and the external air. This is the ſituation with which the deſcription began; and, repeating the operation, when the piſton P is drawn back beyond the duct A, the air from the receiver ruſhes into the barrel; and when it moves forward to the duct B this air is expelled.

The latter is cloſed by a ſpring piſton.

Having exhibited a general deſcription of this pump, with the manner of its working, a more particular illuſtration of ſome of its parts will be given.

When the pump is intended to exhauſt, the receiver muſt be placed over the duct A, leaving the duct B open to the external air; but when it is deſigned to condenſe, nothing more is neceſſary than to ſhift the ſituation of the receiver on the plate, placing it over the duct B, and leaving the duct A open to the external air.

This pump exhauſts or condenſes at pleaſure.

The duct B is continued around the ſpring piſton by means of a circular channel cut into the inſide of the barrel, in order that the air might eſcape from all ſides when the piſtons come into contact.

It may be obſerved that all the back ſpace in the barrel between the collar O and the piſton P makes a part of the capacity of the receiver; or, to ſpeak more accurately, the ſpace O A between the collar O and the duct A: the ſpace A P between the duct and piſton, while it moves from A toward B, being only a temporary dilation of the capacity, and the ſpace A P while it moves from A towards O a temporary contraction of it.

Obſervations.

For the purpoſe of preventing a fluctuation of the air in the receiver, which would be cauſed by this expanſion and con-

Advantage of conſtructing it with two barrels.

traction; and might be detrimental in ſome experiments, the diameter of the duct A is made very ſmall, and another barrel, having ſimilar piſtons and ducts, is added, with its rack placed above the pinion wheel, while the other is placed below it. The advantage of a pump of this kind being conſtructed with two barrels ariſes from the contrary motions of their piſtons; for while one augments the capacity of the receiver by moving forward, the other equally diminiſhes it by moving backward. An equilibrium is thus maintained that prevents any oſcillatory motion in the mercury of the gage, which might ariſe from the operation of a ſingle barrel.

The reſiſtance from the ſpring piſton.

The additional reſiſtance to be overcome in working this pump, above what is to be met with in other pumps, happens only at the ſmall interval while the ſpring piſton is paſſing from its natural ſituation to the duct B. This need not be more than about four times greater than that which is requiſite to overcome the friction of the piſton P, and will be eaſily provided or by increaſing the proportion between the diameter of the pinion wheel and the ſweep of the handle.

ELIZUR WRIGHT, *C. A. S.*

Canaan (Connecticut in America,)
March 12, 1805.

II.

Concerning the State in which the true Sap of Trees is depoſited during Winter. By Thomas Andrew Knight, *Eſq.*[*]

(Concluded from Page 240)

Bulbous and tuberous roots contain the matter that forms leaves,

We have much more deciſive evidence that bulbous and tuberous rooted plants contain the matter within themſelves which ſubſequently compoſes their leaves; for we ſee them vegetate even in dry rooms, on the approach of ſpring; and many bulbous rooted plants produce their leaves and flowers with nearly the ſame vigour by the application of water only, as they do when growing in the beſt mould. But the water in this caſe, provided that it be perfectly pure, probably affords little or no food to the plant, and acts only by diſſolving the matter prepared and depoſited in the preceding year; and hence the root becomes exhauſted and ſpoiled: and Haſſenfratz found that the leaves and flowers and roots of ſuch plants afforded no more carbon than he had proved to exiſt in bulbous roots of the ſame weight, whoſe leaves and flowers had never expanded.

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[*] Phil. Tranſ. of 1805, p. 97.

—and it is highly probable that trees contain the nutriment of their fruits, &c.

As the leaves and flowers of the hyacinth, in the preceding caſe, derived their matter from the bulb, it appears extremely probable that the bloſſoms of trees receive their nutriment from the alburnum, particularly as the bloſſoms of many ſpecies precede their leaves; and, as the roots of plants become weakened and apparently exhauſted, when they have afforded nutriment to a crop of ſeed, we may ſuſpect that a tree, which has borne much fruit in one ſeaſon, becomes in a ſimilar way exhauſted, and incapable of affording proper nutriment to a crop in the ſucceeding year. And I am much inclined to believe that were the wood of a tree in this ſtate accurately weighed, it would be found ſpecifically lighter than that of a ſimilar tree, which had not afforded nutriment to fruit or bloſſoms, in the preceding year, or years.

The preparation of this nutriment in the tree implies that the juices circulate.

If it be admitted that the ſubſtance which enters into the compoſition of the firſt leaves in the ſpring is derived from matter which has undergone ſome previous preparation within the plant, (and I am at a loſs to conceive on what grounds this can be denied, in bulbous and tuberous rooted plants at leaſt,) it muſt alſo be admitted that the leaves which are generated in the ſummer derive their ſubſtance from a ſimilar ſource; and this cannot be conceded without a direct admiſſion of the exiſtence of vegetable circulation, which is denied by ſo many eminent naturaliſts. I have not, however, found in their writings a ſingle fact to diſprove its exiſtence, nor any great weight in their arguments, except thoſe drawn from two important errors in the admirable works of Hales and Du Hamel, which I have noticed in a former memoir. I ſhall therefore proceed to point out the channels, through which I conceive the circulating fluids to paſs.

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

When a ſeed is depoſited in the ground, or otherwiſe expoſed to a proper degree of heat and moiſture, and expoſure to air, water is abſorbed by the cotyledons and the young radicle or root is emitted. At this period, and in every ſubſequent ſtage of the growth of the root, it increaſes in length by the addition of new parts to its apex, or point, and not by any general diſ-

tenſion

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

tenſion of its veſſels and fibres; and the experiments of Bonnet and Du Hamel leave little grounds of doubt, but that the new matter which is added to the point of the root deſcends from the cotyledons. The firſt motion therefore of the fluids in plants is downwards, towards the point of the root; and the veſſels which appear to carry them, are of the ſame kind with thoſe which are ſubſequently found in the bark, where I have, on a former occaſion, endeavoured to prove that they execute the ſame office.

In the laſt ſpring I examined almoſt every day the progreſſive changes which take place in the radicle emitted by the horſe cheſnut: I found it, at its firſt exiſtence, and until it was ſome weeks old, to be incapable of abſorbing coloured infuſions, when its point was taken off, and I was totally unable to diſcover any alburnous tubes, through which the ſap abſorbed from the ground, in the ſubſequent growth of the tree, aſcends: but when the roots were conſiderably elongated, alburnous tubes formed; and as ſoon as they had acquired ſome degree of firmneſs in their conſiſtence, they appeared to enter on their office of carrying up the aqueous ſap, and the leaves of the plumula then, and not ſooner, expanded.

The leaf contains at leaſt three kinds of tubes: the firſt is what, in a former Paper, I have called the central veſſel, through which the aqueous ſap appears to be carried, and through which coloured infuſions readily paſs, from the alburnous tubes into the leaf-ſtalk. Theſe veſſels are always accompanied by ſpiral tubes, which do not appear to carry any liquid: but there is another veſſel which appears to take its origin from the leaf, and which deſcends down the internal bark, and contains the true or prepared ſap. When the leaf has attained its proper growth, it ſeems to perform preciſely the office of the cotyledon; but being expoſed to the air, and without the ſame means to acquire, or the ſubſtance to retain moiſture, it is fed by the alburnous tubes and central veſſels. The true ſap now appears to be diſcharged from the leaf, as it was previouſly from the cotyledon, into the veſſels of the bark, and to be employed in the formation of new alburnous tubes between the baſe of the leaf and the root. From theſe alburnous tubes ſpring other central veſſels and ſpiral tubes, which enter into and poſſibly give exiſtence to, other leaves; and thus by a repetition of the ſame proceſs the young tree or annual ſhoot continues to acquire new parts, which apparently are formed from the aſcending aqueous ſap.

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

But it has been proved by Du Hamel that a fluid, ſimilar to that which is found in the true ſap veſſels of the bark, exiſts alſo in the alburnum, and this fluid is extremely obvious in the fig, and other trees, whoſe true ſap is white, or coloured. The veſſels, which contain this fluid in the alburnum, are in contact with thoſe which carry up the aqueous ſap; and it does not appear probable that, in a body ſo porous as wood, fluids ſo near each other ſhould remain wholly unmixed. I muſt therefore conclude that when the true ſap has been delivered from the cotyledon or leaf into the returning, or true ſap veſſels of the bark, one portion of it ſecretes through the external cellular, or more probably glandular ſubſtance of the bark, and generates a new epidermis, where that is to be formed; and that the other portion of it ſecretes through the internal glandular ſubſtance of the bark, where one part of it produces the new layer of wood, and the remainder enters the pores of the wood already formed, and ſubſequently mingles with the aſcending aqueous ſap; which thus becomes capable of affording the matter neceſſary to form new buds and leaves.

It has been proved in the preceding experiments on the aſcending ſap of the ſycamore and birch, that that fluid does not approach the buds and unfolding leaves in the ſpring, in the ſtate in which it is abſorbed from the earth: and therefore we may conclude that the fluid, which enters into, and circulates through the leaves of plants, as the blood through the lungs of animals, conſiſts of a mixture of the true ſap or blood of the plant with matter more recently abſorbed, and leſs perfectly aſſimilated.

It appears probable that the true ſap undergoes a conſiderable change on its mixture with the aſcending aqueous ſap; for this fluid in the ſycamore has been proved to become more ſenſibly ſweet in its progreſs from the roots in the ſpring, and the liquid which flows from the wounded bark of the ſame tree is alſo ſweet; but I have never been able to detect the ſlighteſt degree of ſweetneſs in decoctions of the ſycamore wood in winter. I am therefore inclined to believe that the ſaccharine matter exiſting in the aſcending ſap is not immediately, or wholly, derived from the fluid which had circulated through

Explanation of the manner in which the juices of plants circulate, their habitudes, changes, &c.

through the leaf in the preceding year; but that it is generated by a proceſs ſimilar to that of the germination of ſeeds, and that the ſame proceſs is always going forward during the ſpring and ſummer, as long as the tree continues to generate new organs. But towards the concluſion of the ſummer I conceive that the true ſap ſimply accumulates in the alburnum, and thus adds to the ſpecific gravity of winter-felled wood, and increaſes the quantity of its extractive matter.

I have ſome reaſons to believe that the true ſap deſcends through the alburnum as well as through the bark, and I have been informed that if the bark be taken from the trunks of trees in the ſpring, and ſuch trees be ſuffered to grow till the following winter, the alburnum acquires a great degree of hardneſs and durability. If ſubſequent experiments prove that the true ſap deſcends through the alburnum, it will be eaſy to point out the cauſe why trees continue to vegetate after all communication between the leaves and roots, through the bark, has been intercepted: and why ſome portion of alburnous matter is in all trees* generated below inciſions through the bark.

It was my intention this year to have troubled you with ſome obſervations on the reproduction of the buds and roots of trees; but as the ſubject of the Paper, which I have now the honour to addreſs to you, appeared to be of more importance, I have deferred thoſe obſervations to a future opportunity; and I ſhall at preſent only obſerve, that I conceive myſelf to be in poſſeſſion of facts to prove that both buds and roots originate from the alburnous ſubſtance of plants, and not, as is, I believe, generally ſuppoſed, from the bark.

I am, &c.

T. ANDREW KNIGHT.

Elton, *Dec.* 4, 1804.

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* I have in a former paper ſtated that the perpendicular ſhoots of the vine form an exception. I ſpoke on the authority of numerous experiments; but they had been made late in the ſummer; and on repeating the ſame experiments at an earlier period, I found the reſult in conformity with my experiments on other trees.

III.

Singular Method of forming Walls and Roofs of Rural Building, in Induſtan, communicated by M. LEGOUX DE FLAIX, *Officer of Engineers*[*].

Advantages of this method of building.

THE method which the Indians have uſed for many years, of forming their rural buildings, unites ſolidity, convenience, and wholeſomeneſs to economy, and facility of execution.

Reſiſts fire and inundations.

Houſes conſtructed in this manner have alſo the advantage of being abſolutely ſafe from conflagration, and of reſiſting even the moſt violent inundations.

In a country where ſtone is ſcarce, the rich build their houſes with bricks, which in many reſpects are preferable to ſtone; but poor people, ſuch as thoſe employed in agriculture, cannot go to that expence, even in India, where labour and materials are ſo cheap.

The habitations of villagers in moſt parts of the globe are built with earth walls, in India they are likewiſe covered with terraces of earth, and it is evident, that buildings formed with both walls and roofs of earth muſt neceſſarily be free from danger of fire.

To prove that buildings of this conſtruction are equally ſafe from inundations, it is ſufficient to ſtate, that on the banks of the Ganges and Indus, (rivers of vaſt magnitude both in their extent and their courſe, and whoſe great bodies of water cauſe the moſt deſtructive effects in their floods,) theſe houſes ſtand uninjured, though ſometimes iſolated in the midſt of immenſe inundations for fifteen or twenty days. It is extremely probable, that houſes built of ſtone or brick would not ſtand this great force of water equally well.

The earth dug from the foundations is pounded fine, and mixed with coarſe ſand or fine gravel,

To form houſes in this manner, the foundations of the outſide and partition walls are dug up, which are ſometimes from five to ſeven feet deep, and always proportioned to the heighth intended to be given to the walls. The excavated earth is expoſed till it becomes perfectly dry; if it is of a fat or argillaceous nature, it is carried to a place, prepared for the purpoſe, where it may be pounded into a duſt, and properly prepared for uſe; when in this ſtate, it is mixed with a third or an half of coarſe ſand, or ſmall gravel paſſed through a ſieve to clear it from pebbles. The fat earth is mixed with the ſand and gravel, and worked up well with it, ſo that the maſs may be of an uniform conſiſtence. It is then moiſtened with water five or ſix hours before it is wanted, and in the quantity neceſſary for a ſingle day's work alone.

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[*] Sonninus's Journal, II. 394.

then moiſtened with water.

The walls are raiſed all together,

from two to four feet thick.

In one two or three courſes in a day according to the thickneſs.

The mixture thus prepared is carried to the place of building, when the foundations are perfectly dry, and the walls are then built equally in every part at the ſame time, on a perfect level, in courſes, and brought up perpendicularly: each courſe of earth is from eight to ten inches in depth, and the whole breadth of the wall, which is ſeldom leſs than two feet thick, and never exceeds four; which dimenſions are always regulated by the intended height of the building, and the force of the floods, if it is near the river. When the walls are three feet and a half, or four feet thick, only one courſe is raiſed in a day; but when they are from two feet and half to three feet thick, two courſes are raiſed, and if they are but two feet thick, three courſes are ſometimes raiſed in that ſpace of time. This depends on the quickneſs of the deſiccation of the walls, which ſpeedily takes place there, where the dryneſs of the air is extreme: this would not perhaps happen in our moiſt climates; if this method of building ſhould be tried here, it would probably be neceſſary to leave them longer to dry, in order to obtain the requiſite tenacity.

Spaces left for beams, doors, and windows.

When the walls are built to the height for the roof, the proper openings are made for the beams and joiſts. It is almoſt needleſs to add, that the apertures neceſſary for the doors and windows are made while the walls are building.

The walls when dry are encloſed in open work caſes of bambou,

at two or three feet diſtance, and the interval filled with fuel.

On the twelfth or fifteenth day, or when the walls are ſufficiently dry, or to the ſame degree to which tiles are dried, the walls are ſurrounded externally and internally with a ſort of open work caſe, made of ſpars of bambou, or of ſome other hard and dry wood. In Indoſtan, where this method of building is general, the workman have bars of iron, which they hire out, that ſerve to ſuſtain the coffer work mentioned, and are placed at every three or four yards; the coffer work is raiſed at two or three feet diſtance from the ſurface of the walls, according to their thickneſs, and the ſpace between is filled up with firewood, turfs, and cakes made of cow and ſheep dung worked together and dried in the ſun.

This

Arranged in ſtages ſeparated by layers of earth.

The upper ſtages firſt kindled.

Walls thus baked to the thickneſs of from ſix to ten inches.

This pile of combuſtibles is arranged in ſeveral ſtages, of three, four, or five feet thick, ſeparated from each other by layers of earth or half dried turf of from eight to ten inches depth; the upper ſtages are firſt ſet on fire, ſo that the wall is baked through its whole extent from top to bottom. The charge of the combuſtibles for each of the ſtages is ſo managed, that the loweſt is the greateſt, and is diminiſhed for each as it is nearer the top of the wall; as the pile burns down the fire of the lower ſtages ſtill acts on the upper part of the walls, which permits the upper ſtages to be of leſs thickneſs. The fire bakes the walls to a thickneſs of from ſix to ten inches, as tiles are baked in a kiln. And thus walls are built in a ſingle piece, and of the greateſt ſolidity, which have the more ſtrength, as there are no junctures in them. Wherefore they ought to prevent the greateſt poſſible reſiſtance to the action of the atmoſphere, the attacks of floods, and the fall of rain, which deſcends in torrents in moſt countries of Indoſtan during the rainy ſeaſon.

These houſes laſt longer than thoſe of brick, and reſiſt floods and rain better.

Experience has conſtantly proved, that the houſes built in this manner not only laſt much longer than thoſe built of bricks, but that they alſo reſiſt better the attacks of the periodical inundations, and thoſe of the annual rains to which they are expoſed in this climate.

Their roofs are made of clay in three layers.

The method in which the terrace roofs (which are called in India *argamace*) are formed for theſe houſes, is the following:

Firſt layer common clay and *ole*, a kind of marl, four or five inches thick.

Second layer potters clay, two or three inches thick.

Immediately after the baking and cooling of the walls, the aſhes and the bars which ſuſtained the coffer work are removed. The beams and joiſts are placed, and covered either with very thin boards, or elſe with ſmall green branches; and upon this ſupport the different layers of the terrace roof are placed. The firſt layer is ſimply clay, with an equal quantity of *ole*, a ſpecies of marl in powder, which is pounded in troughs, ſuch as are uſed for preparing mortar. This firſt layer is four or five inches thick, and it is then levelled, and is moiſtened from time to time, in order to beat it firm with ſmall bats. As ſoon as this is dry, the ſecond layer is laid on, which conſiſts of potters clay worked up in the ſame manner as if prepared for making pottery; this layer is only two or three inches thick at moſt. It is levelled according to the ſlope of the terrace, which is given it in placing the beams and joiſts, and it is conſolidated by light blows of wooden trowels, until it is perfectly dry. When the clay forms cracks in drying they are cloſed by other clay prepared for filling up theſe chinks to the bottoms.

Third layer clay with one fourth brick duſt and one fourth fine ſand 6 or 8 inches thick.

The ſecond layer when perfectly dry, and free from cracks, is covered with a third layer; which is compoſed of pulverized clay mixed with a fourth of brick duſt, paſſed through a cloſe ſieve, and with a fourth of fine ſand. This mixture is worked up in a trough like mortar; it is uſed as ſoon as prepared, and is then ſpread out equally over the whole terrace ſix or eight inches thick; this layer is conſolidated in the ſame manner as the others, and this labour is continued till it is perfectly dry; and then the *argamace* is finiſhed. This terrace is ſtrong, and has ſuch tenacity that the moſt violent rains cannot penetrate it.

Houſes thus built coſt 6 franks for the cubic fathom.

A building of this ſpecies coſts in India but ſix francs (five ſhillings) for the cubic fathom and is entirely performed by maſons. In France it would coſt three times as much (and ſomething more in England) on account of the greater expence of labour and fuel.

May be made with many ſtories.

An houſe of this ſort 430 years built ſeemed quite freſh.

Houſes may be built in this manner of any height required, and of as many ſtories as are thought fit; I have ſeen ſome that had but one ground floor; but I have alſo ſeen others that were elevated two ſtories above the ground floor. One of this laſt ſort, ſituated on the banks of the Gemna in the province of Alabad, was built above 430 years, and the walls, and the whole of the building looked as freſh as if they were new.

IV.

Account of ſome new Improvements on Steam-Engines. By Mr. ARTHUR WOOLF.

(Concluded from page 296.)

Mr. Woolf's improvements in ſteam-engines.

“I have found out and invented a contrivance, by which the temperature of the ſteam veſſel or working cylinder of a ſteam-engine, or of the ſteam veſſels or cylinders where more than one are uſed, may be raiſed to any required temperature, without admitting ſteam from the boiler into any ſurrounding receptacle, whether known by the name of a ſteam caſe, or by any other denomination. That is to ſay, inſtead of admitting ſteam

Mr. Woolf's improvements in ſteam-engines.

ſteam of a high temperature into ſuch receptacle or ſteam caſe, which is always attended with a riſk of exploſion proportioned to the elaſticity of the ſteam employed, I put into the ſaid ſurrounding receptacle, or caſe, oil or the fat of animals, or wax or other ſubſtances capable of being melted by a lower temperature than the heat intended to be employed, and of bearing that heat without being converted into vapour: or I put into the ſaid caſe or caſes mercury or mixtures of metals, as of tin, biſmuth, and lead, capable of being kept in a ſtate of fuſion in a lower temperature than that intended to be employed in working the ſteam-engine; and I ſo form the ſurrounding caſe or caſes as to make it or them admit the aforeſaid oil, or other ſubſtance employed, to come into contact not only with the ſides of the ſteam veſſel or veſſels, or working cylinder or cylinders, but alſo with the bottom and top of the ſame, ſo that the whole may be as much as poſſible maintained in one uniform temperature; and this temperature I keep up by a fire immediately under or round the caſe or caſes that contains the aforeſaid oil or other ſubſtance, or by connecting the ſaid caſe or caſes with a ſeparate veſſel or veſſels, kept at a proper temperature, filled with the oil or other ſubſtance made uſe of as aforeſaid. In ſome circumſtances, or whenever the ſame may be convenient or deſirable, I employ the fluid metals, or mixtures of metals, and oil or other of the ſubſtances before enumerated, at one and the ſame time in the ſame engine; that is to ſay, in the part of the caſe or veſſel expoſed to the greateſt action of the fire, I ſometimes have the aforeſaid metals or mixtures of metals, and in the parts leſs expoſed to the action of the fire, I put oil, or other ſubſtances capable of bearing the requiſite heat without being converted into vapour.

" By this arrangement, and method of applying the ſurrounding heat, I not only obviate the neceſſity of employing ſteam of a great expanſive force round the ſteam veſſel or veſſels, or the working cylinder or cylinders, as already mentioned, to maintain them at the temperature required, but I am enabled to obtain from ſteam of a comparatively low temperature, or even from water itſelf, admitted into the ſteam veſſel or veſſels, all the effects that can be obtained from ſteam of a high temperature, without any of the

Mr. Woolf's improvements in ſteam-engines.

the riſk with which the production of the latter is accompanied, not only to the boiler and other parts of the machinery, but even to the lives of the workmen; for ſuch low ſteam, or even water, (but in every caſe ſteam is preferable,) being admitted into a ſteam veſſel or veſſels, or working cylinder or cylinders, kept at the requiſite higher temperature by the forementioned means, will there be expanded in any ratio required, and produce an effect in the working of the engine which cannot otherwiſe be obtained but at a greater expenſe of fuel, or with the riſk of an exploſion. By this means I can make uſe of ſteam expanded in any required ratio, or of any given temperature, without the neceſſity of ever having the ſteam of any greater elaſticity than equal to the preſſure of the common atmoſphere.

"Another improvement which I make uſe of in ſteam-engines conſiſts in a method of preventing, as much as poſſible, the paſſage of any of the ſteam from that ſide of the piſton which is acted upon by the ſaid ſteam to the other ſide which is open to the condenſer; and this I effect, in thoſe ſteam-engines known by the name of double engines, by employing upon or above the piſton mercury or fluid metal, or metals in an altitude equal to the preſſure of the ſteam. The efficacy of this arrangement will appear obvious, from attending to what muſt take place in working ſuch a piſton. When the piſton is aſcending, that is, when the ſteam is admitted below the piſton, the ſpace on its other ſide being open to the condenſer, the ſteam endeavouring to paſs up by the ſide of the piſton is met and effectually prevented by the column of metal equal or ſuperior to it in preſſure, and during the down ſtroke no ſteam can poſſibly paſs without firſt forcing all the metal through. In working what is called a ſingle engine a leſs conſiderable altitude of metal is required, becauſe the ſteam always acts on the upper ſide of the piſton. For ſingle engines, oil or wax, or fat of animals, or ſimilar ſubſtances, in ſufficient quantity, will anſwer the purpoſe, if another improvement, which conſtitutes part of my ſaid invention, be applied to the engine, namely to take care that in either the double or ſingle engine ſo to be worked, the outlet that conveys the ſteam to the condenſer ſhall be ſo poſited, and of ſuch a ſize, that the ſteam may paſs without

Mr. Woolf's improvements in ſteam-engines.

forcing before it or carrying with it any of the metal or other ſubſtance employed, that may have paſſed by the piſton; taking care at the ſame to provide another exit for the metal or other ſubſtance collected at the bottom of the ſteam veſſel or working cylinder to convey the ſame into a reſervoir kept at a proper heat, whence it is to be conveyed to the upper ſide of the piſton by a ſmall pump worked by the engine or by any other contrivance. In order that the fluid metal or metals uſed with the piſton may not be oxidated, I always keep ſome oil or other fluid ſubſtance on its ſurface, to prevent its coming in contact with the atmoſphere; and to prevent the neceſſity of employing a large quantity of fluid metal, I generally make my piſton of the depth of the column required, but of a diameter a little leſs than the ſteam veſſel or working cylinder, excepting where the packing or other fitting is neceſſary to be applied; ſo that, in fact, the column of fluid metal forms only a thin body round the piſton. In ſome caſes I make a hollow metallic piſton, and apply an altitude of fluid metal in the inſide of the working cylinder.

"It may be neceſſary, however, to ſtate, that in applying my improved method of keeping the ſteam veſſels of ſteam-engines at any required temperature to the engine known by the name of Savary's, in any of its improved forms, in which a ſeparate condenſer has been introduced, I ſometimes employ oil (or any other ſubſtance lighter than water, and capable of being kept fluid in the temperature employed, without being converted into vapour,) in the upper part of the tube or pipe attached to the ſteam veſſel; by which means ſteam of any temperature may be uſed without being expoſed to the riſk of partial condenſation by the admiſſion of any colder body into the ſteam veſſel; for the oil, or other ſubſtance employed for this purpoſe, ſoon acquires the requiſite temperature; and to prevent unneceſſary eſcape of heat, I conſtruct of, or line with, an imperfect conductor of heat, that part of the tube or pipe attached to the ſteam veſſel which may not be heated exteriorly. And further, (as is already the practice in ſome engines, and therefore not excluſively claimed by me,) I cauſe the water raiſed by the engine to paſs off through another aſcending tube than the one attached to the ſteam veſſel, but connected with it at ſome part lower than the oil or other ſubſtance employed in it is ever ſuffered to deſcend to in the working of the engine. The improvement which I have juſt mentioned, of introducing oil into the pipe attached to the ſteam veſſel of ſuch engines, may alſo be introduced without applying heat externally to the ſteam veſſel; but in this caſe part of the effect which would otherwiſe be gained is loſt."

V.

On the Magneſian Earth of Baudiſſero. By M. Giobert.

(Concluded from Page 284.)

The earth of Baudiſſero analyſed.

To aſcertain the proportions of theſe conſtituent parts of the magneſian earth, we lixiviated a given weight of it, and precipitated the ſulphuric acid from one part of it by acetite of barytes, and the lime from the other part by oxalate of ammonia.

It contains, beſides magneſia, ſulphate of lime,

The weight of the oxalate of lime, and that of the ſulphate of barytes, obtained from it, ſhewed us that it contained 1,60 of the ſulphate of lime. The experiments before recited determine the proportion of the ſilex contained.

—with ſilex,

—and carbonic acid,

To prove that of the carbonic acid, we both calcined a given weight of the earth in crucibles, from which ſyphons paſſed into bottles containing lime water; in order that the carbonic acid gas furniſhed by the earth might be precipitated, and alſo diſſolved conſiderable quantities of it in acids by the action of heat, and received the gas produced in bottles filled in like manner with lime water; the firſt method produced conſtantly the moſt. The carbonate of lime formed in theſe different experiments apprized us that 100 parts of the earth contained from 8 to 12 of carbonic acid, and ſometimes a little leſs in the ſtony ſpecies.

—and water.

If this weight of the carbonic acid be deducted from the loſs of weight which this earth ſuffers by the calcination in the fire, which was mentioned before, we ſhall then have the quantity of water which the earth contains. In collecting the reſults of the different experiments, it appears that the earth of Baudiſſero is compoſed of

Magneſia

Proportions tabulated.

Magneſia - - -	68
Carbonic acid - -	12
Silex - - -	15,60
Sulphate of lime - -	1,60
Water - - -	3
	100,20

It may be called with propriety native magneſia.

It is from theſe reſults that I denominate this earth native magneſia. It is doubtleſs found mixed with a little ſilex; but if the title of native alumen is given to the alumenous earth of Halle in Saxony, which contains 24 parts of the ſulphate of lime; if the name of native magneſia is given to that of Moravia, announced by Mitchael, of which 100 parts contain 50 of carbonic acid; it appears to me that the earth which I deſcribe has a much better title to the name which I have given it.

Suppoſed to be produced by the decompoſition of the Corneen ſtone or Cacholong.

The earth of Baudiſſero affords a ſubject for intereſting obſervations in the inveſtigation of its origin. Many facts lead me to believe that this earth and the Corneen ſtone or Cacholong, deſcribed and analyſed by my colleague Bonvoiſin, are both of the ſame nature. It appears to me that Cacholong at a certain point of its decompoſition forms what Bonvoiſin calls the hydrophane of Piedmont, and that in its complete decompoſition it forms the magneſian earth of which I here give the analyſis. Bonvoiſin has declared himſelf of an opinion preciſely contrary to this; for he has ſuppoſed that this earth, far from being the product of the decompoſition of Cacholong, is the element of its formation. Our colleague Gioanetti is of the ſame opinion. In theſe two hypotheſes, the change of one earth into another is manifeſt, that is to ſay, the change of ſilex and alumen into magneſia in my method of conſidering the matter; (for it is principally of theſe two earths that Cacholong and Hydrophane are compoſed, from the analyſis of Bonvoiſin;) and the change of magneſia into alumen and ſilex according to the hypotheſis of Bonvoiſin and Gioanetti. As this ſubject appears to me to be very intereſting, I intend to make a comparative analyſis of theſe ſtones at the different degrees of their decompoſition or entering into the ſtate of agate *(agatiſation)* which ſhall be the ſubject of another memoir.

And therefore that either alumen and ſilex is changed into magneſia, —or the converſe.

There remains yet for me to examine the economical uſes for which this earth may be employed.

The experiment which I related in the beginning of this memoir, of the decompoſition of the ſulphate of iron by this earth, which produced an excellent ſulphate of magneſia, indicates one of the methods in which it might be uſed to advantage.

Sulphate of magneſia may be manufactured from it to advantage by the proceſs with ſulphate of iron, deſcribed before, more pure than that of commerce in general.

Twenty-five pounds of ſulphate of iron coſt only three francs with us, while the price of the ſame weight of ſulphate of magneſia is eight francs, from this it follows that this proceſs may be followed to advantage. To this may be added that the ſulphate of magneſia of commerce, being impure, and mixed with much ſulphate of ſoda, cannot be compared to that which may be procured in this manner, which equals the beſt ſalt from * * canal; ſo that in this compariſon the more pure ſulphate of magneſia thus obtained, may be valued at ten francs at leaſt, and in reality is worth more.

This however is not the beſt method to purſue, when the operator has it in his power to follow the others, which I am going to recite.

The following experiments make known two proceſſes much more economical.

In the firſt experiment I took two pounds of the earth of Baudiſſero, reduced to a coarſe powder, with the ſame quantity of the ſulphuret of iron of Brozo reduced to powder in like manner, I mixed them together carefully, and treated one half in a crucible on the fire, and the other half in an iron capſule.

Or by pounding theſe and heating to redneſs in crucibles:

In both the mixture heated to redneſs emitted ſparks, eſpecially on being ſtirred. It ſeemed to become reduced to a very fine powder; a ſort of boiling took place, produced doubtleſsly by the diſengagement of carbonic acid, and here and there appeared flames of ſulphur, which burned without exhibiting any ſign of the production of a ſulphuret. The ſulphurous odour was not however very troubleſome, from whence it appeared that the magneſia abſorbed with readineſs the ſulphuric and ſulphurous acids in proportion as they were formed by combuſtion. The mixture became of a blackiſh grey, or more properly a black; but which appeared grey from the white particles which ſtill remained mixed with it.

After being left three hours to cool, it was moiſtened with water and put away till next day, a part of it was then lixiviated; the ſolution being made clear and treated with ammonia, gave an abundant and very white precipitate. This circumſtance indicating that much of the magneſia was combined with ſulphuric acid in the operation, all the remainder was lixiviated. The very clear lixivium, evaporated properly, produced at the firſt cryſtalization a pound of ſulphate of magneſia in beautiful cryſtals. The remaining liquor gave on ſucceſſive evaporations a pound and half more of the ſame ſalt in fine cryſtals, very dry and very white. The liquor produced cryſtals to the laſt drop, and the mother-water never became foul.

Leaving to digeſt ſome time, moiſtened with cold water, Lixiviating, and cryſtalizing, fine cryſtals in abundance are had.

The mixture which remained after lixiviation was roaſted a ſecond time, and again produced ſulphate of magneſia: It was then thrown away, although apparently it would have yielded more ſulphate of magneſia after another torrification.

The mother-water, evaporated ſucceſſively gives cryſtals to the laſt drop.

The reſiduum roaſted again and re-lixiviated gives more cryſtals.

In another experiment, pure ſulphur was uſed inſtead of the pyrites; it was eaſy to foreſee that the reſult would be the ſame; it was however deſirable to prove it; and the reſult was perfectly ſatisfactory.

Pure ſulphur uſed in place of the ſulphate of iron anſwers equally well.

The uſe then which may be made of this earth, conſiſts in forming with it ſulphate of magneſia. The means by which this may be done are perhaps the moſt ſimple poſſible. It is ſufficient to reduce to powder the earth and the ſulphur, or the ſulphuret of iron, where it can be eaſily procured, as may be done at Baudiſſero. Theſe ſubſtances ſhould be mixed in almoſt equal parts; for it is uſeful to proceed with an exceſs of the earth, and the more ſo, as its coſt is almoſt nothing: The mixture ſhould be torrified in an oven or kiln, heated to the degree at which ſulphur inflames, and when there appear no more jets of ſulphurous flames, the kiln is to be left to cool. The matter being then drawn out ſhould be moiſtened with water in ciſterns, and left for ſome days, only taking care to ſtir it in that time.

Proceſs for ſulphate of magneſia in the large way with magneſian earth and pyrites, ſimilar to the foregoing,

The part of the ſulphur which in burning had only paſſed to the ſtate of ſulphurous acid, oxigenates gradually, or the ſalt, which at firſt was but a ſulphite changes to a ſulphate. The matter is then to be lixiviated, in the ſame manner that is uſed for nitrous earths, the liquor ſufficiently evaporated, and left to cryſtalize by cooling.

Another proceſs in the large way, where pyrites are burned. The kiln may be covered with a heap of magneſian earth.

Another method may be followed in places where ſulphurets are worked; or where, as at Brozo, there is a manufacture of ſulphate of iron. The kiln, where the pyrites are burned, may be covered with an heap of the magneſian earth; the ſulphuric acid, which is diſengaged will be abſorbed by the magneſia; and to the advantage of putting an end to the complaints of the owners of property near the manufacture, will be added that of *ſulphating* the magneſia, from which the ſalt may afterwards be procured by lixiviation. This laſt proceſs, if it were introduced into the manufactory of Brozo, would produce the ſulphate of magneſia of commerce at a very moderate price.

Which when *ſulphated* by the acid vapours, may be lixiviated.

The magneſian earth uſeful for porcelain and pottery,

As the magneſian earth of Baudiſſero forms an excellent porcelain with ſilex, it preſents beſides an intereſting ſubject for reſearch relative to the fabrication of pottery. With this earth and a quantity of the argillaceous earth of Caſtellamonte ſufficient to unite it into a paſte, I formed ſome crucibles and capſules. Theſe crucibles were expoſed for 48 hours in the furnace of the glaſs-houſe of Po. The earths did not ſeem to have formed a ſufficient union; nevertheleſs the hardneſs of the crucibles was ſuch that they could not be affected by the file. Doctor Gioanetti, who is now engaged in manufacture of ſtone-ware pottery, will hereafter throw light on this ſubject.

and mixed with argil forms crucibles extremely hard.

I end this part of my memoir with obſerving that the trials which I made of this earth as an abſorbent in veterinary medicine ſucceeded perfectly well.

Additions to the preceding Memoir, by the ſame.

The earth of Caſtellamonte is ſimilar to that of Baudiſſero.

Farther reſearches which I made on argillaceous earths have given me to underſtand that the earth of Baudiſſero is not the only one known that conſiſts for the moſt part of magneſia. The ſame kind is alſo found at Caſtellamonte, a large village near that of Baudiſſero.

M. Bertoline, doctor of medicine, one of the moſt eminent of my pupils, having repeated the detail of the experiments which we made at the general ſchool of chemiſtry, in[illegible] to eſſay a particular earth of Caſtellamonte, his country, which he thought would furniſh the alumen which was [illegible] ſucceſsfully in the earth of Baudiſſero; found alſo, by the care of M. Onorato, ſurgeon of Caſtellamonte, who is the [illegible]

[illegible]

prietor of the land where this earth is found, I received a large quantity of it, and we examined it comparatively with that of Baudiſſero.

The earth of Caſtellamonte, which was brought to us had nearly the ſame appearance as that of Baudiſſero; but when it is firſt dug up from the ground, it has on the other hand different external appearances, which ſeem to depend on the different degrees of decompoſition of the Corneen ſtone or Cacholong, which is found at Caſtellamonte as well as at Baudiſſero.

When firſt dug up appears different in colour.

The colour of this earth is a white inclining to bluiſh. In a maſs this earth is opaque; but when ſmall fragments of it of a minute thickneſs are examined, they have a ſemi-tranſparency.

In this reſpect it has a ſtrong reſemblance to horn; it is very ſoft, and may be cut with a knife like hard cheeſe. It is more unctuous to the touch, and a little more adheſive to the tongue than the earth of Baudiſſero.

Reſembles horn, cuts like hard cheeſe, more unctuous, and adheres more to the tongue than the firſt.

Treated with the acids, like the ſoft ſpecies of Baudiſſero, it becomes diluted, and then diſſolves, but has however a very remarkable difference, which is that it diſſolves in all the acids without the leaſt efferveſcence.

Does not efferveſce with acids.

It alſo does not yield the leaſt appearance of carbonic acid on expoſing it to the fire in cloſed veſſels furniſhed with ſyphons, which communicate with lime water.

This earth, like that of Baudiſſero, does not contain the leaſt trace of alumine or of oxide of iron.

It contains, ſimilarly to that of Baudiſſero, a little ſulphate of lime and muriate of magneſia, which may be ſeparated by lixiviation in water.

Conſiſts of the ſame ſubſtances as the firſt.

The remainder conſiſts entirely of magneſia and ſilex; but the proportion of this laſt is greater in it than in that of Baudiſſero. It may be computed at from 10 to 20 hundredth parts.

But contains more ſilex.

When this earth is kept in contact with the air its external characters change.

Its colour becomes by degrees a dull white, the ſame as has been remarked of the earth of Baudiſſero.

Its ſemi-tranſparence is loſt; its particles ſeparate, and in two or three weeks it is found to have abſorbed carbonic acid to that degree as to make as marked an efferveſcence with the

Changes its appearance on expoſure to the air, to a dull white; Loſes its ſemi-tranſparence, and abſorbs carbonic acid, ſo as to efferveſce with acids, and be-

acid

comes like the earth of Baudiſſero, with the ſmall difference noticed.

acid as the earth of Baudiſſero. In a word, it is completely the ſame as this laſt, with this ſole difference, that phyſically conſidered it is leſs compact, and becomes even friable, and chemically conſidered it contains a little more ſilex.

Earth of Baudiſſero has not acid enough to be carbonate of magneſia.

If dug from a ſufficient depth would probably contain no carbonic acid.

It appears then to be well proved that the earth of Baudiſſero and that of Caſtellamonte are each a true native magneſia, mixed with a little ſilex. In the earth of Caſtellamonte it is ſufficiently demonſtrated that it contains no carbonic acid when in the boſom of the earth; and that it only contains it when, after a long expoſure to contact with the air, it can abſorb it from the atmoſphere. That of Baudiſſero contains in truth carbonic acid, but the quantity is much inferior to that which it ought neceſſarily to contain to be conſidered a carbonate of magneſia; beſides the earth of Baudiſſero having been worked for a long time, and being thus in contact with the air, it is from the atmoſphere it muſt have drawn it, and that in proportion to the time it has been expoſed; at leaſt I have no doubt that if the earth of Baudiſſero was dug up from a certain depth, no carbonic acid would be found in it.

Earth of Caſelette probably magneſian alſo.

The author propoſes to analyſe Cacholong and Hydrophane, and write a memoir of the reſult.

I will conclude this addition to the memoir, by obſerving that the earth of Muſinet at Caſelette, being produced by the decompoſition of the ſame Corneen ſtone or Cacholong, ought alſo probably to be a magneſian earth; but I have not yet made any experiments on this earth; Doctor Bonvoiſin, who has given the analyſis of it in its ſtate of Cacholong and Hydrophane ſtone, propoſes in conjunction with me to repeat the analyſis of this ſtone, in the true ſtate above-mentioned, and in its earthy ſtate; which ſhall be the ſubject of a particular memoir. *

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* The laſt uſe which Mr. Giobert mentions for magneſian earth is of the moſt conſequence of the two, for as ſulphate of magneſia is only uſed in medicine, the ſale could not be ſufficiently extenſive to produce much profit on a large ſcale.

The uſe of this earth for pottery is the more deſerving of notice, as it has hitherto been ſuppoſed that argil was alone proper for this purpoſe; and though it was long known that magneſia is of a very refractory nature in the fire, Mr. Giobert ſeems to be the firſt who thought of uſing it in crucibles; which is the more extraordinary, as the *lapis ollaris*, which derives its name from its property of ſerving to make utenſils to bear the fire, is well known to contain a large proportion of magneſia.

VI.

Firſt Communication on an artificial Tan prepared fron Goal, chared Wood, reſinous Subſtances, &c. Abridged from the Original of CHARLES HATCHETT, *Eſq. F. R. S.**

First diſcovery of tannin by M. Seguin and Deyeux.

MR. HATCHETT firſt notices, that the natural tannin was firſt extracted from the matters which contain it, by Mr. Deyeux, who conſidered it as a ſpecies of reſin; that Mr. Seguin firſt diſcovered it to be the ſubſtance which in the proceſs of tanning renders animal ſkins inſoluble in water, and imputreſcible; but that Mr. Chenevix alone had noticed the effect of heat in giving coffee berries the power in decoction of precipitating gelaten. He then ſtates, that his experiments on lac, and ſome of the reſins having ſhewed him the powerful action of nitric acid on ſuch ſubſtances, induced him to try its effect on aſphaltum and jet; theſe with it formed a dark brown ſolution, and a precipitate, which by digeſtion in another portion of the acid became diſſolved, and on evaporation produced a yellow viſcid ſubſtance ſoluble in water and alcohol, and perfectly ſimilar to that obtained by ſimilar means from the reſins, excepting that when burned it had the colour of the fat oils. This reſult led to the ſuppoſition, that the dark brown ſolution was of the carbonaceous

Reſult of experiments on ſolution of lac, induced others on aſphaltum and jet, with nitric acid.

As native magneſian earth would doubtleſs be of great uſe to the potteries of this country, it is a pleaſing conſideration, that it is extremely probable it may be found in England, as well as on the Continent; for not only ſteatites and other magneſian ſtones have already been diſcovered here, but that ſalt, which it is M. Giobert's principal object to manufacture, is the natural produce of this country, and therefore the neighbourhood of Epſom, which gives it its name, may well be ſuſpected of containing beds of an earth ſimilar to that of Baudiſſero.

There is alſo ſome reaſon to ſuppoſe, that this earth may be one of thoſe ingredients in china-ware, which the Chineſe endeavour to keep ſecret; indeed it is hardly probable they ſhould be ignorant of its uſe, in a country, where the fineſt earthern-ware has been manufactured in the greateſt perfection, from periods antecedent to the dates of the authenticated hiſtory of Europe, and where of courſe experiments relative to the compoſition of this article, muſt have been varied to the greateſt extent.—B.

* Philoſ. Tranſ. 1805.

matter, and that the yellow precipitate was the eſſ[illegible]al part of the bitumens, which was confirmed by reſults from amber; ſeveral experiments were tried with various ſorts of pit-coal, from all which the brown ſolution was obtained in abundance, but thoſe which contained little or no bitumen did not yield the yellow precipitate.

Pit-coal treated in the ſame manner,

Proceſs with the Coal.

100 grains of coal, in each experiment, were digeſted in an open matraſs in a ſand heat, with an ounce of nitric acid (of the ſp. gravity of 1.40) diluted with two ounces of water, which when warm produced efferveſcence, and diſcharged much gas; after two days, a ſecond, and ſometimes a third ounce of the acid was added, and the digeſtion continued for five or ſix days, when nearly the whole was diſſolved, except the precipitate which was conſtantly ſeparated.

and charcoal.

Charcoal was next tried, which diſſolved more readily than the pit coal, and left no reſiduum.

The ſeveral ſolutions from aſphaltum, jet, pit coal, and charcoal, were evaporated to dryneſs gradually to prevent burning the reſidue, which from all were of a gloſſy brown ſubſtance, of a reſinous fracture, and had the following properties.

Properties of the reſidua of theſe ſolutions.

1. They were ſpeedily diſſolved by cold water, or alcohol.

2. Their flavour was aſtringent.

3. Expoſed to heat, they ſwelled much, and gave a bulky coal.

4. Their ſolutions in water reddened litmus paper.

5. And gave copiouſly precipitates from muriate of tin, acetite of lead. oxy-ſulphate of iron, of a brown colour, except the tin, which was dark grey.

6. They precipated gold in the metallic ſtate from its ſolution.

7. They alſo precipitated the nitrates of lime, and of barytes, and other earthy ſalts.

8. The fixed alkalis, and ammonia added at firſt, deepened their colour, and afterwards made them turbid.

9. They cauſed precipitates from glue or iſinglaſs ſolutions in water, more or leſs brown according to their ſtrength, which were ſoluble in cold and boiling water, ſo that in their

eſſential properties they proved ſimilar to thoſe formed by the varieties of tannin hitherto known, except that they contained no gallic acid or mucilage.

Reſiduum of animal coal has all the ſame qualities, nearly,

Animal coal from iſinglaſs was alſo tried in the ſame manner, this diſſolved very ſlowly, but left a little of the coal unchanged, its ſolution was of a deeper colour, and managed as the others deſcribed, produced ſimilar effects with the reagents, except ſome difference in the colour of the precipitates; and alſo gave an inſoluble precipitate from the ſolution of iſinglaſs; by which the curious fact is proved, that one portion of the ſkin of an animal may be made to convert another into leather.

and of coak alſo.

Coak gave a ſolution reſembling that of pit coal, but did not produce the ſame yellow precipitate.

Tanning ſubſtance beſt procured from carbon, uncombined with any thing but oxigen.

Theſe experiments ſhew, that the tanning ſubſtance is beſt procured from carbonaceous matter when it is uncombined with any ſubſtance but oxigen; which was confirmed by experiments on Bovey coal, Suſſex coal, Surturband from Iceland, and deal ſaw-duſt, which being diſſolved in nitric acid, and evaporated, the reſidues diſſolved in water, neither precipitated gelaten, or ſhewed any other ſigns of tanning matter; but when the ſame materials are charred, and treated as before deſcribed, they copiouſly produced the artificial tan; as did alſo teak wood, which Mr. Hatchett had proved to contain neither tannin or gallic acid in its uncharred ſtate.

Carbonization of vegetable matter by ſulphuric acid.

Mr. Hatchett had made ſeveral experiments on the ſlow carbonization of vegetable matters in the humid way, principally by ſulphuric acid, occaſionally diluted. Concentrated ſulphuric acid poured on any reſinous ſubſtance reduced to powder, diſſolves it in a few minutes; the ſolution is tranſparent, of a yellow brown colour, and a viſcid oil-like conſiſtence, but after being placed on the ſand bath, grows darker, evolves ſulphuric acid gas, and at laſt becomes a thick liquid of an intenſe black.

Effects of ſolution of turpentine in ſulphuric acid dropped into water.

Sulphuric acid of the above ſtrength poured on common turpentine diſſolves it readily, if a portion of the ſolution is then dropped into cold water, a precipitate of common yellow reſin is formed; if after another hour or two, another portion is treated in the ſame way, the reſin produced is of a dark brown, and that thus formed from a ſolution that has ſtood five or ſix hours, is completely black. When the digeſtion is continued for ſeveral days, until no more gas is given out, the reſin will be converted into a black porous coal, which does not contain any reſin, if the experiment has been properly conducted. This coal was about 43 *per cent* of the reſin uſed, and after being expoſed in a platina crucible looſely covered to a red heat, ſtill amounted to 30 *per cent*, and by the ſlowneſs of its combuſtion, and other circumſtances, approached nearly the character of ſome mineral coals.

Products of this operation diſſolved in nitric acid.

A portion of the coal, the black reſin, brown reſin, and yellow reſin obtained from the turpentine deſcribed, and alſo ſome of the turpentine itſelf, were each diſſolved in nitric acid, and reduced to dryneſs; the reſidua, which varied in colour from yellow to dark brown, according to the ſubſtance employed, were diſſolved in water, and examined with iſinglaſs and other reagents.

Their reſidues after evaporation are tried with gelaten, &c

The ſolution from the turpentine reſiduum, that of the yellow reſin, and the brown reſin, did not precipitate gelaten.

Effects produced.

That from the black reſin yielded a conſiderable portion of the tanning ſubſtance, and that from the coal afforded it in great abundance. Hence it appears, that theſe ſubſtances yielded artificial tan only in proportion to their converſion into carbon.

Various kinds of wood, copal, amber and wax, reduced to coal by ſulphuric acid, yielded ſimilar products on being treated with nitric acid.

Tan formed by alcohol.

Mr. Hatchett formed the artificial tan from the reſins, and gum reſins (ſuch as common reſin, elemi, aſſafœtida, &c.) when reduced to the ſtate of coal from long digeſtion with ſulphuric acid, by means of alcohol, without uſing any nitric acid: In the carbonized ſtate mentioned, they are digeſted in the alcohol, a portion is diſſolved, a dark brown ſolution is formed, which by evaporation yields a maſs ſoluble in water as well as in alcohol, and which precipitates gelaten, acetite of lead, and muriate of tin, but produces only a ſlight effect on oxy-ſulphate of iron.

Suppoſition relative to tan from peat.

The author ſuppoſes, that the tanning matter known to be evolved by peat in certain places, is effected by a proceſs in ſome reſpects ſimilar to the above, ſince if it was produced by its mere digeſtion in water, all peat would afford it, which is contrary to experience.

Mr. Hatchett put his diſcovery to the teſt of real practice, having actually *converted ſkins into leather by the artificial tan* procured as deſcribed, but obſerves, that the production of this ſubſtance, for the preſent, muſt be conſidered only a curious chemical fact, not altogether unimportant, and not capable of economical application, though he hopes, that hereafter a proceſs may be diſcovered for preparing this ſpecies of tan ſufficiently cheap to enable tanners to uſe it in their buſineſs.

Leather made by the artificial tan.

Hope of its economical production.

There is reaſon to ſuppoſe, that it would be ſuperior to common tan for this purpoſe, as it appears from experiments mentioned in Mr. Hatchett's ſecond paper on the ſame ſubject (which will be given in the next number) that "ſolutions of the artificial tanning ſubſtance ſeem to be completely imputreſcible, neither do they ever become mouldy like the infuſions of galls, ſumach, catechu, &c."*

Reaſon for thinking it ſuperior to common tan.

VII.

Memoir on the Diſcovery of a Factitious Puzzolana, preſented to the French National Inſtitute, by M. Dodun, *Engineer in Chief of Bridges and Highways in France* †.

THE depoſited duſt of ancient volcanic ſubſtances, has been long uſed in Flanders, and the adjacent countries, as a ſubſtitute for the Italian puzzolana, under the name of *traſs*, or *aſhes of Tournai*.

Factitious puzzolana of Flanders,

M. Faujas has proved by deciſive experiments, made by order of government, that certain lutulent eruptions of ancient volcanoes at Vivarais, had the ſame qualities as the

that of Faujas.

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* The peculiar tanning property of the water of certain peat bogs and moraſſes, may be otherwiſe accounted for, than by Mr. Hatchett's ſuppoſition, by the fact, that peat is not uniformly the production of the ſame vegetable ſubſtances: wherever heath, tormentil, and perhaps ſome other plants, are found in abundance, the peat water will have this quality; in the caſe of heath, at leaſt, it cannot be doubted; and perhaps the peat which does not yield tan may owe this deficiency to the total abſence of vegetables of this ſpecies.—B.

† *Journal de Phyſique*, Tom. 61.

that of M. Bagge.

puzzolana of Italy, and might be uſed inſtead of it. M. Bagge, a Swede, is alſo known to have compoſed an artificial puzzolana cement, with a black, hard, and flatey ſchiſt; but until 1787, no one ever thought that the territory of France contained in abundance non-volcanic ſubſtances capable of taking place of the Italian puzzolana with economy and advantage.

M. Dodun diſcovered his by chance.

The diſcovery which I here preſent has, like many others of great utility, been the effect of chance.

Saw great beds of ferrigunous oxides at Caſtelnaudery.

Obſerved burnt fragments in the fields like compact lava,

The habit of examining the nature of ſtone in its bed, which enables the obſerver to judge of its qualities at firſt ſight, fixed my attention on an immenſe quantity of calciform fragments of iron ore, in beds of from eight to ten feet thickneſs, following exactly the parallelifm of the ſlightly inclined declivities, in the neighbourhood of Caſtlenaudery. I perceived in the adjacent fields many ſubſtances of the ſame nature ſcattered over the ſurface of the earth, of violet, brown, and black colours, which from their appearance, had a perfect reſemblance to compact lava, which ſeemed extraordinary in a country where there was no appearance of ancient craters, or of volcanic eruptions. Theſe I ſoon found out had been brought to this ſtate by ſerving as hearths, or encloſures to the fires kindled in the fields by the peaſants, either for agricultural purpoſes, or perſonal convenience when they watch their flocks in winter; as I ſaw ſoon after many ſimilar arranged by hand on one another for theſe purpoſes.

which ſimilarity made him think them fit for puzzolana.

The ſimilarity of theſe fragments to volcanic products excited my deſire to form a cement from them, by treating them in the ſame manner as puzzolana earth. The great quantity of iron which theſe oxides ſeemed to me to contain, the abundance of their ſiliceous particles, and the alumen which evidently entered into their compoſition, their great weight, and their non-efferveſcence with acids, altogether made me preſume, that the cement formed from them would bind under water, and my expectation was not deceived.

Convinced by long experiments of its ſuperiority to the Italian.

Propoſed for uſe in the public works.

Fifteen months ſucceſſive experiments, to diſcover the proportion of lime which this oxide would abſorb to harden in water, without cracking when in the air, have convinced me, that my factitious puzzolona had all the good properties of that of Italy, without its faults. At this time I determined to propoſe its uſe in the public works, and demanded that comparative experiments ſhould be made between it and the

and tried comparatively before the Commiſſaries of Languedoc.
In the reſervoir near the lock of St. Roch.

the Italian puzzolana, in preſence of the Commiſſaries of the Province of Languedoc, and of the Directors of the canal which joins the two ſeas. Great blocks of *Beton* compoſition made with both cements, were thrown into the reſervoirs adjacent to the lock of Saint Roch, at Caſtelmandery, being firſt plaiſtered over with the reſpective compoſitions

Its ſuperiority to the Italian ſort.

Six months after, the water was drawn off from the bodies of maſonry, and it was then ſeen that the factitious puzzalona had acquired a ſolidity at leaſt equal to that of Italy. The plaiſter made with the Italian puzzalona was cracked and chapped, but that formed from the factitious kind had entirely preſerved the unity of its ſurface.

Teſtimony in its favour from the ſtates of Languedoc.

The ſtates of *Etats* of Languedoc altogether convinced of the authenticity of this diſcovery, by the reſults of the comparative trials of both kinds of cement which they had ſeen, and by the certificates of their commiſſaries, and perſuaded of the great advantage it would be to France, decreed in 1789, in their laſt meeting, that the factitious puzzolana ſhould not only be uſed inſtead of the Italian in the works under their direction; but moreover, that it ſhould be demanded in favour of the author of it, as a teſtimony of public gratitude, that government ſhould authorize the free circulation of it every where.

Extenſive works of it began.

The great conſumption of this factitious puzzolana obliged me to extend its manufacture, I formed a partnerſhip with the proprietor of the ground. The foundation of an eſtabliſhment on a great ſcale was laid at the mountain itſelf where the materials were found. The works carried on in its vicinity were likely to farther reduce the coſt of the article, which was already one half leſs than that of Italy, and the public were about to enjoy the advantages of this manufacture, when the revolution paralyſed every thing.

Stopped by the revolution.

The diſcovery declared to the conſtituent aſſembly.

Approved of and rewarded.

In 1791 I informed the conſtituent aſſembly of this diſcovery; the certificates which proved it, and the reſults of the experiments were depoſited at the office; the matter was ordered to be examined by M. M. Pelletier and Berthollet, and the aſſembly conſidering, that this factitious puzzolana might be of the greateſt uſe to France, decreed that 2000 francs ſhould be granted to its author, which was paid accordingly.

On this occaſion the celebrated Mirabeau declared the diſcovery to be ſo valuable, "*that if it had not yet been made, public encouragement ſhould be held out to excite it.*"

The troubles of France retards the manufacture of it.

The Conſtituent Aſſembly wiſhed to have numerous ſimilar eſtabliſhments ſet on foot in France, ſo well were they convinced of its national importance; but the misfortunes of the times prevented the execution of a project, which the grand Chief of the empire may eaſily realize, to the advantage of the country, whenever it ſeems good to him to do ſo.

Reſearches on the amelioration of our cements, and particularly on the nature of the materials proper to form artificial puzzolana, led me to try the calcination of various ſchiſts, of the bitumenous, ferruginous, and argillaceous ſorts.

Examination of different ſchiſts.

Contain too little iron for puzzolana.

The black ſlatey ſchiſt of M. Bragge, ſo common in France, was not forgotten: It is almoſt the ſame as that which the elder M. Grathieu eſſayed at Cherbourg laſt year; but I have conſtantly found that theſe ſchiſts always contain too little iron. I perceived that their repulſion of the water was ſlow and feeble, and that their ſolidification in the water was owing to the good quality of the lime.

Puzzolanas owe their qualities to the iron contained.

I was thus obliged to recur to my quartzoſe oxides of iron, from their containing a greater quantity of ferruginous principles; and can aver with the ſkilful Faujas, that the puzzolanas owe their property of hardening in water ſolely to the ferruginous particles which they contain: of this I have had many proofs. This truth is farther demonſtrated in the pudding-ſtones, the breſcias, and generally in all the amygdaloides with a ferruginous baſe or cement.

Theory of cements little advanced.

The theory of our cements is but little advanced; perhaps we take ſimple conjectures for proofs relative to them. We effect the regeneration of ſilex, and of the carbonate of lime; we know the acid gaſes which perform the principal part in the affair: but in this important work we have been long ignorant of the degrees of their reciprocal affinity, their quantity, and the mode of their reſpective combinations. Our knowledge on this matter is confined to a few facts.

Two different preparations of the puzzolana.

Many experiments have proved to me that the puzzolana, which ſooneſt forms a body in the water, is not fit to be employed in the open air, where it cracks and chaps in all directions. And that which is proper for the air, and which acquires

quires and preſerves its tenacity in it, ſets but imperfectly in water. This difficulty, of which the Inſtitute will perceive the cauſe, has obliged me to keep two ſorts of the factitious puzzolana; on the reciprocal uſe of which a memoir of inſtruction will accompany the ſale. The two ſorts may be diſtinguiſhed by their colour.

The factitious puzzolana proper for works under water, is of a reddiſh-brown. That which is fit for works expoſed to the air, is a dark violet. The latter is uſed for terraces, the embankments of baſons, for the compoſition of incloſures, or for light roofs. Bridges of a ſingle arch may be formed with it; and I have ſeen it adhere ſo ſtrongly to glazed tiles, that it was neceſſary to break the tiles to detach it.

One fit for water-works.

Another for expoſure to the air, and proper for terraces, roofs, and arches.

The puzzolana proper for conſtructions beneath the water, forms the moſt ſolid body in it. Three months after immerſion it is an actual ſtone capable of receiving a poliſh. The lime in it is always regenerated into carbonate of lime in ten weeks.

Water puzzolana forms a ſtone capable of a poliſh.

When it may be thought by any one that he has been deceived as to the certainty of theſe effects, it will always be found, that he either has not obſerved the quantities directed of the puzzolana and the lime, or that he has uſed the reverſe of that kind of the cement proper for the work.

Nullity of effect cauſed ſolely by miſtakes of the operator.

I commonly uſed lime in the ſtate of impalpable powder, ſlacked in Laſaye's manner, for works expoſed to the air; and employed lime in the ſtate of *putty*, for works which were to be covered with water. Sometimes I uſed lime in powder for the ſame work. This difference depends on the degree of goodneſs of the lime, on its greater or leſſer richneſs, or its proportional poverty. Cuſtom gives the advantage of knowing the different kinds on mere inſpection.

Lime uſed in powder with it; and in putty.

The uſe of lime in powder appeared to me to merit a preference in the preparation of mortars or cements. I prepared my factitious puzzolana in a certain quantity as ſoon as I knew the proper proportion of the lime; and I had thus the advantage of being able to work it in troughs in the ſame manner as ſulphate of lime. The whole was well mixed together and put into ſacks; by which means the maſons had nothing to do with the mixture of the articles (which is too often left to unprincipled workmen); and being thus maſter of the re-

Advantages of getting the materials ready mixed.

ſpective proportions of the puzzolana and the lime, I could always be aſſured of the ſolidity of my cements.

Exterior characters of the iron oxides uſed.

There remains for me to deſcribe the exterior characters of the quartziferous ferruginous oxides, which form the baſis of my factitious puzzolana, and to relate the analyſis of them which I made about 18 years ago. I will content myſelf with offering the comparative reſults with the Italian puzzolana, both in the dry way and the moiſt.

Exterior Characters of the quartziferous Oxides of Iron.

Slight calcination changes them from brown to red.

A greater changes them to a deep brown.

Their colour is of a reddiſh-brown before calcination, or ſlightly violet. A light torrification gives them a clearer red tint or a deep violet: one more intenſe renders them of a deep brown or of a violet-brown inclining to a black. The degree of the calcination for uſe is confined to thoſe two ſtates.

Long continued heat renders them black and porous like lava.

Urged at a longer continued heat, the colour becomes a deep black, then the ſubſtance becomes porous, entirely ſimilar to certain lavas of our modern or ancient volcanos, with which it is then difficult not to confound them.

Their fracture grained and earthy, and they contain quartz cryſtals.

Their fracture is grained and a little earthy, and ſmall cryſtals of quartz may be diſtinguiſhed in them by the naked eye, and almoſt always angular fragments of gray or milky quartz; a powerful magnifying lens cauſes in ſome fragments the diſcovery of needles of ſchorl, the amphibole of Hauy, and ſome ſmall tourmalines.

Needles of ſchorl, amphibole, and tourmalines.

Their ſmell argillaceous.

Their ſmell is ſtrongly argillaceous on breathing on them with the mouth.

Give no ſparks.

There is no fire produced by the uſe of the ſteel, when it does not ſtrike a quartoſe particle.

Do not effervеſce.

They do not effervеſce with acids either cold or hot.

Are affected by the magnet.

The magnet acts a little on theſe oxides before calcination, and ſtrongly, or perceptibly, after it.

Weight of a cubic foot.

The medium weight of a cubic foot is 125°; that of the Italian puzzolana is but 91°.

Analyſis by the moiſt Way.

Analyſis in the moiſt way of the iron oxides,

I ſhall not weary the aſſembly by a detail of the manipulations relative to the ſolvents and re-agents which art uſes for the decompoſition of bodies, and ſhall only ſay, that ſilex, iron, alumen, and a ſmall portion of mangaueſe, are the conſtituent parts of theſe oxides.

I repeated theſe experiments many times, and had for a medium reſult from an hundred pounds, chemical weight,

50 parts of ſilex;
31 —— of iron;
16 —— of alumen;
3 —— of mangaueſe, and loſs.
———
100

If this analyſis be compared with that of the puzzolana of Italy, which contains in 100 parts—

and of the Italian puzzolana.

50 of ſilex;
25 of alumen;
16 of iron;
3 of lime;
6 of loſs;
———
100

their reſpective properties may be appreciated according to the proportions of their integrant parts.

The exceſs of alumen cauſes the plaiſters made from the Italian puzzolana to crack and chap in the open air: this fault ariſes from their great oxidation. I have been able to replace in them thoſe principles which they loſt by decompoſition.

Exceſs of alumen cauſes the Italian kind to crack.

Analyſis in the dry Way.

I endeavoured to obtain a regulus from theſe oxides of iron by uſing a violent heat. I followed the proceſs of Kirwan for the fuſion of ſiliceous and argillaceous ores of iron; yet I never obtained a ſingle metallic button; and only found at the bottom of the crucible a vitrified maſs of an opaque black, or a ſcoria in the ſtate of crude caſt iron.

Analyſis in the dry way by Mr. Kirwan's proceſſes; afforded no metal.

Deſirous to know if I could procure a malleable button by uſing the blowpipe, taking borax for the flux and ſupporting the oxide on charcoal; I ſtill could only obtain a ſpongy ingot reſembling crude caſt iron, and breaking both when hot and when cold.

The blowpipe tried unſucceſsfully.

Being placed on a ſupport of glaſs (according to my method publiſhed in the Journal de Phyſique, Tome XXXI. pages 116 and 139), the oxide fuſed at the ſecond attempt, the ſup-

The oxide fuſed on glaſs.

port was coloured green, and ſmall grains of iron were ſeen to paſs firſt of a dark-green colour, then of a bright green, and afterwards to diſappear in evaporating. There remained on the globule only a ſlight tinge of blackiſh-green.

General reſult.

The reſult of all theſe facts ſeems to be, that this oxide is entirely deprived of its metallic principle, and that its ſuperoxigenation renders it reducible and refractory.

The oxide may be uſed as a pigment.

The arts may draw ſome advantage from theſe oxides, by uſing them in pigments for buildings. I ſucceeded after many waſhings, in extracting from them a beautiful brown-red colour equal to that of commerce, and applied it to uſe ſucceſsfully.*

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* This paper has been abridged in its introduction, in the details relative to negociations with the Conſtituent Aſſembly, and in ſome other points a little irrelevant to the puzzolana; but all matters directly tending to illuſtrate its nature and properties have been carefully copied.

M. Dodun's diſcovery may be of ſome uſe to this country, as there are in many parts of it large maſſes of iron-ſtone, and ſome is found in the vicinity of moſt coal-mines.

It has been long known that iron ochres have the ſame property of forming puzzolana with lime, when properly roaſted, and this circumſtance is mentioned at large in Chaptal's Chemiſtry. A patent has alſo been obtained in this country for the application of iron pyrites to the ſame purpoſe, the right to which was purchaſed long ago by Mr. Samuel Wyat. But the novelty of M. Dodun's diſcovery is, that poor iron-ſtone is equally fit for this uſe, as the other ſubſtances mentioned, which is of the more importance as it is very plentiful, and may often be procured in ſituations where the others cannot.

It may not be amiſs to mention here, that baſalt treated in the ſame manner, has the ſame property as the puzzolana: the whinſtone, of which the ovoidal paving-ſtones conſiſt moſtly, is of this kind; and it is found in great abundance in theſe countries, in different forms.—B.

Experiments

VIII.

Experiments and Obſervations upon the Contraction of Water by Heat at low Temperatures. By THOMAS CHARLES HOPE, *M. D. F. R. S. Ed. Profeſſor of Chemiſtry in the Univerſity of Edinburgh. From the Edinburgh Philoſophical Tranſactions, for* 1804.

Expanſion of water by cold, &c. near the freezing point.

TO the general law, that bodies are expanded by heat, and contracted by cold, water at the point of congelation, and for ſome degrees of temperature above it, it ſeems to afford a very ſingular and curious exception.

The circumſtances of this remarkable anomaly have been for ſome time believed to be the following:

General or uſual ſtatement of the facts. Ice-cold water contracts by increaſe of temp. to about 42°, and then expands;

When heat is applied to water ice cold, or at a temperature not far diſtant, it cauſes a diminution in the bulk of the fluid. The water contracts, and continues to contract, with the augmentation of temperature, till it reaches the 40th or 41ſt degree. Between this point and the 42d or 43d, it ſuffers ſcarcely any perceptible change; but when heated beyond the laſt-mentioned degree, it begins to expand, and increaſes in volume with every ſubſequent riſe of temperature.

and ſuffers the reverſe change by cooling.

During the abſtraction of caloric, the peculiarity in the conſtitution of water equally appears. Warm water, as it cools, ſhrinks, as other bodies do, till it arrives at the temperature of 43° or 42°. It then ſuffers a loſs of two degrees without any alteration of denſity. But when farther cooled, it begins to dilate, and continues to dilate, as the temperature falls, till congelation actually commences, whether this occurs as ſoon as the water reaches the 32°, or after it has deſcended any number of degrees below it.

This ſuppoſed peculiarity has

Suppoſing this peculiarity of water to be eſtabliſhed, it muſt appear, indeed, a very odd circumſtance, that heat ſhould produce contraction in this fluid, while it cauſes expanſion in other bodies *; and no leſs ſtrange, that within one range of temperature it ſhould contract, and in another expand, the very ſame ſubſtance. Before a deviation from ſo general a law ſhould be received as matter of fact, the proofs of its exiſtence ought to be clear and indiſputable.

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* Is this mode of change peculiar to water?—I do not know of any experiments with other fluids, except that mentioned on page 343. Perhaps it may be common to all, or at leaſt to all thoſe which expand by congelation. Deciſive trials of this point are the more deſirable, becauſe ſome of Count Rumford's general inductions require or ſuppoſe that ſeawater ſhould not be thus affected.—N.

been hitherto deduced from experiments in narrow necked veſſels,

The experiments hitherto publiſhed, from which this ſingularity has been deduced, have all of them been performed upon water contained in inſtruments ſhaped like a thermometer glaſs, and conſiſting of a ball with a ſlender ſtem; and the expanſive or contractive effects of heat and cold have been inferred, from the aſcent or deſcent of the fluid in the ſtem.

of which the capacities alſo vary by change of temperature.

To ſuch experiments it has been objected, that the dimenſions and capacity of the inſtrument undergo ſo much change, from variation of temperature, that it is difficult, if not impoſſible, to determine how much of the apparent anomaly ought to be imputed to ſuch changes, and that it is not improbable that the whole of it may be aſcribed to them.

The author ſhows the effect by other means.

The object of this paper, which I have now the honour to read to the ſociety, is to prove by a ſet of experiments, conducted in a manner altogether different, that the common opinion is founded in truth, and that water preſents itſelf as that ſtrange and unaccountable anomaly which I have already deſcribed.

Previous hiſtory.

It is worth while, before detailing my experiments, to give a ſhort account of thoſe obſervations which led to the diſcovery of the fact, and which in ſucceſſion have extended our knowledge of it, as well as of thoſe obſervations which have at different periods been offered to diſcredit, and to bring it into doubt.

Dr. Croune firſt obſerved that water appears to expand before it freezes.

The firſt obſervation relative to this ſubject was made by Dr. Croune, towards the cloſe of the 17th century, while engaged in inveſtigating the phenomena of the great and forcible, though familiar, expanſion which happens to water at the inſtant of freezing; a matter which occupied in a conſiderable degree, the attention of his fellow-members of the Royal Society of London in the earlier years of that inſtitution.

His narrative. The experiment ſhewed that water roſe in a long necked veſſel by cooling:

I ſhall relate in his own words his firſt obſervation: " I filled a ſtrong bolt-head about half-way up the ſtem with water, a day or two before the great froſt went off, marking the place where the water ſtood; and placing it in the ſnow on my leads, while I went to put ſome ſalt to the ſnow, I found it above the mark ſo ſoon, that I thought the mark had ſlipt down, which I preſently raiſed to the water, and as ſoon as ever I mixed the ſalt with the ſnow, the water roſe very faſt, about one-half inch above it. I took up then the glaſs, and found the water all fluid ſtill: it was again ſet down in the ſalt and ſnow; but when I came about an hour after to view it, the ball was broke, and the water turned to hard ice, both in the ball and ſtem*."

From this experiment Dr. Croune drew the concluſion, that water, when ſubjected to cold, actually began to expand before it began to freeze. On announcing it, however, to the Royal Society, on the 6th of February 1683, Dr. Hooke immediately expreſſed ſtrong doubts, and aſcribed the aſcent of the water in the neck of the veſſel to the ſhrinking of the glaſs occaſioned by the cold.

Whence he inferred an actual expanſion. But Dr. Hooke aſcribed the effect to the veſſel.

To obviate this objection, and to preclude, as far as was poſſible, the influence of the change of capacity in the apparatus from an alteration of its temperature, a bolt-head was immerſed in a mixture of ſalt and ſnow, and into it, when cooled, was poured, to a certain height, water previouſly brought to near the freezing point. The water began inſtantly to riſe as before, and when it had aſcended about one-fourth of an inch in the ſtem, the veſſel was taken out, the whole water remaining fluid.

Dr. C. repeated his exp. with the ſame event, in a veſſel previouſly cooled;

Theſe experiments, ſupported by others of a ſimilar nature, communicated by Dr. Slare to the Society on the 20th of the ſame month, appear to have ſatisfied its members, in general, of this fact, that water, when on the point of congealing, and while ſtill fluid, is actually ſomewhat dilated previous to the remarkable expanſion which accompanies its converſion into ice.

which gave ſatiſfaction;

Dr. Hooke, however, continued unſhaken, and retained the doubts he had expreſſed.

but not to Dr. Hooke.

Remarkable as the fact, as now ſtated, muſt have appeared, it ſeems not to have excited particular attention, nor to have ſolicited more minute examination; and indeed though philoſophers did not loſe ſight of it, yet for near a century no one inveſtigated it more carefully. Mairan, in his treatiſe on ice in 1749, and Du Creſt in his diſſertation on thermo-

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* Birch's *Hiſtory of the Royal Society*, Vol. iv. p. 263.

meters

Modern experiments of De Luc.

meters in 1757, appear to be well aware of this property of water, but it is to M. De Luc that we owe the knowledge of the leading and more intereſting circumſtances, (*vide Recherches*, &c. 1772.)

Having devoted his attention to the examination and improvement of the thermometer, he was naturally led to the inveſtigation, while engaged in aſcertaining the phenomena of the expanſion and contraction of different fluids by heat and cold.

He uſed thermometer glaſſes, and found the water to deſcend by cooling till 41°, and then riſe till freezing;

He employed in his experiments thermometer glaſſes; and the included water, at or near the term of liquefaction, deſcended in the ſtem, and appeared to him to ſuffer a diminution of bulk by every increaſe of temperature, till it arrived at the 41ſt degree. From this point its volume increaſed with its temperature, and it aſcended in the tube. This fluid, when heated and allowed to cool, ſeemed to him to contract in the ordinary way, till its temperature ſunk to the 41°, but to expand and increaſe in volume, as the temperature fell to the freezing point.

The denſity of water, he thence inferred, is at its maximum at 41°, and decreaſes with equal certainty whether the temperature is elevated or depreſſed.

ſo that its denſity at 50° and at 32° appears the ſame.

M. de Luc ſays, indeed, that very nearly the ſame alteration in volume is occaſioned in water of temperature 41°, by a variation of any given number of degrees of temperature, whether they be of increaſe or of diminution; and conſequently that the denſity of water at temperature 50, and at temperature 32°, is the ſame.

His theory.

This philoſopher did not conceive that the conſtitution of water, in relation to caloric, undergoes a change at the temperature of 41°, ſuch that ſhort of this degree caloric ſhould occaſion contraction, and beyond it expanſion. He imagined that heat in all temperatures tends to produce two but quite oppoſite effects on this fluid, the one expanſion, the other contraction.

In low temperatures, the contractive effects ſurpaſs the expanſive, and contraction is the conſequence: In temperatures beyond 41°, the expanſive predominate, and the viſible expanſion is the exceſs of the expanſive operation over the contractive.

In

Sir Cha. Blagden's obſ. that water may be cooled many degrees below 32° without freezing, and continues to expand.

In 1788, Sir Charles Blagden added the curious obſervations, that water, which by ſlow and undiſturbed refrigeration permits its temperature to fall many degrees below its freezing point, perſeveres in expanding gradually as the temperature declines; and that water having ſome muriate of ſoda or ſea-ſalt diſſolved in it, begins to expand about the ſame number of degrees above its own term of congelation that the expanſion of pure water precedes its freezing, that is, between eight and nine degrees. More lately, (*Philoſophical Tranſactions*, 1801), he, or rather Mr. Gilpin by his direction endeavoured to aſcertain, by the balance and weighing bottle, the amount of this change of denſity cauſed by a few degrees of temperature.

Count Rumford's applications of this doctrine to the œconomy of nature.

Every one muſt be familiar with the uſe which Count Rumford has made of this peculiarity in the conſtitution of water, in explaining many curious appearances that preſented themſelves in his experiments upon the conducting power of fluids, and in accountiug for certain remarkable natural occurrences. The Count, with his uſual ingenuity, has endeavoured to point out the important purpoſes which this peculiarity ſerves in the economy of nature, and to aſſign the final cauſe of ſo remarkable an exception from a general law.

Mr. Dalton's experiments,

In recording the obſervations and opinions that have been publiſhed concerning this point, I might now, in order, notice thoſe of Mr. Dalton of Mancheſter, related in the fifth volume of the *Mancheſter Memoirs*, which tended to confirm and enlarge our knowledge of it. But as Mr. Dalton himſelf has called in queſtion the accuracy of the concluſion which had been drawn from his experiments, and from thoſe of preceding obſervers, I ſhall only remark, that they are of the ſame nature, and nearly to the ſame purport, as thoſe of M. de Luc.

who queſtions the truth of the concluſions,

It was in conſequence of a communication with which Mr. Dalton favoured me, three months ago, that my attention was directed to this ſubject. He informed me, that after a long train of experiments he was led to believe that he, and his predeceſſors in the ſame field of inveſtigation, had fallen into a miſtake with regard to the contraction of water by heat, and its expanſion by cold, in conſequence of overlooking or underrating the effect which the change in the capacity of the thermometer-ſhaped apparatus employed, muſt occaſion on the apparent volume of the fluid. He ſtated, in general terms, that becauſe the point of greateſt apparent denſity is different in different veſſels; viz. In earthen-ware at 34°, glaſs 42°, braſs 46°, and lead 50°.

that on ſubjecting water to different degrees of temperature, in inſtruments made of different materials, he found the point of greateſt denſity was indicated at a different temperature in each.

In an apparatus, having a ball of earthen-ware, it was at the 34th degree; of glaſs at the 42d; of braſs at the 46th; and of lead at the 50th. And as water could not follow a different law, according to the nature of the ſubſtance of the inſtrument, he conceived that the appearance of anomaly in this fluid originated entirely in the containing veſſel, which muſt cauſe the fluid in the ſtem to fall or riſe according as its expanſions are greater or leſs than thoſe of the included liquor.

A detail of theſe important experiments has, ere now, been tranſmitted for publication in the *Journals of the Royal Inſtitution of London* *.

Mr. Dalton ſupports Dr. Hooke.

I have already noticed that Dr. Hooke endeavoured to explain in the ſame manner the original experiment of Dr. Croune. This explanation apparently gathers much force from theſe experiments of Mr. Dalton.

De Luc and Blagden were attentive to the veſſel:

It is proper, however, to ſtate, that M. de Luc was perfectly aware of the alteration in the dimenſions of his glaſs apparatus, but deemed the change too trifling to have any material influence.

Sir Charles Blagden paid greater attention to the circumſtance, and by calculation attempted to appreciate what allowance ought to be made for the change of capacity in the amount of the apparent changes of volume.

and various reaſons afford ground for doubt.

When it is conſidered, that the whole amount of the apparent change is but very ſmall, and that the expanſibility of the glaſs is with difficulty aſcertained, and is variable by reaſon of the fluctuating proportions of its heterogeneous conſtituents, it muſt be acknowledged, that preciſion in ſuch a calculation cannot poſſibly be attained, and can ſcarcely be approached. On this account, all the experiments already noticed are open to the explanation of Dr. Hooke, and in ſome meaſure liable to the objection which he had urged. I confeſs, that the experiments of Mr. Dalton, in perfect concurrence with that explanation, created conſiderable doubts reſpecting the exiſtence of the peculiarity of water; againſt the probability of which circumſtance, all analogical reaſoning, and every argument *à priori*, ſtrongly militate.

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* They were tranſmitted to our Journal by the author in Vol. X. page 93.

Unwilling to remain in uncertainty, and conſidering it as a point of much curioſity and intereſt, I have endeavoured to inveſtigate the ſubject by experiments conducted in a totally different manner, equally calculated to exhibit the ſingular truth, but free from the objections to which the others are liable. In them, it was my object to provide, that neither the changes of the actual volume of the water, nor the alterations in the dimenſions of the inſtrument, ſhould have any influence whatever.

The author's experiments were not made uncertain by the cauſes before ſtated.

I have already taken occaſion to ſtate, that the purpoſe of this paper is to prove, by experiments on the principle now mentioned, that in the conſtitution of water there really exiſts the ſingularity often noticed.

I ſhall firſt ſtate the plan of the experiments, and then detail the particulars of the moſt remarkable of them.

When any body is dilated, whether by heat or cold, it neceſſarily becomes leſs denſe, or ſpecifically lighter; and the oppoſite effects reſult from contraction. This is the circumſtance, as every one knows, which cauſes various movements among the particles of fluids, when any inequality of temperature prevails in the maſs; hence theſe particles are little acquainted with a ſtate of reſt.

His attention was directed to ſhew whether water riſes or ſinks by the preceding changes of temperature;

If a partial application or ſubtraction of heat produce an inequality of denſity in a maſs of fluid, the lighter parts riſe to the ſurface, or the denſer fall to the bottom.

It readily occurred, that I might avail to myſelf of theſe movements, and upon ſtatical principles determine the queſtion in diſpute.

I had only to examine attentively water, as it was heated or cooled in a jar, and to obſerve, by means of thermometers, what ſituation the warmer, and what the cooler parts of this fluid affected.

which could be done by thermometers duly placed.

If I ſhould find that ice-cold water, in acquiring temperature, ſhowed, in its whole progreſs, the warmer parts near the top, it would indicate that water follows the uſual law, and is expanded like other bodies by heat.

Or if I ſhould obſerve that warm water, in cooling to the freezing point, had the coldeſt portion uniformly at the bottom, the

For the cold portions of water during

change of temperature would conſtantly be at the bottom, if denſeſt, throughout ſuch change.

the ſame concluſion would follow; while a different inference, and the exiſtence of the ſuppoſed anomaly, would be deducible ſhould the event prove different. The only circumſtance, I can figure to myſelf as tending in any meaſure to render this mode of examining the point doubtful, is, that water near its congealing point may have ſo little change of denſity occaſioned by a ſmall variation of temperature, that its particles may be prevented by their inertia, or by the tenacity of the circumfluent maſs, from aſſuming that ſituation which their ſpecific gravity would allot to them.

It will appear, however, very clear, from the circumſtances of the experiments which I ſhall immediately detail, that no obſtacle to the ſucceſs and preciſion of the experiments proceeded from this ſource.

It is not neceſſary for me to relate all the experiments I have made. I ſhall reſtrict myſelf to the detail of ſix, which preſent varieties in the modes of procedure, and which afford the moſt ſtriking reſults.

Exp. I. Ice-cold water expoſed to a warm atmoſphere, was warmer (1½°) below till 38°, after which it was warmer at top.

Exp. I. I filled a cylindrical jar of glaſs 8½ inches deep, and 4½ in diameter, with water of temperature 32°, and placed it on a table, interpoſing a conſiderable thickneſs of matter poſſeſſed of little power of conducting heat. I ſuſpended two thermometers in the fluid, nearly in the axis of the jar, one with its ball about half an inch from the bottom, the other at the ſame diſtance below the ſurface. The jar was freely expoſed to the air of the room, the temperature of which was from 60° to 62°.

The experiment commenced at noon:

	Top Thermom.	Bottom do.
	32°	32°
In 10 minutes,	33+	34+
— 30 ———	35.5	37
— 50 ———	37	38+
— an hour,	38	38+
— ——— and 10 minutes,	42	38.25
— ——— —. 30 ———	44	40
— ——— —. 50 ———	46+	41+
— 2 hours and 10 minutes,	48	42.5
— ——— —. 30 ———	50	44
— ——— —. 50 ———	50.5	45
— 4 hours,	54	49

Confiding in the indications of the thermometers, from this experiment we learn, that when heat flows on all ſides from the ambient air into a column of ice-cold water, the warmer portions of the fluid actually deſcend, and take poſſeſſion of the bottom of the veſſel.

This downward courſe proclaims an increaſed denſity, and teſtifies that the cold water is contracted by heat. As ſoon, however, as the fluid at the bottom exhibits a temperature of 38°, this courſe is retarded and ſoon ſtopped, and with the riſe of temperature beyond 40° is totally changed; for when the maſs attains this degree, the experiment equally ſhows, that the warmer fluid aſcends and occupies the ſummit, by its route announcing its diminiſhed denſity, and proving that water is now expanded by heat.

Whence it is concluded that the colder (upper) fluid was rarer at the temperatures near freezing; and the warmer (upper) fluid was rarer at the higher part of the ſcale.

Exp. II. I filled the ſame jar with water of temperature 53°; and that I might obſerve the phenomena of cooling, I placed it in the axis of a much larger cylindrical veſſel, nearly full of water, of temperature 41°, and, by an earthen-ware ſupport, raiſed it about three inches from the bottom, taking care that the water ſhould be on the ſame level in both veſſels. As ſoon as I had adjuſted the two thermometers, as in the former experiment, I obſerved that the top of the fluid was ſtill at 53°, but the bottom had fallen to 49°.

Exp. 2. Water at 53° was every where cooled by enveloping the veſſel with ice-cold water. It was warmer at top till 42°, after which it was warmer (4°+) below.

	Top.	Bottom.
In 9 minutes,	52°	45
— 15 ———	52	44

Now, to accelerate the cooling, I withdrew by a ſyphon the water from the large cylinder, and ſupplied its place by ice-cold water, mixed with fragments of ice, which by repeated cautious agitation was kept uniformly at the temperature of 32°.

In 23 minutes,	48°	42+
— 38 ———	44	40
— 43 ———	42	40
— 46 ———	40	40
— 52 ———	36	40
— 58 ———	35—	39
— 65 ———	34	37
— 75 ———	34	36
— 103 ———	34	34

This experiment is the counterpart of the foregoing, and from the teſtimony of the ſame inſtruments, it appears, that when a cylinder of water of 53° is cooled by circumfluent iced fluid, the colder part of the water takes poſſeſſion of the bottom of the veſſel, ſo as to eſtabliſh a difference of temperature from the ſurface, amounting ſometimes to 8°. And that as ſoon as the fluid at the bottom arrives at the 40th degree, the temperature of the fluid in that ſituation is ſtationary till the ſurface reaches the ſame point.

Whence the ſame concluſion is deduced as in the former experiment.

During the ſubſequent refrigeration, the progreſs of the cooling undergoes a total change. The thermometers tell that the colder fluid riſes to the ſurface; ſo that the top gets the ſtart of the bottom ſoon by 4°, and attains the loweſt temperature of 34° very long before the other falls to the ſame degree.

Theſe circumſtances, I think, lead to the concluſion, that by the loſs of caloric, water at 53° is contracted and rendered ſpecifically heavier, and that this continues to happen till the water come to the temperature of 40°, at which period an oppoſite effect is produced; for now the water, as it cools, becomes ſpecifically lighter, or is expanded.

In this, as well as the former experiment, the complete change in the ſituation, which the warmer and colder parts of the fluid affected, in the progreſs both of the heating and cooling, while every external circumſtance of the proceſs continued unaltered, is particularly worthy of remark.

Exp. 3. A tall jar, nearly 18 inches high, containing water at 50°, was cooled round its *upper part* by ice and ſalt. The temperature fell quickeſt at bottom, till 40°, where it continued ſtationary; after which the ſurface ſunk to freezing, and congealed.

Exp. III. I took a glaſs jar, 17.8 inches deep, and 4.5 in diameter internal meaſure, having a neck and tubulature very near the bottom. I provided alſo a cylindrical baſon of tinned iron, 4.8 inches deep, and 10 inches in diameter, with a circular hole in the middle of the bottom, large enough to receive the top of the jar. By means of a collar and cement I ſecured this baſon, ſo that it encircled the upper part of the jar.

The object of the contrivance was to have the means of applying a cooling medium to the ſuperior portion of a cylinder of water, and it anſwered the purpoſe completely. I introduced the ball of a thermometer through the tubulature, till the extremity of it nearly reached the axis at three-fourths of an inch above the riſing of the bottom, and having fixed it in this ſituation, I rendered the aperture water-tight, by a perforated cork and lute.

This very tall jar was placed on a table, with the interpoſition of ſome folds of thick paper, in a room without a fire, of the temperature 42°.

I filled it with water of 50°, and poured into the baſon, which embraced the top, a mixture of powdered ice and ſalt.

From time to time I explored the temperature near the ſurface, by inſerting the bulb of a thermometer to the depth of half an inch nearly in the axis.

The experiment laſted 50 hours.

	Bottom.	Top.	Air.
One o'clock,	50°	50°	42°
In 11 minutes,	46.+	—	
— 15 ———	45	48	
— 21 ———	44—	46—	
— 31 ———	42	44	
— 41 ———	41	42	
— 51 ———	40 +	34	At this time a thin film of ice began to form in contact with the glaſs.
— 1 hour 6 min.	40	34	
— ——— 20 ———	39.5		
— ——— 44 ———	39.5		
— 4½ hours,	39.5		A cruſt of ice of ſome thickneſs now lined the glaſs, and air had fallen to 40°.
— 5½ hours,	39		
— 11 hours, *i. e.* at midnight,	39		Cruſt of ice complete.
— 19 hours, *i. e.* next morning,	39		Air 40°.
— 26 hours,	40		Air 40°. So much ice had melted that the cake was detached from the ſide of the veſſel, and floated.
— 32 ——	40		
— 41 ——	40		Air 41°. Ice not all melted.
— 50 ——	41		Air 42°. Ice not entirely gone.

Review of the facts and remarks.

This long protracted experiment preſents ſome ſtriking facts, and its general import, with regard to the ſubject of inveſtigation, agrees with the preceding. In it we ſee, that when the frigorific mixture abſtracted caloric from the upper extremity of a cylinder of water, nearly 18 inches long, and at 50°, the reduction of temperature appeared ſooner, and advanced quicker, at its lower extremity than in the axis at the top, not two and a half inches diſtant from the cooling power. No one can entertain a doubt that this is owing to a current of cooled and condenſed fluid deſcending, and a correſponding one of a warmer temperature aſcending. Now, if water obſerved the ſame law that other bodies do, and had no peculiarity of conſtitution, the ſame progreſs of cooling ſhould continue. This, however, the experiment teaches us, is not the caſe: as ſoon as the fluid at the bottom exhibits a temperature of 40°, it ceaſes. The colder fluid remains at top, and quickly loſing temperature, ere long begins to freeze. The continuance of the colder fluid at the ſurface ſurely denotes, that it is not more denſe than the ſubjacent warmer water. The legitimate inference from this is, that water of temperature 40° is not contracted by being cooled to 32°.

As the fluid below 40° continued at top, it was not *denſer* than that at 40°.

Did water obſerve the uſual law, and loſe volume along with temperature, this experiment, by its long duration, afforded ample time for the manifeſtation of it.

For not leſs than two days did ice-cold water maintain poſſeſſion of the top, and for the ſame period the temperature at the bottom never fell below 39°. No current, therefore, of cold and condenſed fluid moved from the ſurface, to affect the inferior thermometer, or to atteſt the contraction of water by cold.

Yet the experiment does not ſhow that it was *rarer*.

This experiment, however, I muſt remark, does not warrant the concluſion, that the water is actually expanded, though it in no degree oppoſes it. It proves no more, than that the contraction ceaſes at 40°; and that water of 32° is not more denſe than of 39° or 40°. Nay, ſome may perchance alledge, that it does not prove ſo much; conceiving, that if at 40° the contraction, without ceaſing altogether, becomes very inconſiderable, the difference of denſity occaſioned by the ſubſequent reduction of temperature may be ſo very trifling, as not to enable the cold particles to take that ſituation which their gravity aſſigns to them, in oppoſition to the inertia and tenacity of the ſubjacent maſs; and therefore that the colder, though heavier fluid, may be conſtrained to remain above. That this allegation ſhould have no weight attached to it, the circumſtances of the ſucceeding experiment will clearly ſhow, as I ſhall ſoon notice.

It might be even alledged that a ſmall exceſs of denſity prevailed;

but this is not entitled to regard.

That heat which paſſed by direct communication

Before quitting the conſideration of the preſent experiment, it may be worth while to remark, that it may ſeem rather ſurpriſing,

priſing, that the bottom of the fluid was not apparently affected in its temperature by the ice which ſo long occupied its ſurface. It might be expected, though no cold currents deſcended from above, that the caloric ſhould be conducted from below, and that the temperature ſhould by that have been reduced *. I ſuppoſe that the caloric did paſs from the lower ſtrata upwards, but extremely ſlow, by reaſon that fluids, as Count Rumford taught us, are exceſſively bad conductors of heat, and ſo very ſlowly, that the caloric entered from the atmoſphere with ſufficient quickneſs to prevent any depreſſion of temperature below the 39th degree.

muſt have been very ſlowly tranſmitted.

This experiment, I may conclude with remarking, is very well calculated to exhibit the error of the popular opinion, that "heat has a tendency to aſcend."

* ANNOTATION, BY THE AUTHOR, †

The opinion of Count Rumford that fluids cannot conduct heat from particle to particle,

This experiment may perhaps be thought to give countenance to the opinion of the very ingenious Count Rumford, that fluids cannot conduct heat, and that no interchange of heat can take place between the particles of bodies in a fluid ſtate, ſeeing that for two days the fluid at the bottom of the veſſel never fell below 39°, though the ſurface was at 32°.

From the circumſtances detailed in his ſeventh eſſay, the Count concluded, that heat cannot deſcend in a fluid. From the preſent, it might with equal juſtice be inferred, that heat cannot aſcend.

appears to be inaccurate.

Had I not the fulleſt conviction that this celebrated philoſopher has puſhed his ideas too far, I might be diſpoſed to conſider this experiment as according well with the hypotheſis.

Soon after the intereſting ſpeculations of the Count appeared, I began to inveſtigate the ſubject; and, by a pretty long train of experiments, which I have annually taken an opportunity of detailing in my lectures, ſatisfied myſelf that he aſſigned to fluidity a character that does not belong to it. Though ſince the date of theſe experiments, the public has become poſſeſſed of ſeveral ſeries, well deviſed, and, in my opinion, of themſelves concluſive, it may yet be worth while to ſtate the tenor and reſult of them, by which the value of their teſtimony in favour of the conducting power of liquids may be eſtimated.

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† As this note ſubjoined at the foot of the page after the words *temperature ſhould by that have been reduced*, in the original, is of ſuch conſiderable length, I have taken the liberty of putting it in the ſame type as the text.—N.

Experiments to ſhew that heat can deſcend in fluids:

The experiments were of two deſcriptions.

The one ſet, of the ſame nature nearly with thoſe of Count Rumford, was deſigned to examine, Whether heat, when applied to the ſurface, can deſcend in a fluid; and the other to diſcover, Whether, on the mixture of different portions of fluid at different temperatures, an interchange of caloric takes place between the particles;—Water, oil and mercury, having been the ſubjects of the Count's experiments, were employed for the firſt ſet.

To water (and to oil) the heat was communicated from the bottom of a metallic veſſel (in contact with the ſurface of the fluid, and) heated by boiling water within.

To explore the conducting power of water and oil, the apparatus which I uſed conſiſted of two veſſels of tinned iron, both cylindrical, and the one ſomewhat larger than the other. The larger had a diameter of eleven inches, and into it were poured the ſubjects of the trial, to different depths on different occaſions. The ſmaller was ten and a half inches in diameter. By three hooks it was ſuſpended within the larger pan, in ſuch a manner, that the bottom of it exactly reached and came in contact with the ſurface of the fluid. This ſmaller veſſel became the ſource of the heat, by being filled with boiling hot water. The water was changed frequently, care being taken to avoid, by the uſe of a ſyphon, all agitation and diſturbance.

This hot veſſel did not touch that which contained the fluid under experiment; and the containing veſſel was kept cold round the ſurface of the fluid, and therefore did not carry any heat downwards.

In experiments of this nature, the difficulty is to prevent the conveyance of caloric by the ſides of the veſſel. I attempted, and, I think, I ſucceeded, in overcoming this difficulty, by encircling the larger veſſel, as a height exactly correſponding with that of the ſurface of the fluid within, with a gutter or channel about half an inch in depth; and by cauſing a ſtream of cold water to flow conſtantly through a ſyphon into this gutter, while from the oppoſite ſide it ran off by a ſmall ſpout.

The water was ſeveral degrees colder than the ſubject of the experiment; and keeping cool the portion of the veſſel with which it was in contact, it intercepted the heat that would otherwiſe have travelled by this route to the bottom.

Mercury was tried in glaſs veſſels.

For mercury I had recourſe to veſſels of glaſs.

In all the experiments a thermometer bore teſtimony that the caloric deſcended from the ſurface to the bottom of the fluid, and demonſtrated, at leaſt to my conviction, that fluids can conduct heat.

In all the experiments the heat deſcended

The progreſs of the heat, however, was very ſlow, and atteſted the important fact, for which we ought to be thankful to the Count—That fluids are very bad conductors.

ſlowly.

The ſecond ſet of experiments was calculated to examine, in a very different manner, the poſition, That all interchange and communication of heat between the particles of fluids is impoſſible.

When a hot and a cold fluid are mixed together and well agitated, very ſoon an uniform is produced. This equality muſt proceed either from a communication of heat from the warmer to the colder fluid, agreeably to the common opinion, or from a perfect intermixture of hot and cold particles, according to the notion of Count Rumford. To which cauſe it ought to be attributed, I conceived I might diſcover, by aſcertaining whether, after ſuch an intermixture, any ſeparation of the hot and cold portions took place. If the equilibrium of temperature be owing to intermixture without interchange of caloric, the hotter particles, as ſoon as the agitation ceaſes, ought, by reaſon of their greater rarity, to accumulate, to a certain degree, at the ſurface, and there exhibit a temperature above the common one.

Other experiments of mixing fluids.

I firſt tried water, and mixing this fluid boiling-hot, with an equal quantity nearly ice-cold, in a ſtoppered glaſs jar, I ſhook them well for a ſhort time.

I then noticed the reſulting temperature, and raiſing the ball of the thermometer towards the ſurface, I had an opportunity of obſerving, that it never roſe the ſmalleſt portion of a degree above the common temperature which had been eſtabliſhed.

When hot and cold water are mixed, they acquire a common temperature throughout, and never ſeparate.

I next made a ſimilar experiment with alcohol, ſelecting it on account of its remarkable dilatability. I ſhook well, for half a minute, a mixture of equal parts of alcohol at tempe- 40° and at temperature 170°. The reſulting temperature of the maſs was 140°.

The ſame effect was found with alcohol.

Now, if this was a mixture of particles at 40° and at 170°, as the difference of ſpecific gravity between the fluid at theſe temperatures is very conſiderable, ſome ſeparation of the warmer and lighter particles from the others, ought, I conceive, to have taken place. The temperature of the top, however, never indicated the arrival of warmer particles. It never aſcended above the point of equilibrium.

From theſe experiments I concluded, that the uniformity of temperature was eſtabliſhed by an actual communication and interchange of heat between the particles.

Count Rumford remarked that the mixture might be too complete to allow of ſeparation;

It may not, however, be improper to ſtate, that Count Rumford, with whom ſeveral years ago I had the pleaſure of converſing upon this ſubject, alledged, that the intermixture might be ſo complete as to prevent any ſeparation whatever.

but oil and water do acquire the common temperature by mixture, and exhibit the ſame when ſeparate.

If it be a property eſſential to fluidity, that heat cannot paſs from one particle to another, the particles of different fluids ought ſo be equally incapable of imparting caloric mutually to each other. Unfortunately, however, for the ſpeculation, the the caloric is ſo communicated Though, *à priori*, I entertained no doubts reſpecting the reſult of the experiment, I poured a quantity of olive oil which had been heated by immerſion in a veſſel of boiling water for half an hour, upon an equal volume of water of 38°, and agitated the mixture, by ſhaking for a quarter of a minute. The common temperature produced was 78°, and the heat had gone from the oil into the the water; for when the fluids ſeparated, and had arranged themſelves according to their ſpecific gravity, both of them had the ſame temperature of 78° *.

The experiments of the two deſcriptions now recorded, left on my mind little doubt that the Count had overſtrained his concluſions.

Exp. 4. The tall jar of Exp. 3, containing water at 40°, was cooled round its *lower part* by ice and ſalt.

† *Exp.* IV. I took the ſame tall jar, and ſtopping the tubulature with a cork, I filled it with water of temperature 40°, and placed it in a pan After ſuſpending two thermometers, as in experiment firſt and ſecond, I poured a mixture of ice and ſalt into the pan, to the depth of 4.2 inches, the air of the room being 40°, as in the laſt experiment.

The temperature fell as quickly at top as at bottom:

	Bottom.	Top.	Air.
Eleven o'clock,	40°	40°	40°
In 10 minutes,	38+	38+	

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* This is alſo very ſtrikingly the caſe with mercury and water.—N.

† The text is here reſumed.

	Bottom.	Top.
In 20 minutes,	38°—	38°—
— 30 ———	37—	37—
— 40 ———	36	36
— 60 ———	35.5	35.5
— 80 ———	35	35
- 100 ———	34.5	35
- 120 ———	34—	34
— 8 hours,	34—	34

A cruſt of ice began to form on the inſide of the glaſs when the water in the axis of the bottom and of the top was at 36°. In the courſe of the experiment, it became at leaſt an inch thick.

We learn from this experiment, that cold applied to the lower part of a cylinder of water, nearly 18 inches long, and having the temperature of 40°, is actually as ſpeedily perceived at the ſummit as in the axis of that part, on the external ſurface of which it immediately acts. As fluids conduct heat ſo very tardily, this can only ariſe from currents of cooled water aſcending from the bottom, and theſe cold currents cannot move upwards, were not the water of them ſpecifically lighter than that of the incumbent warmer fluid.

whence it is concluded that the cooled water aſcended from the bottom from its greater rarity below 40°;

The water, therefore, which at the bottom is cooled by the contiguous frigorific mixture, muſt be expanded by the loſs of caloric.

This experiment ſecures full force to the laſt, as it obviates the objection already noticed, and alſo precludes another. I have already ſtated, that it may perhaps be alledged, that the fluid at the top, in experiment third, though cooled to 32° did not deſcend, becauſe below 40°, the contraction is ſo trifling, that it does not occaſion a difference of ſpecific gravity ſufficiently great to cauſe the particles to deſcend, when oppoſed by the inertia and tenacity of the fluid through which they have to fall: or it may be conceived, that the deſcent is ſo tardy, that time is given to the ambient air or ſubjacent fluid to furniſh heat enough to raiſe the temperature of the deſcending ſtream, and by that arreſt it in its downward courſe.

and the objections to Exp. 3. are removed.

But from the particulars above recorded, it is manifeſt, that the change of denſity between the temperature of 32° and 40° is quite ſufficient to put into motion the particles, and to enable them to overcome the obſtacle ariſing from inertia and tenacity, and to withſtand the arreſting effects of atmoſpheric heat.

Though theſe experiments, and ſome others of a ſimilar nature, carried conviction to my mind, and perfectly ſatisfied me reſpecting the reality of the anomaly of water, I determined to vary ſomewhat the mode of making the experiment, ſo as to obtain ſtill more ſtriking reſults.

Another experiment with a ſtill taller jar, 21 inches high.

For the fifth experiment, I uſed an apparatus which conſiſted of a ſtill taller jar. It was 21 inches high, and 4 in diameter. I adjuſted at the middle of its height a perforated baſon of tinned iron, 2 inches in depth, and 10 in diameter. As this baſon embraced the middle of the jar, I could, by filling it with hot water, or a frigorific mixture, apply heat or cold to the middle portion of the fluid in the jar, and thence, by the thermometer, learn what courſe the heated or cooled fluid ſhould take.

Exp. 5. The laſt mentioned jar was filled with ice-cold water. Heat was applied to a zone of two inches near the middle by means of warm water in a circumambient veſſel.

Exp. V. I filled the jar with water at 32°. I placed it upon ſeveral folds of thick carpet, previouſly cooled to the ſame degree. The air of the room going from 33° to 35°, I introduced two thermometers, as in experiments firſt and ſecond. I then poured water of temperature 68° into the baſon, and by means of a ſpout ariſing from the ſide of it, and a ſyphon connected with a reſervoir of water at the temperature now mentioned, I renewed the contents of the baſon frequently, but without cauſing any agitation.

The temperature roſe below to 36° but remained unchanged at top: but after the lower water had attained 39° it became ſtationary, and the temperature at top ſoon roſe to 48.°

	Bottom.	Top.	Air.
At commencement,	32°	32°	33–35
In 10 minutes,	35	32	
— 15 ——	36–	32	
— 20 ——	36+	32	
— 25 ——	37	33	
— 30 ——	38	33*	* From this time I charged the baſon with water of temperature 88°, and renewed it frequently.
— 38 ——	38+	33	
— 45 ——	39–	33	
— 50 ——	39+	41	
— 55 ——	39+	45	
— 60 ——	39+	48	

Nothing can be more deciſive with regard to the queſtion in diſpute, than the particulars of this experiment. Heat is applied to the middle of a column of ice-cold water. The heated portion has an equal ſhare of the column of cold fluid above it and beneath it. There is nothing to determine its courſe in one direction or another, excepting its actual change of denſity.

The thermometer evinces that the warm current ſets downwards, and carries the increaſed temperature to the bottom. There, this inſtrument indicates the ſucceſſive riſe of ſeveral degrees, before the ſurface indicates the ſmalleſt acquiſition of heat.

Whence it is deduced, that a *warm* current of water between 32° and 39°+ deſcended becauſe *denſer* and that when the temperature was more than 39+ the *warm* current aſcended becauſe *rarer*.

The inference is plain, that the cold water is contracted by the heat.

The change of the effect of heat is equally well illuſtrated by this experiment.

No ſooner did the inferior portion attain the temperature of 39°, than the heated fluid altered its courſe, and, by aſcending, carried the increaſe of temperature very rapidly to the ſurface, ſo that it ſoon ſurpaſſed the bottom, and continued to riſe, while the other remained ſtationary.

Exp. VI. I filled the jar uſed in the laſt experiment with water of temperature 39½°, the air and the ſupport being at 39°. Diſpoſing the thermometers in the uſual manner, I introduced a mixture of ſnow and ſalt into the baſon.

Exp. 6. The ſame jar was filled with water at 39½° and a freezing mixture applied to the middle zone. The fluid at bottom was ſcarcely changed, but that at top froze.

	Bottom.	Top.	Air.
At commencement,	39.5	39.5	39°
In 10 minutes, -	39+	38+	
— 25 ———	39+	36.5	
— 35 ———	39	36–	
— 55 ———	39	35	
— an hour and 10 min,	39–	34+	
———— 35 ——	39–	34–	
— 2 hours, -	39–	33+	

. At this time ice began to be formed on the ſide of the veſſel.

This experiment ſpeaks in as decided language as the preceding. It ſhows that when a portion, in the middle of a column of water at temperature 39.5 is cooled, the colder fluid riſes, and does not deſcend through the warmer maſs, and preſents the unequivocal demonſtration, that water of temperature 39½° is actually expanded by loſing heat.

So that the water cooled below 39½ did riſe by expanſion.

The different experiments which I have in detail recorded, agree perfectly with each other in the evidence they give

relative

The general fact is, that heat cauſes ice-cold water to contract to 40°, and afterwards to expand.

relative to the ſubject of inquiry. The general import of them is, that water which is ice-cold, or a few degrees warmer, when heated, becomes ſpecifically heavier,—that water of 40° when heated becomes ſpecifically lighter,—that water above 40°, by the loſs of heat, or by cold, is rendered ſpecifically heavier; and that water below 40° is, by the ſame cauſe, rendered ſpecifically lighter.

Such being the general import, the concluſion is irreſiſtible, that heat, in low temperatures, cauſes water to contract, and at ſuperior temperatures to expand. The opinion, therefore, is founded in truth, that water poſſeſſes a peculiarity of conſtitution in relation to the effects of caloric, and that it is, within a ſhort range of temperature, an exception to the general law of "expanſion by heat."

The greateſt denſity lies between 39½° and 40°.

So far as I can judge from theſe experiments, I am diſpoſed to believe that the point at which the change in the conſtitution of this fluid in relation to heat takes place, lies between 39½°, and the 40th degree.

I am not at preſent aware of any objection to the method I have followed in eſtabliſhing this ſingular anomaly, and in removing any doubts which may have ariſen from the unavoidable influence which the inſtrument muſt have in the mode of conducting the inveſtigation that had previouſly been adopted.

Theſe experiments ſhew the nature of the change, but not its amount. Whether the expanſions and contractions be the ſame at equal intervals from 40°?

The plan of operation above deſcribed, however, only aſcertains the fact; it gives no data for aſcertaining the amount of the anomalous effect of heat.

I have already ſtated, that M. de Luc alledged, that from the temperature of 41°, the expanſion occaſioned by cold was very nearly equal to that produced by the ſame number of degrees of heat; and conſequently that water poſſeſſes the ſame denſity at any given number of degrees of temperature above and below 41°. The firſt experiments of Mr. Dalton appeared to confirm this opinion, and to enlarge the range to which it applied, by extending it to temperatures as far below 32°, as water allows itſelf to be cooled before it begins to freeze. From one circumſtance that conſtantly occurred, I am inclined to think, that the amount of the dilatation by cold is inferior to that cauſed by heat.

During the heating or cooling of water below 40°, the difference of temperature between the top and bottom of the fluid was leſs than what occurred during the cooling or heating of the fluid through the ſame number of degrees above it; and I conceive that, when other circumſtances, but particularly the rate of the change, are alike, the difference of temperature between the upper and lower parts of the fluid, as it depends upon, may prove a meaſure of the difference of denſity.

Alcohol, when heated or cooled, preſents, by reaſon of its greater expanſibility, a greater difference of temperature in theſe ſituations than water; and upon the ſame principle I infer, that water from 40° is more expanded by an equal number of degrees of elevation than of depreſſion.

It is a difficult problem to explain, how theſe contrary changes by heat are effected.

As the concurrence of the teſtimony of the experiments above related with the general opinion, will probably remove every doubt reſpecting the matter of fact, it remains a very difficult problem for thoſe who are fond of philoſophical inveſtigation, to explain how heat ſhall occaſion in the ſame fluid, without producing any alteration of mechanical form or chemical condition, at one time contraction and at another expanſion, and to reconcile the contractive effect to the conceived notions of the mechaniſm of the operations of this energetic agent.

The queſtion ſtated.

When heat cauſes expanſion, it is imagined to act by inducing a repulſion among the particles of bodies, which, oppoſing and overpowering the coheſive attraction, cauſes the particles to recede.

In what manner, then, the addition of heat can occaſion, or allow, the particles of water to approach each other, and how the ſubtraction of it can make them retire to a greater diſtance, I confeſs I can in no meaſure comprehend.

Sir Charles Blagden's explanation; viz.

An explanation, abundantly plauſible at firſt view, very readily ſuggeſts itſelf to every one who is aware of the great and forcible expanſion which happens to this fluid at the moment of its congelation. It it ſtated by Sir Charles Blagden, in the paper already quoted.

As water expands in freezing by virtue of a new arrangement of the particles,

The remarkable dilatation which water experiences at the inſtant of being converted into ice, is very generally aſcribed, and I preſume very properly, to a new arrangement which the particles aſſume, determined probably by their polarity; by which one ſide of the particle A is attractive of one ſide of B, while it is repulſive of another.

Now,

It is probable, that the arrangement and the expanſion may begin before ſolidity enſues,

Now, if this polarity operates with ſo much energy as to impart almoſt irreſiſtible expanſive force at temperature 32°, it is reaſonable to ſuppoſe that it may begin to exert its influence, though in a far inferior degree, at temperatures ſomewhat more elevated. The expanſion, therefore, that takes place, during the fall of temperature from 40°, may be imputed to the particles beginning or affecting to aſſume that new arrangement which their polarity aſſigns them, in which arrangement theſe particles occupy more ſpace than before.

and the contrary.

Again, when heat cauſes water of 32° to contract, upon the ſame principle, it may be conceived to operate, by counteracting the ſmall portion of the diſpoſition to polarity that ſurvives the liquefaction.

I am afraid that we cannot reſt ſatisfied with this explanation. We muſt not be deceived by the plauſibility of it.

The ſtate of perfect fluidity depends upon the circumſtance, that the particles of any body admit of ready motion upon each other, and that the change of relative ſituation meets with little or no ſenſible reſiſtance.

Objection. This advance towards congelation, ought to impair the fluidity:

Water certainly poſſeſſes fluidity in a great degree, and its particles muſt of courſe encounter but little reſiſtance, as they glide the one upon the other. But if theſe particles ſhall begin to exert any degree of polarity, by which certain faces become more diſpoſed to attach to each other than certain others, this tendency would neceſſarily oppoſe that indifference with regard to poſition, which is eſſential to fluidity, and of courſe muſt impair the fluidity, and induce ſome degree of tenacity or viſcidity.

which does not appear to be the caſe.

To appearance, however, water at 32° has its fluidity as perfect as at temperatures conſiderably elevated. Unwilling to truſt to appearance, where experiment might decide, I have attempted in various ways to aſcertain whether the water ſuffers any ſenſible diminution in this reſpect while it is expanded by cold. The following method I deem the moſt correct.

Experiments with Nicholſon's gravimeter.

For the purpoſe, I employed a gravimeter, the one contrived by Mr. Nicholſon for diſcovering the weight and ſpecific gravity of ſolids.

This is a convenient inſtrument, but, unfortunately, it is by no means ſo tickliſh as a balance. Duly loaded, ſo as to be equiponderant with the water in which it is plunged, Mr.

Nicholſon ſays, it is ſenſible to the 20th part of a grain*. The one I have, though its ſtem be ſlender, is ſcarcely ſenſible to leſs than two or three twentieths of a grain.

The want of ſenſibility in the gravimeter ariſes, in a great meaſure, though not entirely, from a certain degree of tenacity ſubſiſting among the particles of the fluid; and any thing that tends to increaſe this tenacity, muſt, in the ſame proportion, augment this want of ſenſibility.

It roſe and fell in warm, and

To aſcertain whether any ſenſible change in the tenacity or fluidity accompanies the expanſion of water by cold, which the theory requires, I examined the mobility of the inſtrument when immerſed in water at different temperatures. I firſt plunged it into this fluid, heated to between 60° and 70°. Under due loading, which ſunk it to the mark on the ſtem, it was not ſenſible to a weight leſs than two or three twentieths of a grain.

in ice-coldwater with equal facility.

I then tried it in ice-cold water, and found that its ſenſibility was in no perceptible degree impaired. The coldneſs of the water, it muſt be remembered, cauſes ſome degree of contraction of the gravimeter. This contraction cannot fail to render the inſtrument in ſome ſmall meaſure more ſenſible, and, ſo far as it goes, to counteract the ſluggiſhneſs produced by any increaſed tenacity in the fluid.

Whence the author concludes that the fluidity is not ſenſibly changed.

But as the body of the inſtrument is made of glaſs, the amount of the contraction muſt be very ſmall, and the change of ſenſibility ariſing from it ſo very trifling, as certainly by no means to obſcure ſuch an effect as an increaſe of tenacity would occaſion. I therefore with ſome confidence conclude, that the fluidity of the water is not ſenſibly diminiſhed, and conſequently that the polarity has not begun to exert any ſenſible influence; it can ſcarcely, therefore, be accounted the cauſe of the dilatation.

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* Perhaps the difference of ſenſibility in my inſtrument, and that of the learned Profeſſor, may have ariſen from a difference of the diameters of the ſtems. Mine was of one-fortieth of an inch. It was well rubbed with a clean linen cloth, which rendered the ſurface equally diſpoſed either to deſcend or aſcend; and the inſtrument was not judged to be in equilibrio with the fluid, except when the ſurface about the ſtem was neither prominent nor depreſſed. This was eaſily known by the reflected image of the window frame, or other objects being ſeen cloſe to the ſtem without diſtortion.—N.

Annotation.—W. N.

Sir Charles Blagden's theory of expanſion of water by heat does not ſuppoſe that it ſhould be leſs fluid.

It does not ſeem to me that Sir Charles Blagden's explanation does neceſſarily imply that the fluidity of the maſs taken as a whole, ſhould be ſenſibly impaired when tried by the application of a mechanical teſt. It might be impaired in the ſame manner as the water is affected by mixing ſmall floating fragments of a ſolid along with it. When a ſaline ſolution which would become ſolid by cold, ſuch for example as the ſulphate of ſoda, is cooled below its point of congelation, the cryſtals will be differently formed according to circumſtances.

Inſtance of cryſtallization differing according to circumſtances.

If the fluid be gently ſhaken or made to oſcillate, a ſhower of minute cryſtals will gradually fall through the fluid; and the whole maſs will be a conſiderable time before the cryſtallization is finiſhed; but if, inſtead of this method of agitation, the glaſs be ſcratched by a quill underneath the fluid, in Sir Charles Blagden's way, or if a ſmall inſtrument, having a cryſtal of the ſalt adhering to it, be dipped into the ſolution, the cryſtals will radiate with great rapidity from that centre of perturbation, and in a few ſeconds the whole of the ſolution will become rigid.

Whence it is conjectured that the expanſion of water by cold may ariſe from minute cryſtals of ice in the fluid.

This common and very ſtriking experiment of chemical lecturers, ſeems to me to indicate at leaſt a poſſibility that ſmall cryſtals of ice may be formed and float diſtinctly from each other in water, at 40 degrees and lower: and I think the metals afford us a number of inſtances in which a conſiderable interval of temperature is found to be between the commencement of cryſtallization and the ſolidification of the whole maſs. In pewterers ſolder the interval is not leſs than 40 degrees. This hypotheſis of ſuch diſſeminated particles of ice, which ſeems to be nothing more than an expreſſion of Sir Charles Blagden's theory in different words, will explain why the colder water ſhould be lighter;—namely, becauſe it muſt contain more ice, and alſo why the expanſion ought not to begin but at ſome definite temperature.

A meaſure of the greater or leſs fluidity of bodies is very deſirable.

Though it does not appear to me that the theory of Sir Charles muſt neceſſarily imply a change in the mechanical reſiſtance of water from what may be called rigidity; yet there are many other reaſons why philoſophers ſhould be deſirous of meaſuring the variations of fluidity in bodies: that is to ſay, the greater or leſs facility with which their parts are moved amongſt each other. The ingenious attempt of Dr. Hope to aſcertain this from the reſiſtance made by a fluid to the perforation

Dr. Hope's trial may be modified probably by a change in the attraction or repulſion between the water and the ſtem of the gravimeter.

ration of its ſurface by a cylindrical ſolid, is liable to the objection that it ſuppoſes the attraction or repulſion between the ſolid and the fluid to remain unchanged by variations of temperature; whereas the contrary ſeems moſt probable. The doctor's experiment muſt be grounded upon a poſition that the greater the depreſſion or the greater the elevation of a fluid round a ſmall cylinder partly immerſed in it, the greater muſt be the reſiſtance from imperfect fluidity. But theſe effects are evidently as much governed by the attraction or repulſion of the ſolid with regard to the fluid as by the reſiſtance which the experiments are intended to meaſure. I have ſomewhere read that water clocks and other inſtruments for meaſuring time, by the paſſage of water through ſmall holes, go ſlower in cold weather. This may ariſe from contraction of the hole, though my author aſcribes it to imperfect fluidity. After ſome meditation on this problem it ſtill appears to me to be ſurrounded with difficulties. Perhaps it may be one of the beſt methods to ſuffer the fluid to drop from a capillary ſyphon in different temperatures. I am diſpoſed to think that the drops would be ſmalleſt and the whole quantity in a given time greateſt when the fluidity was the moſt perfect, or at leaſt when the adheſion of the particles of the fluid to each other was the leaſt. But even here the attraction of the ſmall capillary extremity of the tube from which the drop would fall would require to be conſidered; and on this account the method would be preferable (if ſo) to Dr. Hope's only becauſe the repetition of a great number of drops or quantity of effluent water would give a greater degree of preciſion to the reſult.

Water clocks are ſaid to go ſlower in cold weather; becauſe the water is leſs fluid.

Suppoſition that moſt water would drop from a capillary tube when it was moſt fluid.

Is it likely that the rope pump turned regularly a certain number of turns in a given time would raiſe more water when coldeſt and leaſt fluid? If it did not might we not infer that the fluidity of water is not ſenſibly affected by change of temperature?

Will the rope pump ſhew a difference in the tenacity or fluidity of water hot or cold?

IX.

Obſervations on Turf, from the German Rathbeger fur alle Stænd. *By* Doctor Collenbusch.

It is not very probable that a man placed beſide a fountain of pure water ſhould ſuffer himſelf to die of thirſt through neglect of uſing it, or poſſeſſing food in abundance, ſhould not appeaſe his hunger with it; nevertheleſs inſtances of this kind are not wanting.

Wood fuel very ſcarce in Germany. Other matter may be ſubſtituted for it.

Every one complains in Germany of the ſcarcity of wood for fuel. It is known that ſubſtances have been found in other places which can ſupply its place, and that they have been formerly uſed here; but all this cannot induce any one to ſearch for *turf*.

Ungrounded prejudices prevent the uſe of turf for fuel.

It is eaſily conceived that proprietors of woods, through the fear of having their profits diminiſhed, ſhould endeavour to perpetuate ancient prejudices, and to extend the opinion that the plague only ceaſed its ravages ſince the uſe of *turf* for fuel has been diſcontinued; but it is difficult to imagine that magiſtrates inſtead of encouraging the preparation of this fuel, ſhould endeavour to prevent thoſe from doing ſo, who wiſhed to engage in it.

Uſed in Germany from the moſt remote periods.

It is very likely that the diſcovery of the uſe of *turf* as a combuſtible was firſt due to chance; and beſides the uſe of this fuel in Germany has been continued from periods more remote than any written documents extend to.

Various erroneous opinions formed of its production and uſe.

The principal cauſes which have prevented the ſearch after *turf*, are the erroneous opinions which have been formed of the manner in which it has been produced, of its preparation, and its uſe; ſome of which are as follow.

1ſt That turf is found in veins like metals.

Some think, for example, that *turf* has been formed at the moment of the creation, ſuch as it is now found in the earth, and that there are veins of turf, as there are of iron, copper, tin, and other metals; but experience proves the falſity of this opinion, for there is found in almoſt all parts of Germany turf covered with more or leſs earth, (if only a proper ſearch be made for it) beneath which layers of trees may be ſeen, which proves that there formerly were foreſts in the ſame places.

Others

2d That it was formed at the deluge.

Others believe that at the time of the deluge vaſt foreſts were overthrown, and afterwards covered with herbs, reeds, and other plants, and that theſe vegetables having rotted by degrees, became at laſt this black combuſtible maſs reſembling earth, which muſt have required an enormous quantity of vegetables, as plains of many leagues ſquare are found covered with beds of it to the depth of more than 25 feet, beneath which trees are diſcovered of great hardneſs, and almoſt petrified.

3d That it was tranſported by the ſea from the weſt.

Others imagine that it is more probable that the ſea tranſported the materials of the turf from the weſtern countries to the eaſtern, and covered with them the trees which are found buried beneath the turf. It is very true, that theſe trees have their roots turned towards the weſt, and their heads to the eaſt. But then it is difficult to explain how this ſubſtance could be carried to countries diſtant from the ſea, and even to the tops of the higheſt mountains in upper Saxony, on the *Brocken* and the *Alps*.

4th That it was waſhed down [illegible] torrents.

Many perſons are of opinion that torrents and rivers have drawn together and depoſited leaves and branches of trees on the low grounds, and that they have thus accumulated the conſtituent elements of the turf; but this cannot take place in countries in which no large rivers are found, nor on high mountains. The microſcope clearly ſhews that turf, eſpecially that kind which is from the ſurface of the earth, is compoſed of moſſes, herbs, ruſhes, and other vegetables, and their roots ſtrongly interlaced, of which the greateſt part is changed into earth.

The microſcope ſhews it compoſed of vegetable fibres.

Paper made from it and leather tanned by its water.

Paper has actually been compoſed from turf, and the water which has ſettled in turbaries is uſed to tan leather, which proves that it is principally compoſed of vegetables. Chemical reſearches have alſo diſcovered in it a mineral reſin which principally promotes its combuſtibility. It appertains then partly to the vegetable, and partly to the mineral kingdom.

Turf produced artificially by ſinking deep and wide trenches, which fill up by vegetation.

Turf may be produced artificially, by digging trenches 6 feet deep, and from 15 to 20 feet ſquare; the trenches become filled with water, and produce the firſt year a green ſlimy moſs, the ſecond year this moſſy vegetation covers the water to the height of two feet, and a great quantity of filaments are diſcovered in it mixed with leaves and flowers, in the third year a ſtratum is eſtabliſhed, which attracts the duſt and the ſeeds which float in the air, and engender a quantity of marſh plants, of reeds, and of herbs, which the fourth year become ſo heavy that they fall to the bottom. They then become compreſſed there, and by ſucceſſive repetitions of this operation, all the trench becomes filled up in the courſe of 30 years; however this turf would probably require 100 years before it would equal the ancient turf.

Three ſpecies of it.

1ſt The ſurface turf.

Found wherever water ſtagnates and is covered with weeds,

and where trees covered with moſs are half up-rooted by the wind.

Although this turf is always the ſame in its conſtituent parts, it nevertheleſs differs in having theſe parts variouſly mixed, which occaſions its being divided into three ſpecies. The firſt comprehends the *ſurface turf*, and is the moſt common kind; it is found almoſt every where; but it contains in ſome places more combuſtible matter, which makes its colour vary. This ſpecies is always ſure to be found wherever places are diſcovered where the water ſtagnates, whether on plains, elevations, or declivities, in ſuch a manner as to form a thick blueiſh cruſt, and depoſits a yellow mud; or where the ſoil is covered with moſs, reeds, ruſhes, or ridges; and if at the ſame time the feet of the paſſenger ſinks into the looſe ſoil, if the earth bends beneath his feet; if trees are perceived (which are commonly little pines or fir trees, or ſometimes other kinds of trees,) covered with much moſs, inclined to one ſide, and half rooted up at the other by the wind; in all theſe places turf will be found near the ſurface, and it is only neceſſary to remove the ſod to perceive it. But this operation may be performed more quickly and eaſily with the Engliſh borer, which alſo will ſhew the depth of the bed.

To procure it the water ſhould be drained off.

Eaſy method of finding the deſcent for the drains.

To procure the turf, the water ſhould be drained off, which is eaſy to do if the country is elevated or has valleys in its vicinity; but the operation is more difficult when the earth is level. As perſons are not always to be found capable of taking the levels of ground, the places ſhould be remarked where the water ſettled in ſpring when the ſnow melts; theſe places ſhould be marked by ſtakes, and afterwards the trenches ſhould be made to paſs this way which are to be dug, to let the water run off.

Method of preparing the ſurface turf.

To cut the turf an iron ſpade is uſed, which ſhould be neither round nor pointed, but terminating in a ſtraight line; this ſhould be ſcrewed down as far forwards as poſſible, along the ſide of a ſtretched cord, by a line 14 inches long and ſix broad; the detached part is ſeparated from the depth of three inches, at two ſtrokes of the ſpade, to the length of 16 inches, and 4½ inches broad, and this piece of turf is afterward divided in two.

Method of drying it.

In order that the pieces of turf may dry quickly, they ſhould be placed on planks, and diſpoſed ſo that the air might freely circulate between them, and that they could receive the rays of the ſun.

Should not be dried too much in the ſun,

When the turf is thus dried to a certain degree, it is placed under ſheds to compleat the drying; for if it was expoſed to the ſun till it was entirely dry, it would loſe its ſtrength and burn like ſtraw.

or kept too long.
Upper and lower beds of it the worſt.
Beſt kind dark brown and heavy.
Bright brown or reddiſh worſe.

It is alſo diſadvantageous to cut a large proviſion of it for many years, for the laſt made is always the beſt. The upper and lower beds are alſo obſerved to be of inferior quality to thoſe in the midſt; the beſt turf is that of a brown colour inclining to black, is heavy, and its texture is traverſed by a ſmall quantity of roots; this kind produces a ſtrong and laſting fire, and its ſmell is very ſupportable. The more it is of a bright brown colour, the greater number of roots in it, and the lighter it is, the worſe is its quality. This ſort conſumes more ſpeedily, and may ſerve to advantage where a quick fire is wanted; its odour, it is true, is very diſagreeable, but its aſhes are excellent.

Grey or yellow ſort the worſt.

The turf which inclines to a grey or yellow colour, and which is mixed with reed, is always the worſt ſort, but always good enough to heat kilns or ovens, and its aſhes are good; this ſpecies is ſeldom found below the depth of two ells; it is reproduced after ſeveral years.

Second ſpecies, the crumbling turf.

The ſecond ſpecies of turf is the *crumbling turf (moder-torf)*, this kind is found more abundantly in Holland; its cutting and preparation require much more pains than the ſurface turf.

Third ſpecies, the mountain turf.

The third ſpecies, or the *mountain turf*, is dug up from pits and galleries, and is reduced to regular forms like the preceding kind.

Wood not proper for fuel in all caſes more than turf.

It is objected to the uſe of turf that it cannot be employed as a ſubſtitute for wood in all the places where wood is burned; but it ſhould not be forgotten that wood itſelf is not fit for every work where fire is required; that in order to be employed in founderies it muſt be charred with much trouble, and with a loſs of two thirds of its weight, and that wood as well as turf is of different qualities and produces different effects.

Turf may be charred.

Turf is alſo ſuſceptible of amelioration, eſpecially the ſurface turf, the crumbling turf, and the mountain turf likewiſe; for it may be reduced to charcoal, and will thus ſerve for every work which requires fire; and in this caſe it yields neither ſmell nor ſmoke. The more ſtrongly the turf is compreſſed before its carbonization, the more excellent is the charcoal.

Advantages from the uſe of turf.

Cabbages, &c. may be planted where it has been extracted.

Its uſe admits of having more manufactories where fire is uſed.

To compenſate for the inferiority of turf to wood, granting that it is inferior, its uſe will prevent the great price which will otherwiſe neceſſarily be paid hereafter for timber for building, and will admit of the woodlands being proportionally reduced; the places alſo where the ſurface turf has been dug up, if it has not been from too great a depth, may ſerve for ſituations wherein to plant cabbages, beets, and madder, or they will ſerve for fiſh ponds. The uſe of turf will admit of the multiplication of manufactories which uſe fire, of mines and forges; aged perſons and children may be employed in preparing it; its aſhes form a good manure, and the mould which falls from it may eaſily be converted into aſhes.

X.

Experiments on the remarkable Effects which take place in the Gaſes, by Change in their Habitudes, or elective Attractions, when mechanically compreſſed. By THOMAS NORTHMORE, *Eſq. In a Letter from the Author.*

To Mr. NICHOLSON.

Devonſhire Street, Portland Place,
Dec. 17, 1803.

SIR,

The author's reaſon for early publication.

IT was my intention to have poſtponed troubling you with the following experiments upon the condenſation of the gaſes, until I had brought them to a greater degree of perfection; but being informed that ſeveral of them have already, by means of which I am ignorant, and probably in a mutilated ſtate, found their way to the preſs, any further delay ſeems improper. If then you deem the preſent communication worthy a place in your intereſting Journal, it is entirely at your ſervice.

His fuſpicion that the affinities of the gaſes would be changed by compreſſion.

It had long ago occurred to me, that the various affinities which take place among the gaſes under the common preſſure of the atmoſphere, would undergo conſiderable alteration by the influence of condenſation; and the ſucceſs attending the *violent* method adopted by the French chemiſts, which violence did not appear to me requiſite, afforded additional encouragement to my undertaking ſome experiments upon the ſubject.

I communicated this to the late chemical operator in the Royal Inſtitution, a gentleman eminently converſant in the ſcience, and with whom I was then engaged in a ſeries of experiments: he not only approved of my deſign, but ſeemed to think it not improbable that an extenſive field might thus be opened to future diſcoveries. Whether theſe opinions are juſtly founded, is now left for you, Sir, and the public to judge.

Difficulties of the undertaking as to the inſtruments.

In entering upon a field entirely new, obſtacles were of courſe to be expected: nor without reaſon; for though I had applied to one of the moſt eminent philoſophical inſtrument-makers in London, Mr. Cuthbertſon, yet I began to fear, even at the outſet, that his ſkill would be ſet at defiance.

Condenſing-pump.

Receivers.

Various objections.

The firſt inſtruments which he made for the preſent purpoſe were, a braſs condenſing-pump, with a lateral ſpring for the admiſſion of the gas by means of ſtop-cock and bladder; two pear-ſhaped receivers, one of metal of the capacity of ſeven cubic inches, and another of glaſs of about three and a half: theſe were connected by a braſs ſtop-cock, having a ſcrew at each end. The metallic receiver was ſoon found to be of little or no utility, as well on account of its liability to be acted upon by the generated acids; its being too capacious, and thus conſuming too large a quantity of gas: as becauſe, though the reſult of an experiment might thus be known, yet the changes which the ſubjects might undergo would neceſſarily eſcape obſervation. The glaſs receiver obviated all theſe difficulties, and one or two imperfect experiments were performed with it; but the ſtop-cock ſpeedily failed in its effect. For the power of the compreſſed gaſes was ſo great, partly from their elaſticity, and partly (where affinities had operated) from their corroſive quality, as abſolutely to wear a channel in the metal of which the plug was made, and thus to effect their eſcape. But not to trouble you any farther with the obſtacles that occurred, and which are mentioned only to prevent unneceſſary expence to others; I have at laſt, by Mr. Cuthbertſon's aſſiſtance, procured a connecting-tube, to which a ſpring-valve is adapted that has hitherto anſwered every purpoſe. *See Plate XIV. Fig.* 2, 3, 4, 5, 6.

Inſtruments now uſed by the author.

Pump.

The inſtruments which I now uſe, are, 1ſt. An exhauſting ſyringe; 2d. A condenſing-pump, with two lateral ſprings for different gaſes; 3d. The connecting ſpring-valve; and laſtly, glaſs receivers, which ſhould have been of various ſizes, but the one mentioned above having burſt, that which I have principally uſed in the following experiments, is of about five cubic inches and a quarter in capacity, and made of glaſs well annealed and a quarter of an inch in thickneſs. Beſides theſe inſtruments, I have occaſionally applied Mr. Cuthbertſon's double ſyphon-gage (*See Fig.* 6), by which the number of atmoſpheres condenſed in the receiver, or rather the elaſtic power of the gaſes, may be meaſured; but this is rendered of leſs ſervice, becauſe a ſtop-cock muſt then be placed between the receiver and ſpring-valve, which frequently impairs the whole experiment; and alſo becauſe, after a certain degree of condenſation, and more particularly upon the admixture of the gaſes, new affinities uſually take place, which tend to diminiſh the elaſticity: the greateſt number of atmoſpheres my gage has yet meaſured, is eighteen. Theſe, Sir, with ſome bladders and ſtop-cocks, various iron ſcrew-keys, and a wooden guard for the legs in caſe of burſting, conſtitute the principal part of the requiſite apparatus.

Glaſs receiver.

Syphon gage.

Eighteen atmoſpheres compreſſion.

I now proceed to the experiments, premiſing that the firſt four were made with the imperfect apparatus, when the gas was continually making its eſcape through the ſtop-cock.

Experiment I.

Exp. 1. Hidrogen, oxigen, and nitrogen gave water, and probably nitrous acid.

Into the glaſs receiver, of three cubic inches and a half capacity, were compreſſed in the following order: Hidrogen, two (wine) pints; oxigen, two pints; nitrogen, two pints.* The reſult was, water which bedewed the inſide of the receiver; white floating vapours (probably the gaſeous oxide of nitrogen); and an acid which reddened litmus paper. Mr. Accum was preſent at this experiment, and from his opinion, as well as from ſucceeding experiments, I have reaſon to think that this acid is the nitric.

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* Theſe gaſes therefore occupied about five times the capacity they were condenſed into.—N.

Experiment II.

Exp. 2. The ſame, but the oxigen firſt.

As a difference of arrangement in the order of the gaſes tends conſiderably to vary the reſult, I repeated the former experiment (having firſt poured a little lime-water into the receiver) by injecting firſt the oxigen, about three pints, then equal quantities of hidrogen and nitrogen. Much of this gas eſcaped, owing to the imperfection of the inſtrument; but upon the affuſion of the nitrogen, the white vapours again appeared in the receiver; water ſeemed likewiſe to be formed; and ſome yellow particles were ſeen floating upon the lime-water. Theſe particles probably aroſe from the reſinous ſubſtance, uſed in faſtening on the cap of the receiver, being diſſolved by the nitrous gas formed during condenſation.

I would juſt obſerve, that the magnet *ſeemed* to be affected during this experiment; but as there is iron uſed in the machine, this may be otherwiſe accounted for.

Experiment III.

Exp. 3. Carbonic gas and hidrogen. Water and a changed gas.

Two pints of carbonic acid, and two of hidrogen, were ſubjected to condenſation. The reſult was, a watery vapour, and a gas of rather offenſive ſmell.

Experiment IV.

Exp. 4. Phoſphorus in condenſed air.

Trying to inflame phoſphorus by the condenſation of atmoſpheric air, the bottom of the machine (where it had been repaired) burſt out with an exploſion. This happened when I had immerſed the apparatus in water to diſcover where the air eſcaped. The receiver was full of the fumes of the phoſphorus, which was itſelf diſperſed in the veſſel of water. I afterwards repeated this experiment with the more perfect apparatus, but I could not inflame the phoſphorus, and the fumes which aroſe at firſt ſoon diſappeared. There was juſt enough acid (probably phoſphoric) formed on the inſide of the receiver to tinge litmus.

Experiment V.

Exp. 5. Repetition of Exp. 1. with better apparatus.

Having now the ſpring-valve, and new receiver of five cubic inches and a half capacity *, I poured in two ſcruples of ſolution of potaſh, and then injected two pints of hidrogen, two of nitrogen, and three of oxigen. This quantity was hardly ſufficient for the capacity of the receiver, and the reſult was only a ſmell of the gaſeous oxide of nitrogen, a few yellowiſh fumes, and ſcarce enough acidity to tinge the edge of the teſt paper: of courſe, I could not effect the formation of nitrate of potaſh.

Experiment VI.

Exp. 6. Nitrogen (firſt) and then hidrogen and oxigen.

I now determined to begin with the nitrogen, which always appeared to me to undergo the moſt important chemical changes, and therefore injected two pints of nitrogen, three of oxigen, and two of hidrogen. Upon the condenſation of the nitrogen, it ſpeedily aſſumed an orange-red colour, which upon the acceſſion of the oxigen, gradually diminiſhed, and at length diſappeared, though at firſt it ſeemed rather deeper. A moiſt vapour, coating the inſide of the receiver, aroſe upon the compreſſion of the hydrogen, which moiſture was ſtrongly acid to the taſte, coloured litmus, and, when very much diluted with water, acted upon ſilver.

Experiment. VII.

Exp. 7. The ſame, but different arrangement.

Nearly the ſame as the laſt, but with different arrangement. The nitrogen, three pints and a half, was firſt introduced; then the oxigen, two pints; and next the oxigen, three and a half. The nitrogen formed the orange-red colour as before; the hidrogen produced white clouds at firſt (*quære* ammonia?) which afterwards diſappeared, and the orange-red colour became lighter; but upon the affuſion of the oxigen, the colour did not diſappear as in the laſt experiment, but, if any thing, became darker. I then injected two pints more of hidrogen, but this had little or no effect upon the colour. Some vapour was generated, which was, as uſual, ſtrongly acid.

Experiment VIII.

Exp. 8. Nitrogen over limewater.

Previous to the burſting of the ſmall receiver, I had put in it a ſcruple of lime, and condenſed upon it three pints o nitrogen. The reſult was, a little reddiſh colour at firſt, which ſoon vaniſhed. Upon repeating this experiment in the large receiver, I could produce no colour at all. In my preſent ſtate of knowledge I am unable to account for this circumſtance; but as ſoon as I get my new receivers of a ſmaller capacity, I mean to repeat the experiment.

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* One fifth part of a pint very nearly.—N.

Beſides the above, I have made various other experiments with different gaſes, but I think it right to repeat them with greater accuracy before I ſubmit them to the eye of the public: if upon that repetition they appear to me to be attended with reſults of ſufficient importance to occupy a place in your Journal, I will take the liberty of communicating them to you, and am, Sir,

Your moſt obedient ſervant,

THO. NORTHMORE.

P. S. I think it neceſſary to add, that during the courſe of the above-mentioned experiments, there was a great variation of temperature in the atmoſphere, from the heat of 70 degrees of Fahrenheit to the cold of 33.

Explanation of the Figures, by Mr. Cuthbertſon.

Deſcription of a valve for condenſing.

Fig. 2, 3, 4, 5. Plate XIV. repreſents ſections of the ſeveral parts of the ſpring-valve for the condenſing ſyringe; *a* is a female ſcrew, intended to receive the male at the end of the ſyringe; *b* is a ſquare, to which is a key to ſcrew it perfectly tight to the ſyringe: *d,* Fig. 3. is a female ſcrew fitted to the male *c*; *c* is a male ſcrew fitted to the female of the glaſs receiver; Fig. 4. is a round ſteel arbor, turned with a conical part and flat ſhoulder at *a*; *a b* is a ſpiral ſpring; Fig. 5. is a hollow braſs cylinder ſerving as a cover and guide to Fig. 4; the piece Fig. 2. has a ſmall hole drilled through the center, and turned out at the end *c*, ſo as to fill the cone *a*, Fig. 4; Fig. 3. is turned out at *f* ſo wide as to receive Fig. 5.

If the plane ſhank of 4 be put into the hole *a c* till its cone ſhut cloſe into the hollow cone at *c*, Fig. 2. and the other, end with the ſpiral, covered by Fig 5. ſcrewed tight upon the flat end of *c*, and *d* be ſcrewed to *c*, all the joints being properly ſupplied with oil, and leathers, it is fitted for uſe.

Fig. 6. Repreſents a ſection of the condenſing or double ſyphon gage, being a glaſs tube bent into the form of the

figures, the end a is mounted with a braſs ſcrew, having a hole through it correſponding with the inſide of the tube, the leg $b\ c$ is filled with mercury, and d is hermetically ſealed: $d\ c$ is divided into atmoſpheres.

XI.

Account of a Graphometer for meaſuring the Angles of Cryſtals. In a Letter from Mr. ROBERT BANCKS, *No.* 411, *Strand.*

To Mr. NICHOLSON.

SIR,

Great advantage of diſtinguiſhing minerals by their figure.

I NEED not point out to you, and to the learned readers of your Journal, how great the advantage will be, whenever the ſame may be realized, of diſtinguiſhing ſubjects of the mineral kingdom by their external appearance. This has long been done, with conſiderable preciſion, by operative men who have acquired their ſkill from continued practice, but without being able to communicate the knowledge they poſſeſs by any ſimple indications, ſuch as might be given in writing, or through the medium of the preſs. Neither need I on this occaſion point out how much we are indebted to the labours of Bergman, Romé de l'Iſle, and above all, Haüy, for ſcientific inveſtigations of the forms of cryſtals, which at preſent bid fair to afford us criterions of the moſt extenſive uſe. My preſent object is to communicate what I hope will be thought one ſtep, however ſmall, towards facilitating the admeaſurement of their angles. In your firſt vol. at page 132, you have given an account of the graphometer of Carangeau, which is now conſiderably known and eſteemed. I have rendered that inſtrument ſomewhat cheaper, and eaſier in the execution, and more correct in its uſe. For the ſake of thoſe who may not have that volume at hand, I ſhall briefly ſtate, that the inſtrument conſiſts of a ſemi-circle, like that which I am about to deſcribe, and a pair of compaſſes or legs having their centre in the centre of the ſemi-circle, but capable of having their points drawn back, ſo as to admit of their application to any ſmall cryſtals. The arc of the ſemi-circle is divided

Cryſtallography.

Carangeau's graphometer for cryſtals.

divided into two quadrants by an hinge, ſo that one part may be turned back out of the way of any mineral, which may require to be brought up towards the centre for admeaſurement; and the ſame arc can afterwards be reſtored to its place, in order to ſhew the degree and fraction of the angle.

In my improved inſtrument I avoid this joint, and obtain a much firmer framing by making my arc in the form of a protractor, as in Fig. 1. Plate XV. having an hollow centre at A, and a ſtud at B, both lying in the direction of that diameter which terminates the graduations. The compaſſes, or radii, or legs, are ſhewn in Fig. 2. ſeparate from the arc. Their centre C is made like thoſe of the common proportional compaſſes, and admits of the legs C D, C F being conſiderably lengthened or ſhortened when the two pieces are applied to each other. D E the fixed leg is repreſented as beneath F G the moveable leg or radius, and the lower end of the centre pin is made to fit the hole A preciſely, at the ſame time that the ſtud at B being admitted into the long perforation towards E, the piece D E becomes ſteadily attached to the ſemi-circle, as is ſeen in Fig. 3.

Improved graphometer. The ſemi-circle is entire, and the compaſſes or callipers are uſed ſeparately for meaſuring the cryſtal, and applied to the ſemicircle for reading off.

The uſe is obvious. The cryſtal muſt be meaſured by the detached compaſſes as in Fig. 2, which are much more handy for all deſcriptions of minerals than Carangeau's entire inſtrument; and when thus ſet, if fig. 2 be applied to fig 1, as before directed, the angle will be read of at the fiducial edge of G.

I hope you and your readers will conſider this as the uſeful ſimplification of a valuable inſtrument, and ſhall be happy to receive your ſanction by its appearing in a work ſo generally known and eſteemed as your Journal.

I am, Sir,

Your obedient Servant,

ROBERT BANCKS.

Nov. 1, 1805.

Accounts

ACCOUNT OF NEW BOOKS, &c.

Philoſophical Tranſactions of the Royal Society of London for 1805. Part II. Quarto 353 pages, with an Index, and Six Plates. Nicoll.

Philoſophical Tranſactions of the Royal Society.

THIS part contains the following communications, 1. Abſtract of Obſervations on a Diurnal Variation of the Barometer between the Tropics. By J. Horſburgh, Eſq. 2. Concerning the Difference in the Magnetic Needle, on board the Inveſtigator, ariſing from an Alteration in the Direction of the Ship's Head. By Matthew Flinders, Eſq. Commander of his Majeſty's Ship, Inveſtigator. 3. The Phyſiology of the Stapes, one of the Bones of the Organ of Hearing; deduced from a comparative View of its Structure, and Uſes in different Animals. By Anthony Carliſle, Eſq. F. R. S. 4. On an Artificial Subſtance which poſſeſſes the principal Characteriſtic Properties of Tannin. By Charles Hatchett, Eſq. F. R. S. 5. The Caſe of a full grown Woman in whom the Ovaria were deficient. By Mr. Charles Pears, F. L. S. 6. A Deſcription of Mal-formation in the Heart of an Infant. By Mr. Hugh Chudleigh Standart. 7. On a Method of analyzing Stones containing fixed Alkali, by Means of the Boracic Acid. By Humphry Davy, Eſq. F. R. S. 8. On the Direction and Velocity of the Motion of the Sun and Solar Syſtem. By William Herſchel, L. L. D. F. R. S. 9. On the reproduction of Buds. By Thomas Andrew Knight, Eſq. F. R. S. 10. Some Account of Two Mummies of the Egyptian Ibis, one of which was in a remarkable perfect State. By John Pearſon, Eſq. F. R. S. 11. Obſervations on the ſingular Figure of the Planet Saturn. By William Herſchel, L. L. D. F. R. S. 12. On the Magnetic Attraction of Oxides of Iron. By Timothy Lane, Eſq. F. R. S. 13. Additional Experiments and Remarks on an Artificial Subſtance, which poſſeſſes the principal Characteriſtic Properties of Tannin. By Charles Hatchett, Eſq. F. R. S. 14. On the Diſcovery of Palladium, with Obſervations on other Subſtances found with Platina. By William Hyde Wollaſton, M. D. F. R. S. 15. Experiments on a Mineral Subſtance, formerly ſuppoſed to be Zeolite, with ſome Remarks on Two Species of Uran-glimmer. By the Rev. William Gregor.

Improved Graphometer by Mr. Banks.

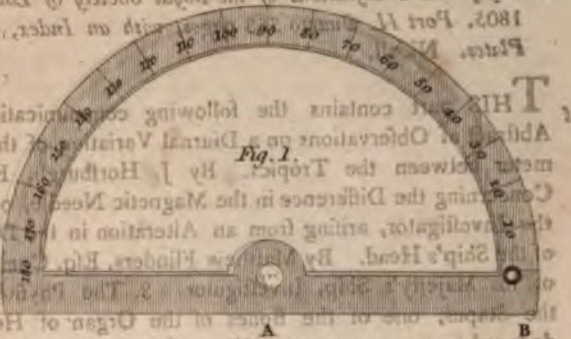


Fig. 1.

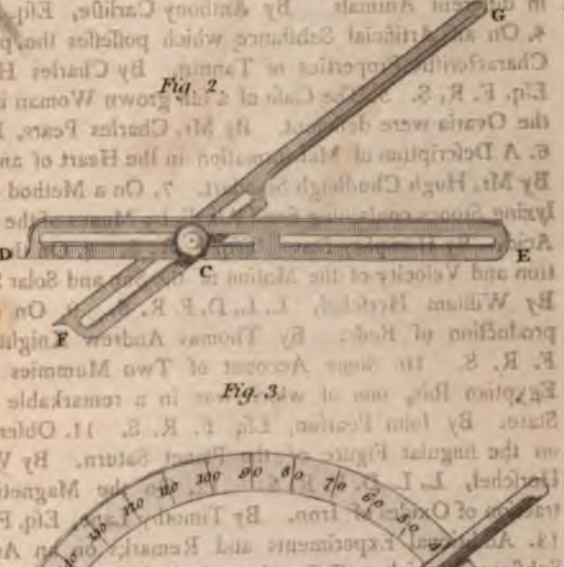


Fig. 2.

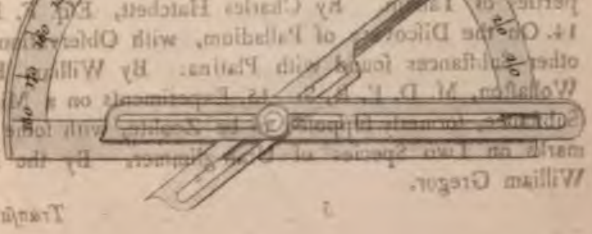


Fig. 3.

Mutlow Sc. Russell Ct.

Tranſactions of the Royal Society of Edinburgh.

Tranſactions of the Royal Society of Edinburgh (being the Continuation of Part II. together with Part III. of the Fifth Volume) Edinburgh Quarto 100 *pages Continuation of Part II, and* 126 *Pages, Part III. No Plates.*

THE heads of memoirs and communications made to the Society ſince their laſt publication are diſquiſitions on the origin and radical ſenſe of the Greek prepoſitions, by Mr. James Bonar, and experiments on the contraction of water by heat, by Dr. Thomas Charles Hope. Theſe two papers of which the latter is inſerted in our Supplement, complete the ſecond part: and the third part contains the hiſtory of the Society conſiſting of the following articles. 1. Of the Diurnal Variations of the Barometer, by Mr. Playfair. 2. Aurora Borealis obſerved in Day-Light, by the Rev. D. Patrick Graham. 3. Phenomenon of Two Rain-Bows interſecting one another, by Mr. Playfair. 4. On the Combuſtion of the Diamond, by Sir George Mackenzie, Bart. 5. Remarks on the Baſalts of the Coaſt of Antrim, by the Rev. Dr. Richardſon. 6. Rule for reducing a Square Root by a continued Fraction, by James Ivory, Eſq. 7. Singular Variety of Hernia, by Mr. Ruſſel. 8. Concerning the Chartreuſe of Perth, by the Abbé Mann. 9. Explanation of the Old Word Skull or Skoll, by the Rev. Dr. Jamieſon. 10. Biographical Account of the late Dr. James Hutton, by Mr. Playfair. 11. Minutes of the Life and Character of Dr. Joſeph Black, by Dr. Ferguſon. 12. Appendix Liſt of Members elected ſince the Publication of the laſt Volume. 13. Liſt of Donations.

Academical Inſtitutions in America.

THREE Inſtitutions for the promotion of Natural Phyloſophy and the Arts, having been eſtabliſhed in the united ſtates of America, not many months ago, of which no notice has hitherto appeared in this work, it is hoped the following account of them will not be unacceptable.

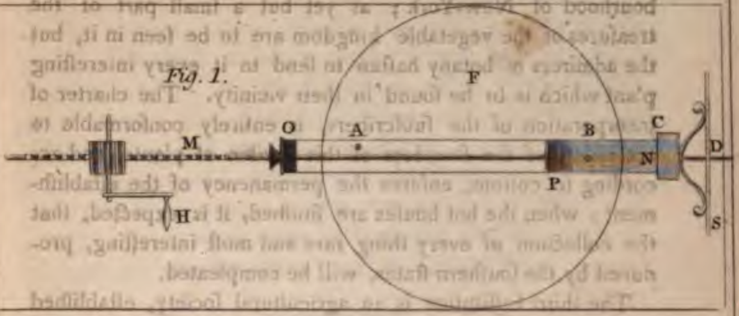
The firſt is an Academy of the Fine Arts, of which the firſt idea is due to Mr. Livingſton: The public were ſo ſenſible of its importance, that long before the arrival of the plaiſter of Paris caſts, which he preſented to the infant Society, the number of ſubſcribers, at 25 piaſters each, amounted to 180.

The ſecond Inſtitution is a Botanic Garden in the neighbourhood of New-York; as yet but a ſmall part of the treaſures of the vegetable kingdom are to be ſeen in it, but the admirers of botany haſten to ſend to it every intereſting plant which is to be found in their vicinity. The charter of incorporation of the ſubſcribers, is entirely conformable to the views of the founders of this garden of plants, and according to cuſtom, enſures the permanency of the eſtabliſhment: when the hot houſes are finiſhed, it is expected, that the collection of every thing rare and moſt intereſting, produced by the ſouthern ſtates, will be compleated.

The third Inſtitution is an agricultural ſociety, eſtabliſhed at Waſhington, under the ſpecial protection of government. The preſident of the United States, who is a moſt enlightened agriculturiſt, the chief men of the adminiſtration, the ſenators, and the deputies of congreſs, are all members of it officially. The ſociety being now wealthy from the ſums granted by government, and the numerous ſubſcriptions of aſſociates and correſpondents, have purchaſed an handſome houſe, and a farm of thirty acres; they have alſo began a library; and are in poſſeſſion of the fine collection of ploughs, and other inſtruments of Agriculture, which formerly belonged to general Waſhington: the form of its adminiſtration, the number and the ſucceſſion of its members, the capital which it may poſſeſs (ſpecified in buſhels of corn) and its whole organization is regulated by its charter of incorporation; which conſtitutes this aſſociation a body politic, and fixes the perpetuity of its continuation: It is reported, that the anſwers which it returned to the numerous queſtions propoſed by the ſocieties of the different ſtates ſoon after its eſtabliſhment, will form a very intereſting work which will ſoon be publiſhed.

Mr. Wright's new Air Pump.

Fig. 1.



Mr. Cuthbertson's Valve for a Condenser &c.

Fig. 6.

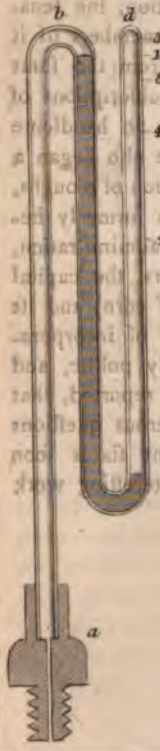


Fig. 2.

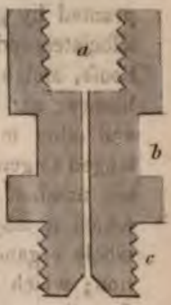


Fig. 4.

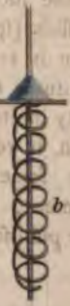


Fig. 5.

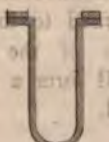
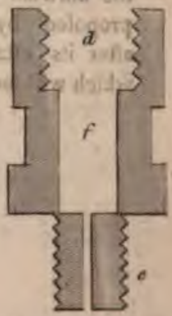


Fig. 3.



THE
YORK
RY
ASTOR, AND
TILDEN ATIONS
R L

Criterion of Life.

Criterion of Life.

DR. STRUVE has contrived an apparatus which is mentioned in the foreign Journals, but not deſcribed. The object of its application, is to ſhew by means of galvaniſm, whether the appearance of death be real; a purpoſe ſufficiently intereſting to every human being, who has for a moment reflected on the ſatisfaction which recovery from apparent death muſt give to the friends and relatives of the individual ſuppoſed to be dead; and on the ſtill more impreſſive and dreadful incident of recovery after burial. Our galvanic and anatomical philoſophers will find no difficulty in applying this powerful agent to ſo good a purpoſe, in which the learned Doctor has the merit of taking the lead.

Mr. SESSKEN who has ſucceſsfully laboured in the conſtruction of a reflecting teleſcope of thirteen feet focus, has lately ſupplied the Obſervatory at Lilienthal, with two mirrors of fifteen feet focus and eleven inches aperture, which prove to be excellent, and bear the magnifying power of 2000 very well, on the proper objects, and at ſuch ſeaſons as are fit for making obſervations of this nature.

Numbering of Houſes.

Numbering of houſes.

A NEW mode of numbering houſes has lately been adopted at Paris, which is attended with much advantage, and deſerves to be followed in this country. Over each door the numbers are painted in large diſtinct characters, and in conſpicuous colours; they are generally either brown or red, on a yellow ground, ſurrounded by a blue ſquare; but the principal ſingularity, which is the object of this notice, is, that all the odd numbers are placed at one ſide of the ſtreet, and the even numbers at the other; by which means, may be ſeen at once on entering the ſtreet, at which ſide of the way the houſe is, which is ſought for; by which much time may be ſaved, not only by its making it unneceſſary ever to croſs the ſtreet more than once, but alſo by its always preventing the trouble of returning back again on the other ſide of the ſtreet from that already paſſed, to find a particular number, which often happens, where the old method of numbering is uſed, from the order of the numbers proceeding regularly down one ſide of the ſtreet, and back again in the reverſed direction on the other; and which, when the ſtreets are very long, as many are in this metropolis, is often attended with ſerious inconvenience; but in the new method of numbering, this can never occur, as in it the numbers proceed in the order of progreſſion in the ſame direction at both ſides of the ſtreet.

Index

INDEX

TO VOL. XII.

A.

ABERDEEN, in want of a public library, and the advantages which might be derived from ſuch an eſtabliſhment, 267

Abernethy, 250

Abſorption of water in ſoils, 85

Achard, M. on a new mode of obtaining ſugar from beet-root, 259

Acids, manufactories of, how far prejudicial to the health of the neighbourhood, 126

Aeroſtation, 298

Air, atmoſpheric, new experiments on the reſpiration of, 249.—Diminution of in the proceſs, 251

Air-pump, on a new conſtruction, 305

Alligator of North America, turbid ſtate of, 131

American animal, called Jagiſho, account of, 139

American Indians, on their ſuppoſed Welch origin, 181.—Improbabilities attending the idea, 187

Analyſis of meteoric ſtones, 3.—Of muriatic acid, 58.—Of ſoils, 81.—Of porcelain earth, 277.—Of magneſian earth of Baudiſſero, 320.—Of factitious puzzolana, 336

Animal exertion and mechanic power compared, as to their identity, 8

Antoni, 42

Aqua-fortis, diſtillation of, 127

Arabic antiquities in Spain, 301

Arcy, Chevalier de, 41

Aſtronomical prize, Lalande's, 142

Aſtronomy, 297

Aſhburner, Mr. 171

Attraction of deliqueſcent ſalts, table of, 242

Azote neceſſary for the ſlow combuſtion of phoſphorus, 250.—Experiments to determine how much is abſorbed in the proceſs of reſpiration, 252.—Remarks on its uſes, 253

B.

Bailbet, 52

Balance, variations of, method of regulating, 56

Bancks, Mr. R. his inſtrument for meaſuring the angles of cryſtals, 374

Banff, in want of a public library, 268

Banks, Sir Joſeph, reference to his pamphlet on the blight in corn, 145

Barberry buſh productive of blight in corn, 145.—Doubt whether this diſeaſe affects the grain, 146, 154

Bartholomew's Hoſpital, lectures at, 61

Burton, Dr. on the torpid ſtate of the North American Alligator, 131

Bartram, Mr. 132, 194

Baudiſſero, magneſian earth of, 284, 320

Baumé, 278

Beaunier and Gallois, experiments made by, with water blowing machines, 48

Bedford, Duke of, 193

Beet-root, new method of obtaining ſugar from, 259

Bellows applied with the reverberatory furnace in refining iron, 38—Cylindrical of Namur, 52

Bergman, 105, 374

Bernouilli, Mr. 99

Berthier's prizes in fortification, 300

Berthollet, 124, 249

Berzelius, on Cerite, 105, 108

Beyer, Mr. 296

INDEX.

Bile, on, 269.—Former opinions reſpecting erroneous, ib.—New experiments on, 270
Biot, M. on the formation of water by mere compreſſion, 212.—His extenſions of the theorems in the horologium of Huygens, 296
Blagden, Sir Charles, 29.—On the expanſion of water, 343, 359, 362
Blaſting Rocks, improved mode of, 41, 60.—Danger of the old method, 61.—Farther obſervations, 171
Blight in corn, 145, 149.—Frequently miſtaken for ſhrivelled grain prematurely reaped, 153
Bohemian Society, 34
Bonvoiſin, on the Carneen ſtone, or Cacholong, 321
Books, new, 376
Boſſu, 131
Botanic Garden at Copenhagen, 299
Boulton and Watts, 174
Bounet, 39
Brale, Mr. 124
Bralle, Mr. his new proceſs for ſteeping hemp, 61
Breweries, 124
Brauer, Mr. his drawings of the diſeaſes in corn, 146
Brugnatelli, 275
Buffon, 114, 129
Bulbous roots, compoſition of ſoils proper for, 95.—Blown in water, 308
Bunda Language, dictionary of, 301
Butter, proceſs of making, 219.—Purification of by fuſion, 219

C.

Cadet, on the deliqueſcence and Effloreſcence of ſalts, 240
Canova's ſculptural exertions, 301
Carangeau's graphometer for cryſtals, 374
Carbon, gaſeous oxide of, experiments on the reſpiration of, 254
Carbonate of potaſh, on the formation of, 274
Carbonic acid, quantity produced in reſpiration, 254
Caſeletta, earth of, 326
Caſtellmonte, earth of, 324
Catgut manufactories prejudicial to the health of the neighbourhood, 125
Celtic words diſcoverable in the American dialects, 191
Cerite, experiments on, 105
Ceruſe, pure and beautiful, 143
Chaptal, on manufactories injurious to health, 122.—Reference to his chemiſtry, 338
Charkow, Univerſity of, 298
Charts printed by moveable types, 300
Chenevix, Mr. on artificial tan. 327
Childs, Mr. his communication reſpecting the Welch origin of a tribe in North America, 183
Chimney, draft of, increaſed by Steam, 2
Chineſe fire works, 273
Chromate of lead and ſilver, diſſolved in nitric acid, 144
Circle, diviſion of an arch of, 226
Clapham Church, deſcription of the frame-work by which the roof was raiſed, 176
Claproth, *ſee* Klaproth
Clegg, Mr. his portable ſteam-engine, 174
Cloſe, Mr. W. his apparatus for raiſing water, 16.—On blaſting rocks, 171
Cobalt, pure, method of obtaining it, 258
Collenbuſch, Dr. on Turf, 364
Colour manufactories, 124
Colour for marking the heads of pieces of cotton or linen in the rough, 206
Colours, abſtract of a memoir on, 112.—Theory of, imperfect, 121
Compaſs for taking bearings in a chart, 224
Conducting power of fluids, obſervations and experiments on, 133
Cooling, experiments on, 70
Copenhagen, botanic garden at, 299
Corn, diſeaſes of, 145.—Country names of, 147

INDEX.

Corneen Stone, or cacholong, 321
Correſpondent, a, on the ſtate of provincial, ſcientific and literary ſocieties, 267
Croune, Dr. his diſcovery of the expanſion of water prior to freezing, 340
Crow, an American, Anecdotes of, 194
Cruickſhank, Dr. 59
Cumberland, G. Eſq. on the diſeaſes of corn, 145.—On a project for extending roads on an inclined plane, 266
Curaudeau, M. 274
Curriers' ſpent oil, 220
Curwen, Mr. on draining lands, 177
Cuthbertſon's improved air-pump, 306, 369

D.

Dalton, Mr. on Count Rumford's experiment relating to the denſity of water, 28.—Remarks on, 180.—Experiments on fluids, 134, 343
Davy, H. Eſq. on the analyſis of ſoils, 81.—On nitrous oxide, 249, 255
Decamdra, Mr. 35
Delauny, Mr. 303
Deſcotils on the decompoſition of ſulphate of lead by muriatic acid, 221
Devon iron-works, 55
Deyeux's diſcovery of artificial tan, 327
Diſpatches, propoſed method for expeditious conveyance of, 266
Dodun, M. on factitious puzzolana, 331
Draft of a chimney increaſed by the admiſſion of ſteam, 2
Draining land, experiments on, 177
Du Hamel, 233.—On the ſap of trees, 309, &c.
Dundonald, Lord, on the analyſis of ſoils, 81
Dye-houſes, 125

E.

Elecampaine root, experiments on, 97
Electric ſpark, may conſiſt of light driven from compreſſed air, 214
Electrical machines, improvement in, 103
Electrical conductors, 297
Elkington, Mr. objections to his mode of making drains, 177
Enquirer, an, on inſtances of waſteful negligence in certain northern fiſheries, 269
Excitability of the parts of animals by galvaniſm, 100

F.

F. A. his account of thermometers for regiſtering temperatures in the abſence of the obſerver, 215
Faujas, on the eruptions of ancient volcanoes, 331
Fire-arms, on the initial velocity of projectiles diſcharged from, 41
Fire-works unknown in Europe, deſcription of, 273
Fiſher, Mr. his improved method of blaſting rocks, 172
Fluids, conducting power of, 133
Fogget, M. on blaſting rocks, 60
Force. power, riſe and application of the terms, 8.—Definition of, 9
Fortification, prizes in, given by the French miniſter of war, 300
Foſter, Dr. 131
Fothergil, 250
Franklin, Dr. 297
Froſt, perpetual, prevalent in the abyſſes of the ocean, 220
Fryer, Mr. Michael, 225

G.

Gallois, ſee Beaunier.
Galvaniſm, 59.—New diſcoveries in, 99
Gaſeous oxide of carbon, *ſee* carbon
Gaſes, compreſſed, affinities of, 368
Geneva, Lake of, ſudden and irregular fall of, 198.—Explanations by various authors, 200.—Singular appearance of,

202.—Objections to the theories quoted 203.—Another theory offered, 204

Geographical dictionary of Ruſſia, 303

Geography, 297

Geometry, 296

Giddy, Davies, Eſq. M. P. on the inviſible emiſſion of ſteam and ſmoke, 1. Remarks on, by W. N. 47

Gieſecke's propoſed expedition to Greenland, 299

Gilding on metals, detrimental to the health of the workmen, 127

Gilpin, Mr. his experiments on the expanſion of water, 343

Gioannetti, Dr. 277

Giobert on the magneſian earth of Baudiſſero, 277, 284, 320, &c.

Goener, 301

Goodwin, 250

Gough, John, Eſq. on the diviſion of an arch of a circle, 285

Graham, Mr. 132

Graphometer for meaſuring the angles of cryſtals, 374

Gravity, weight and heavineſs terms admitting of diſtinction, 14

Greek words found in the American dialects, 191

Greenland, propoſed mineralogical expedition to, 299

Gregory, Mr. O. on mechanic power, in reply to Mr. Hornblower, 7

Griffith, Maurice, his relation reſpecting the Welch American Indians, 183.—Obſervations on, 187

Grobert, colonel, his machine for meaſuring projectiles, 42, 45

Gums arabic and adracanth, experiments on, 256

Guyton, Morveau, on manufactories injurious to health, 122

H.

Haiter, Mr. 303

Harding, Mr. obtaining the aſtronomical prize of Lalande, 142.—His newly diſcovered ſtar, 288

Haſſenfratz, J. H. on the art of bending wood, 30

Hatchett, Mr. on artificial tan, 327

Hauffman's experiments to aſcertain the beſt colour for marking pieces of cotton or linen, 206

Hauy, 374

Heat, inveſtigations reſpecting, 65, 72, 54.—Apparatus for tranſmitting downwards through fluids, 136

Heavineſs, *ſee* Gravity.

Hedwig, 239

Hemp, new proceſs of ſteeping, 61.—Its manufacture injurious to health, &c. 124

Herſchel, Dr. his experiments on teleſcopes, 285.—Obſervations on Mr. Harding's newly diſcovered planet Juno, 289

Heringer on cerite, 105, 308, 310

Hoodleſs, Mr. 179

Hooke, Dr. on the expanſion of water, 341

Hope, Dr. on the denſity of Water, 180.—On the contraction of water by heat, 339, 362

Horn, Mr. 61

Hornblower, Mr. J. C. reply to his remarks on mechanic power, by M. Gregory, 7.—His deſcription of the framed work by which the roof of Clapham church was raiſed, 176

Howard, Mr. on meteoric ſtones, 3

Humboldt, on the longitude of Mexico, 297

Hutton, Dr. on the velocity of projectiles, 42

Huygens's theorems extended, 296

Hydraulic machine, by Mr. Cloſe, deſcription of, 16

I.

Identity of animal exertion and mechanic power, 8

Indoſtan, ſingular method of conſtructing walls and roofs there, 313

Inſtruments requiſite for the analyſis of ſoils, 83

Inverneſs, advantages which would reſult from the eſtabliſhment of a public library there, 268

Inviſible emiſſion of ſteam and ſmoke, 147

Iron, caſt and malleable, experiments on, 34

Izarn, Dr. 3

J.

Jackſon, Rev. Mr. 246

Jagiſho, an American animal of former times, account of, 139

Jefferſon, Mr. 141

Jeſſop, Mr. on blaſting rocks, 60

J. P. on light as a body, 104

Juno, the planet, obſervations on the nature and magnitude of, 289

K.

Kirwan, Mr. on the Analyſis of ſoils, 81

Klaproth on falſe tungſtein or cerite, 105 —His death, 142

Knight, Andrew, Eſq. on the ſtate in which the true ſap of trees is depoſited during winter, 240, 308

Kœliſon, a new muſical inſtrument, 142

Kotzebue on the ancient MSS. found at Portici, 303

L.

Lalande's aſtronomical prize, 142

Lampadius, G. A. on the difference between caſt and malleable iron, 34

Languages, elements of, 8

Lavoiſier, 250

Lawſon, Mr. 132

Lead, ſulphate of, on its decompoſition by muriatic acid, 221

Leather, compounds uſed in dreſſing, 220

Legoux de Flaix, on the baked walls and roofs of rural buildings in Indoſtan, 313

Leipſic fair, 300

Leverian Muſeum, to be ſold, 144

Lewis, Dr. on water-blowing machines, 48

Light may be as ſtrongly reflected by a rough ſurface as by a ſmooth one, 74. — Queſtion reſpecting, 104. — Experiments on, 166

Linen, &c. compound for marking, 207

Linnæus, 131

Literary and Philoſophical Society at Newcaſtle upon Tyne, twelfth report, 60

Luc, M. de, his experiments on the expanſion of water, 342

Luminous bottle, method of preparing, 276

M.

Mc Gilivray, Mr. 190

Macquer, 277

Madoc, or Madog, the Welch prince, on his ſuppoſed emigration to America, 188

Magiſtrates, caution to, 128

Magneſian earth of Baudiſſero, 284, 320

Manufactories, from which a diſagreeable ſmell ariſes, ſolution of a queſtion concerning, 122

Marking linen, &c. compoſition for, 207

Maſlouſky's new muſical inſtrument, 142

Mathey's machine for meaſuring projectiles, 42

Mauſoleum of the Arch-Ducheſs, Chriſtiana, 301

Mechain, on the occultation of π ſcorpio, 297

Mechanic powers, on, 7

Medical inſtitution, 62

Menzies, 250

Metallic veſſels, 72

Metals loſe not their power of reflecting with their poliſh, 74

Meteoric ſtones, on, 2.—Analyſis of, 3, &c.

&c.—New hypotheſis reſpecting their origin, 6
Meuſnier, General, 115
Mexico, longitude of, accurately determined, 297
Mildew in corn, 147.—Does not ariſe from an external cauſe, 148
Milk, memoir on, by M. Thenard, 218
Mineral preparations, how far pernicious to the health of thoſe employed, and to the neighbourhood, 127
Mineralogical expedition to Greenland propoſed, 299
Molard, 124
Monge, 124
Mons, Van, information by, reſpecting ceruſe, 143.—On the prevention of putrefaction, 144
Morning Chronicle, extract from, giving an account of the eruption of Veſuvius, 222
Mud walls of Indoſtan, 313
Muriatic acid, compoſition of, 58
Murray's experiments on fluids, 134
Muſquet barrels burſt by a charge of gunpowder confined by ſand, 40
Muſical inſtrument, new, 142

N.

Namur, cylindrical bellows of, 52
Napion, 277
Newton, Sir Iſaac, 7, 112
Niccolanum, *see* Nickeline
Nicholſon's experiments on fluids, 134,
Nickel, on, 75.—Malleable and magnetic, 77, 78.—External character, ib.—Specific gravity, ib.—A noble metal, 78, 80.—Its magnetic property weakened by copper, and deſtroyed by arſenic, 79.—Action of acids on, ib.—Beſt mode of forcing from iron, ib.—Oxide of, 80.—Precipitates, ib.
Nickeline, a newly diſcovered metal, 261.—Experiments on, ib.
Northmore, Mr. T. on the effects which take place in gaſes, when compreſſed, 368

O.

Oakley, Mr. 149
Occultation of the pleiades by the moon, of α ſcorpio, and of the comet in 1793; 297
Olbers, Dr. 293
Oughtred, 225
Oxigen and hydrogen may unite by preſſure, 212
Oxigen gas more productive of carbonic acid, in reſpiration, than atmoſpheric air, 254

P.

Pacchioni, Dr. on the compoſition of muriatic acid, 58
Pallas, Dr. 133
Pape, the Rev. D. his improvement of Rye Harbour, 245
Paper manufactories, 124
Peele, Mr. on muriatic acid, 59
Pelletier's apparatus for obtaining carbonate of potaſh, 274
Pennant, Mr. extract of a letter from relative to the non-exiſtence of Welch Indians in America, 190
Peterhead, in want of a public library, 268
Pfaff, Profeſſor, his new experiments on reſpiration, 249
Piazzi, Mr. 293
Pictet on the occultation of the pleiades by the moon, 297
Pignotti, Sig. Lorenzo, 58
Piracy of books puniſhed, 301
Piron, Mr. on the temperature of the ſea, 220
Plowman, T. Eſq. his improved Sheepfold, 192
P. M's quotation of a deſcription of fireworks unknown in Europe, 273

Porcelain earth, analyſis of, 277
Portici, ancient manuſcripts diſcovered at, 303
Poudrette manufactories, 125
Poullaouen iron works, 48
Pouſchkin, Count, on the ſolution of chromate of lead and ſilver, 144
Powel, David, his account of the emigration of the Welch, under their prince Madoc, 188
Power, mechanic, on, 7.—Indentified with animal exertion, 8.—Riſe, application, and definition of the term, ib.
Prieur, memoir by, on colours, 112.
Prince's improvement in the air-pump, 306
Projectiles, method of meaſuring the initial velocity of, 41
Prony, M. 10.—His method of meaſuring the initial velocity of projectiles diſcharged from fire-arms, 41
Provincial ſocieties for ſcientific and literary improvements, ſtate of, 267
Prouſt Profeſſor, on the meteoric ſtone of Sigena, 2
Pruſſian blue manufactories not prejudicial to the health, 128
Putrefaction, animal, pernicious in proportion to its humidity, 125.—Prevented, 144
Puzzolana, factitious, 331

R.

Radiation of heat, 74
Rail roads, 266
Reaping, on, 153
Reſpiration, chemical reſearches concerning, and ſhort hiſtory of, 249.—New experiments on, 250
Reflection of heat by poliſhed and blackened bodies, 66
Reverberatory furnace for experiments on caſt and malleable iron deſcribed, 34
Richman, Profeſſor, 297
Richter, Dr. J. B. on pure nickel, 75—On a metal much reſembling nickel, 261
Ritter, Mr. on galvaniſm, 99.—Biographical notice of, 101, 102
Roads on an inclined plane, project for, 266
Robertſon, Dr. 188.—His balloon, 298
Robins, Mr. Benj. 41
Robinſon Profeſſor, 7.—Correction of a miſtake in 12.—Diſtinction between gravity, weight, and heavineſs, 14
Rocks, Mr. Jeſſop's improved mode of blaſting, 60
Roebuck, Mr. on water-blowing machines, 55
Rome de l'Iſle, 374
Roſe, Mr. on the diſcovery of a new vegetable ſubſtance, 97
Rumford, Count, remarks on his experiments on the denſity of water, 28.—On the velocity of projectiles, 41.—On heat, 65, 154.—On coloured ſhadows, 114.—His opinion on the non-conducting power of fluids controverted, 133.—On the expanſion of water by beat, 343, 351
Ruſſian marine inſtitution, 302.—Eſtabliſhments for Education, 302.—Geographical dictionary, 303
Ruſt in corn, 149.—Prevented by ſteeping, ib.
Rye Harbour, account of the means uſed to make it navigable for ſhips of conſiderable burthen, 245

S.

Sal-ammoniac manufactories not injurious to health, 128
Salt of the ſea, origin of, 59
—— ſpirit of, manufacture of, not injurious to the health of the neighbourhood, 127
Salts, deliqueſcence and efflorescence of, 240.—Experiments, 241.—Remarkable facts, 244

Salts, deliqueſcent, table of, in the order of their attraction, 242

Sand, an uſeful aſſiſtant to gun-powder in blaſting rocks, &c. 41, 60, 171

Sap of trees during winter, 233, 308.—Experiments on, 234

Severein the engineer, notice of his death, 145

Saw-makers, their method of hardening ſteel, 63

Scientific News; for Auguſt, 58.—For September, 142.—For October, 220.—For November, 296

Sea, temperature of, 220.—At great depths eternally frozen, ib.

Seguin, 250.—On the oil uſed by Curriers, 220

Sheepfold, improved, 192.—Advantages of, ib.—Method of placing on hilly grounds, 193

Sigena, account of a meteoric ſtone, which fell there, 2

Silliman, Benj. Eſq. 305

Singer, Mr. on an improved mode of applying the points in electrical machines, [illegible]

Six's Thermometer, objections to it, 216

Slaughter houſes, 125

Smeaton, Mr. 10, 13

Smut in corn, 148

Soils, analyſis of, 81.—Subſtances found in, ib.—Inſtruments requiſite for, 83, 96.—Mode of collecting, 83.—Aſcertaining their ſpecific gravity, 84.—Phyſical properties, ib.—Quantity of water abſorbed by, 85.—Separation of extraneous matters from, 85, 86.—Examination of 86, &c.—Products, 91.—Simplification of the analyſis, 92.—Improvement of, 93.—Difference of under different climates, 94.—Compoſition of, ib.—Rendered fertile by changing the compoſition of the earthy parts, 96

Sonini's Journal, extract from, 276

Springs, method of making, 63

Starch, a powder reſembling, obtained from elecampane root, 97.—Manufactories, [illegible]

Starck, N. D. Eſq. his compaſſes for taking bearings in a chart deſcribed, 224

Staunton, Sir G. his embaſſy to China, extract from the account of, 273

Steam applied to aſſiſt the refining of iron, 37.—Its inviſible emiſſion into the air, 1, 47

Steam-engine applied to carriages, 1.—A portable one, 174.—Improvements in, 294, 316

Steel, blued, properties of, 63

Steeping hemp, 61.—Grain to prevent ſmut, 149

Steinacher, M. on carbonate of potaſh, 274

Steward, Profeſſor, 8

Stones, meteoric, on, 2.—Shower of, 3.—Analyſis of, 3, &c.—New hypotheſis reſpecting their origin, 6

Straw, obſervations made on diſeaſed, 151

Succulent plants, preſervation of, 64

Sugar, new mode of obtaining from beet-root, 259

Sulphate of lead, ſee lead

Sulphuric acid, manufactories of, 126

Syphon, inverted, Mr. Cloſe's method of raiſing water by means of, 16

T.

Tan, artificial, prepared from coal, &c. 327

Taſſaert, M. 105

Teleſcopes, experiments for aſcertaining how they enable the ſpectator to diſtinguiſh the real diameter of objects, 285

Teſſier, 124

Theatre in the Crimea, 302

Thenard, M. on milk and butter, 218.—On bile, 269

Thermometers for regiſtering temperatures in the abſence of the obſerver, 215

Thomas de Thomon, 302
Thomſon's experiments on Fluids, 134. 239
Thornhill, Mr. 61
T. I. B. on the maximum denſity of water, 180
Toulmin, Mr. on the ſuppoſed Welch tribe of American Indians, 181
Trades, objectionable, on account of their pernicious qualities, 123
Trail, Dr. on the conducting power of fluids, 133
Traveller, a, on the ſtate of provincial ſocieties for ſcientific and literary improvement, 267
Trees, on the ſtate in which their ſap is depoſited during winter, 233
Trevithick's ſteam-engines, 1
Tromſdorf, M. on obtaining pure cobalt, 258
Truſler, Dr. on heavineſs and weight, 15
Tungſtein, falſe, 105
Turf, obſervations on, 364

V.

Valli, Dr. on the prevention of putrefaction, 144
Vapour, aqueous, applied to the refining of iron, 37
Varniſh capable of reſiſting the actions of acids on metals, proceſs for making, 207
Vaucher, M. on the ſeiches of the lake of Geneva, 198
Vauquelin on cerite, 105, 108.—On gum-arabic and adracanth, 256
Veſuvius, eruption of, 222
Vegetable ſubſtance, a new, 97
Vibrations of a balance, on the, 56
Vieta, 225
Vinegar manufactories, 127
Von Einfiedel, 35

W.

Wagener's invention for printing charts, 300
Wallis, 225
Water, apparatus for raiſing, by means of condenſed air, 16.—Remarks on Count Rumford's experiments on the denſity of, 28, 180, 339.—How far uſeful in refining of iron, 37.—Abſorption of in ſoils, 85.—A bad conductor of heat, 162.—Effect produced on by air blown from the human lungs, at the depth of ſeveral feet below the ſurface, 106.—Formation of by compreſſion, 212
Water-blowing machines, 48.—Experiments with 50.—General concluſions, 54
Water-clocks, 363
Water colours, experiments in, 208
Weight and heavineſs, not ſynonimous terms, 14
Welch Indians of North America, obſervations and conjectures relative to, 181
Welter's apparatus for obtaining carbonate of potaſh too complicated, 274
White colour, new definition of, 11
Williams, Dr. 190
W. N. on the burſting of muſquet barrels by a charge of gun powder confined with ſand, 40.—On the inviſible emiſſion of ſteam into the air, 47.—On water-blowing machines, 54.—On the ſeiches of the Lake of Geneva, 203.—In anſwer to F. A. on improved thermometers, 217.—On the contraction of water by heat, 362
Wood, on the art of bending, 30
—— felled in autumn or winter more firm than when ſuffered to ſtand till ſpring or ſummer: correction of the general opinion as to the cauſe, 233
Woolf, Mr. his improvements in ſteam-engines, 294, 316
Wright Elizur, his newly conſtructed air pump, 305
Wyatt, Mr. Samuel, 338

Y.

Young, Mr. Charles, on the vibrations of a balance, 56.—On the analyſis of ſoils, 81

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